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**98**

# ESSAYS IN THE PHILOSOPHY AND HISTORY OF LOGIC AND MATHEMATICS

Roman Murawski

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OF LOGIC AND MATHEMATICS

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**Roman Murawski**

# ESSAYS IN THE PHILOSOPHY AND HISTORY OF LOGIC AND MATHEMATICS

**Foreword**

**Jan Woleński**

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Printed in The Netherlands

*Dedicated to*

*my wife Hania and daughter Zosia*

# CONTENTS

# FOREWORD

Roman Murawski is a hard mathematical logician, working on technically advanced problems. He obtained several important results concerning satisfaction classes, decidability, incompleteness, recursion theory and the foundations of arithmetic. These interests and achievements are summarized in Murawski's book *Recursive Functions and Metamathematics. Problems of Completeness and Decidability, Gödel's Theorems*, Kluwer Academic Publishers, Dordrecht 1999 (Synthese Library, vol. 286), one of the best introductions to the fields indicated by the title. This monograph combines three features, namely mathematical sophistication, philosophical depth and historical accuracy. Due to the character of *Recursive Functions and Metamathematics*, this book can effectively serve a very broad range of readers, coming from mathematics and philosophy, in particular those interested in mathematical logic and its problems.

The present collection brings together 18 of Murawski's essays (three of them are co-authored) in the philosophy and history of logic and mathematics. This material is naturally divided into two parts: philosophy of mathematics (9 essays) and history of mathematics and logic (9 essays). The papers in the first part vary from general surveys of the present philosophy of mathematics to studies devoted to very concrete topics, like Cantor's philosophy of set theory, the Church thesis and its epistemological status, the history of the philosophical background of the concept of number, the structuralistic epistemology of mathematics and the phenomenological philosophy of mathematics. Murawski was always deeply involved in investigations concerning the Hilbert program in the foundations of mathematics. These interests are represented in his *Essays in the Philosophy and History of Logic and Mathematics* by works dealing with the relation of Hilbert's ideas to those of Leibniz and Kant as well as with the development of the consciousness of the distinction between truth and provability. In other his papers devoted to Hilbert's program and its development (cf. References; unfortunately they could not be included into the present volume for technical reasons) he showed that Gödel's theorems, contrary to first reactions to them, do not invalidate Hilbert's hopes to establish theoretical mathematics on formalistically understood foundations – just the opposite, the results of the so-

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called reverse mathematics show that Hilbert's program can be in fact partially realized.

The second part of Murawski's collection is mostly devoted to particular topics with the exception of the last paper, which outlines a general perspective and philosophical background of the development of symbolism in mathematical logic. Three papers touch on the history of mathematics and logic in Poland: Józef M. Hoene-Wroński, Polish works on decidability and on predicate calculus. Other studies concern Giuseppe Peano and his rôle in the creation of contemporary logical symbolism, Emil L. Post's works in mathematical logic and recursion theory, the formalist school in the foundations of mathematics (John von Neumann and David Hilbert) and the algebra of logic in England in the 19th century. Although some of these studies concern well-known facts and persons or schools, they are additionally illuminated by Murawski by locating them in a wider historical perspective, in particular, by taking into account what happened later in logical studies. As a historian of logic in Poland, I would like to stress that Murawski's works on logic in Poland remind us of some forgotten, but important persons, like Hoene-Wroński, as well as of directions of investigations that are often neglected. The Polish school of logic and the foundations of mathematics became famous for many-valued and modal logic of Jan Łukasiewicz, systems of Stanisław Leśniewski, the metamathematical and semantic works of Alfred Tarski, generalized quantifiers introduced by Andrzej Mostowski or the hierarchy of recursive constructions proposed by Andrzej Grzegorczyk, but several other achievements, precisely those described by Murawski, also deserve attention.

Murawski studied with Andrzej Mostowski. The Mostowski school definitively treated mathematical logic as a part of pure mathematics. This fact meant that logical investigations had to fulfil all typical professional criteria of mathematical work. On the other hand, Mostowski considered the history and philosophy of mathematics as extremely important. He documented this attitude by his beautiful *Thirty Years of Foundational Studies*, Societas Philosophica Fennica, Helsinki 1965, commonly considered as the best survey of the development of mathematical logic and the foundations of mathematics after the discovery of incompleteness phenomena by Gödel in 1931. Mostowski demanded that every technical study in mathematical logic should also be elaborated from a historical and, if possible, philosophical point of view. This manner of doing logic and the foundations of mathematics was a continuation of Tarski's (he was Mostowski's mentor) attitude, but also Łukasiewicz's idea of how the history of logic is to be done. Łukasiewicz, who was the pioneer of the modern history of logic in Poland, maintained that the only way of doing historical work consisted in looking at the past from the perspective of what happened later and until now. In order to understand, for instance, Aristotelian syllogistic, one should try to locate it in the body of contemporary logic. Murawski is an eminent follower of this line of thought. Perhaps his treatment of the Hilbert program is the best example here. Murawski's way of elaborating how the original formalistic project functions is not limited to its Hilbertian version, but explains its internal limitations by showing what follows from its partial realizations. This perspective, consisting in discovering the past by using the spectacles displaying the contemporary situation is perhaps the most impressive feature of the present collection. Thanks are due to Roman and also to the publisher for collecting these extremely important philosophical and historical studies in one book.

*Jan Woleński*

Cracow, June 2009

# PART I

# PHILOSOPHY OF MATHEMATICS

# 1

# CANTOR'S PHILOSOPHY OF SET THEORY[1]

Set theory founded by Georg Cantor (1845–1918) in the second half of the nineteenth century (his main papers in this domain were published between 1874 and 1897) has already found his place in mathematics becoming not only a language of mathematics but providing also firm foundations of it – in fact, the whole of mathematics can be reduced to set theory.[2] Before set theory reached this status and before it became a well founded and fully recognized mathematical theory, it passed through the difficult period of crises (connected among others with the discovery of various antinomies). The reaction of contemporary mathematicians was also not always positive – recall critical and full of scepticism reactions of L. Kronecker, one of the university teachers of Cantor from Berlin. The reason for those reactions was among others the free usage of the infinity by Cantor. On the other hand in set theory were interested philosophers and theologians – at the very beginning even more than mathematicians. The reason for their interest was the fact that set theory was a theory of infinite objects and infinity – since the antiquity – puzzled scientists and caused many troubles. One should add that the inspirations of Cantor in constructing and developing set theory were not only of mathematical but also of philosophical nature – what today, when still more and more technically sophisticated results appear, is not always recognized. Hence it is worth looking back at the very beginnings of the theory of sets and to consider not just the mathematical aspects but rather the philosophical and ideological motivations of its founder.

It should be stressed that we shall restrict ourselves to the presentation of philosophical views of Cantor and try to reconstruct them on the basis of his papers and letters. We shall make no valuations and will not enter a discussion with them – those are left to the reader.

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[1]Originally published in Polish under the title "G. Cantora filozofia teorii mnogości", *Studia Filozoficzne* 11–12 (1984), 75–88. Translated and reprinted with the permission of the publisher.

[2]This is the main thesis of logicism, one of the three principal doctrines in the modern philosophy of mathematics.

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Start from the fact that Cantor was interested in philosophy and had a good knowledge of it. In his library he had works of classical writers from Plato and Aristotle through Augustine, Boëthius, Thomas Aquinas, Descartes, Spinoza, Locke, Leibniz to Kant. He not only possessed them but read them and knew them quite well. In fact one finds in his writings many quotations from works of famous philosophers and discussions with their views in connection with his own conceptions concerning, for example, the way of existence of mathematical objects or the infinity. He gave also classes in philosophy for students, devoted among others to Leibniz's philosophy (it was for him an occasion to compare his own views on the infinity with those of Leibniz). He had also friends among philosophers – let us mention here E. Husserl and H. Schwarz who received their *Habilitation* at the University in Halle where Cantor was professor between 1872–1913.

Cantor was attracted especially by scholastics. He found by them problems and considerations similar to those which fascinated him and saw the similarity and certain relationship between his abstract set-theoretical considerations on the one hand and the consideration of scholastic logic and theology on the other. In fact philosophy was for him not something foreign and exterior with respect to mathematics. He saw certain strong and interior connections between them. How important for him was the fact that readers of his papers should have knowledge both in mathematics as well as in philosophy follows from his remark from the introduction to one of his papers in which he says that he wrote this paper in fact for two circles of readers, "for philosophers who follow the development of mathematics till the modern time and for mathematicians who know most important old and new philosophical ideas"[3] (*Grundlagen einer allgemeinen Mannigfaltigkeitslehre*, 1883; see also Cantor 1932, p. 165). Cantor was convinced that "without a bit of metaphysics it is impossible to establish any strict science" (this sentence can be found in his notes from 1913 made in pencil and never published). This conviction can be easily seen also in his works devoted to set theory.

Starting the presentation of Cantor's philosophical views in a detailed way one should begin with his ideas concerning the reality of scientific concepts (for example of finite integers or infinite numbers). Cantor maintained (in the paper quoted above, cf. Cantor 1932, pp. 165–204) that the reality of mathematical concepts can be understood: (1) as an intra-subjective, hence immanent reality guaranteed by definitions which assign a given concept a place in human thinking which is well defined and different from other concepts (he adds that a concept "modifies in a certain way the substance of our spirit", p. 181); and (2) as an extra-subjective reality in which the concept appears as "a reflection of processes and relations taking place in an outer world located vis-à-vis in-

[3] If not stated otherwise, all quotations form Cantor's works are given in my own translation.

tellect" (p. 181). A characteristic advantage of mathematics Cantor sees in the fact that "by developing its ideas (*Ideenmaterial*) only the immanent reality of its concepts must be taken into account and consequently there is no obligation to consider their extra-subjective existence" (p. 182). Therefore one can speak about "free mathematics".

Note that Cantor was not ready to treat consistency as a unique and sufficient criterium of the existence in mathematics. He was convinced that mathematical concepts posses not only immanent (in a mathematician's mind) but also extra-subjective existence. Indeed, he was convinced that a mathematician does not create mathematical objects but discovers them. He expressed it already in the third thesis of his *Habilitation* dissertation from 1869 where he wrote: "Numeros integros simili modo atque corpora coelestia totum quoddam legibus et relationibus compositum efficere" (cf. Cantor 1932, p. 62). He confirmed it also at the end of his creative activity when he provided his fundamental work, consisting of two parts, namely *Beiträge zur Begründung der transfiniten Mengenlehre*, part I: 1895, part II: 1897; cf. Cantor 1932, p. 282–351) with the following three mottos:[4] "Hypotheses non fingo", "Neque enim leges intellectui aut rebus damus ad arbitrium nostrum, sed tanquan scribe fideles ab ipsius naturae voce latas et prolatas excipimus et describimus", "Veniet tempus, quo ista quae nunc latent, in lucem dies extrahat et longioris aevi diligentia".

For a "normal" mathematician who is working in, say, function theory, the definite decisions concerning the ways in which mathematical objects do exist are in fact not of a great importance for his research activity. On the other hand this is very important, even crucial, for set theory. Therefore Cantor attached importance to it. His conviction that mathematical objects posses existence independent of our thinking and of our discoveries, an existence in a certain sense primitive with respect to them implied his particular research attitude. It was the reason and source of his lonely defence of his ideas concerning set theory and of his stubborn attempts to solve the continuum problem.[5] It was not the modesty but just his philosophical convictions that forced him to write in a letter to G. Mittag-Leffler from 1884 the following words: "all this is not my merit –

[4]The first one has been taken from Newton what Cantor explicitly says. Two others are given without any indication of the source. We have ascertained that the second motto is a quotation from Francis Bacon's *Scripta in naturali et universali philosophia*, IV, 4: *Prodrimi sive anticipationes philosophiae secundae. Praefatio*. We were unable to determine the source of the third motto. May be it comes from Cantor himself?

[5]The impossibility of solving this problem – which was in a sense inconsistent with his Platonic views concerning the nature of mathematical objects – was one of the reasons of his nervous breakdown in the fall of 1884 and of the later mental disease which lasted till the end of his life. The fact that he cannot solve the problem had a strong influence on him — there were periods when he had serious doubts whether the set theory developed by him has any sense and can be treated as a scientific theory.

with respect to the contents of my works I was only a reporter and an official (*nur Berichterstatter und Beamter*) (cf. Fraenkel 1932, p. 480).

Note that Cantor attributed to the concepts of set theory not only the existence in the world of ideas but also in the physical world. Hence he was convinced of the real existence in this world of, say, sets of cardinality $\aleph_0$ or of the cardinality of continuum. In a letter to G. Mittag-Leffler from 16th November 1884 (cf. Meschkowski 1967, pp. 247–248) he wrote that the set of atoms in the All is countable (i.e., of the cardinality $\aleph_0$) whereas the cardinality of the set of "atoms in the ether" (*Ätheratomen*) belong to cardinals of the second class, hence this set is not countable. Since the picture of the world provided by the modern physics is different from that of Cantor's days (e.g., one has to reject the idea of the existence of the ether) the examples given by him do not convince us.

Notice also that two theses of Cantor, namely the thesis on specific "freedom" of mathematics and the thesis that mathematical objects are given to us and not created by us, are in fact not fully compatible and consistent.

Consider now particular philosophical problems connected with the set theory. Cantor treated basic objects of it, hence classes, sets, ordinals or cardinals, in accordance with his Platonic attitude. Explaining the concept of a set he wrote:

> By an "aggregate" (*Menge*) we are to understand any collection into a whole (*Zusammenfassung zu einem Ganzen*) M of definite and separate objects *m* of our intuition or our thought. These objects are called the "elements" of M

(*Beiträge zur Begründung der transfiniten Mengenlehre*, 1895; see Cantor 1932, p. 282; English translation p. 85). In the paper *Über unendliche lineare Punktmannigfaltigkeiten* (1883) he wrote:

> By a concept of 'variety' (*Mannigfaltigkeit*) or a 'set' (*Menge*) I understand generally any multiplicity (jedes Viele) which can be thought of as a unity (*als Eines*), that is, any totality (*Inbegriff*) of a definite elements which by a certain law can be unitied in a one object.

And he added (cf. Cantor 1932, p. 204):

> I hope I am defining in this way something which is connected with Plato's *εἰδος* or *ιδέα* as well as with this what Plato in his dialogue *Philebos or the highest good* calls *μικτόν*. He sets this against the concept of *ἄπειρον*, i.e., of unboundedness or undetermined what I am calling an improper infinity (*Uneigentlich-unendlich*) as well as against the concept of *πέρασ*, i.e., a limit and describes this as an ordered "mixture" (*Gemisch*) of both. Both those concepts come from Pythagoras what was indicated already by Plato himself.

In a similar way Cantor treated cardinals and ordinals. In *Mitteilungen zur Lehre vom Transfiniten* (1887–1888) he wrote (cf. Cantor 1932, p. 380):

> Cardinal numbers as well as order types are *simple* conceptual objects; each of them is a *real unity* (eine wahre Einheit) (*μονάς*) because in it a certain multitude and variety of *units* is *unified* into a *uniform* whole.
>
> Elements of a set $M$ which we consider, should be seen as separated; in an intellectual picture $\overline{M}$ of this set [...] which I call its order type are units connected in one organism.

> In a certain sense every order type can be thought as a whole consisting of a *matter* and a *form*; units contained in it being conceptually different are the *matter* whereas the order between them corresponds to the *form*.

As usual, Cantor refers to ideas of his predecessors and writes (cf. Cantor 1932, pp. 380–381):

> If one considers the definition of a finite ordinal number by Euclid then one must *admit* that he ascribes a number – as we do – to a *set* what corresponds to *its real origin* and does not make it only a "sign" which is ascribed to a single thing in a subjective process of counting. [...] It seems to me that he conceives *units* in a number separated in a similar way as elements in a discrete set to which it refers. Euclid's definition lacks at least an explicit indication that a number has certain *comprehensive character* [that it is something *homogeneous*] (*der einheitliche Charakter der Zahl*) what is very important.

Considering cardinals numbers Cantor wrote in *Beiträge zur Begründung der transfiniten Mengenlehre* (cf. Cantor 1932, p. 282; English translation p. 86):

> We will call by the name 'power' or 'cardinal number' of M the general concept which, by means of our active faculty of thought, arises from the aggregate M when we make abstraction of the nature of its various elements *m* and of the order in which they are given. [...]
>
> Since every single element *m*, if we abstract from its nature, becomes a "unit" the cardinal number $\bar{\bar{M}}$ is a definite aggregate composed of units, and this number has existence in our mind as an intellectual image or projection of the given aggregate M.

The concept of a set as well as the concept of an order type and of a cardinal number understood by Cantor not precisely enough led to the discovery of antinomies. Two of them knew already Cantor himself. He discovered the first one – the so called antinomy of the set of all ordinal numbers – in 1895 and in 1896 communicated it in a letter to Hilbert. Independently it was discovered also by Burali-Forti who published it in 1897 (in the journal *Rendic. Palermo* 11 (1897), 154–194). It is called today the antinomy of Burali-Forti. It can be formulated as follows: Let $W$ be a set of all ordinal numbers. As a set of ordinal numbers, $W$ is well-ordered. Let $\alpha$ be its order type. By theorems on ordinal numbers, $\alpha$ is greater than all elements of the set $W$, hence greater than any ordinal number. But this leads to a contradiction: indeed, on the one hand $\alpha$ does not belong to $W$, i.e., $\neg\alpha \in W$, and on the other $\alpha$ as an ordinal number, belongs to $W$, hence $\alpha \in W$.

The second antinomy known to Cantor is the so-called antinomy of the set of all sets. Cantor wrote about it in a letter to R. Dedekind from 31st August 1899 (cf. Cantor 1932, p. 448). Let $\mathcal{A}$ be the set of all sets. Let for a given cardinal number $\mathfrak{a}$, $M_{\mathfrak{a}}$ denote a set of cardinality $\mathfrak{a}$. For every cardinal number $\mathfrak{a}$ choose one set $M_{\mathfrak{a}}$. Let now $\mathcal{B} = \bigcup M_{\mathfrak{a}}$. Let $\mathfrak{a}_0$ be the cardinality of $\mathcal{B}$. By appropriate properties of cardinal numbers one has that the cardinality $\mathfrak{a}_0^{'}$ of the set of all subsets of a set of cardinality $\mathfrak{a}_0$ is bigger than $\mathfrak{a}_0$, i.e., $\mathfrak{a}_0^{'} > \mathfrak{a}_0$. On the other hand, since the family $\mathcal{B}$ is the sum over all cardinal numbers, there must be in it a representative $M_{\alpha_0'}$. Hence $M_{\alpha_0'}$ is a subset of $\mathcal{B}$ and consequently $\alpha_0' \leqslant \alpha_0$. So we obtain two contradictory statements:

$$\alpha_0' > \alpha_0 \quad \text{and} \quad \alpha_0' \leqslant \alpha_0.$$

What type of solution did Cantor see? Hence the possibility to form sets from any multiplicities leads to antinomies, one should, according to Cantor, restrict the possibilities of forming sets. In a letter to Dedekind from 28th July 1899 he wrote (cf. Cantor 1932, pp. 443–444):

> If we start from the notion of a definite multiplicity (*Vielheit*) of things, it is necessary, as I discovered, to distinguish two kinds of multiplicities.
>
> For a multiplicity can be such that the assumption that *all* its elements 'are together' leads to a contradiction, so that it is impossible to conceive the multiplicity as a unity, as 'one finished thing' (*ein fertiges Ding*). Such multiplicities I call *absolutely infinite* or *inconsistent multiplicities*.
>
> As we can readily see, the 'totality of everything thinkable', for example, is such a multiplicity.
>
> If, on the contrary, the totality of elements of a certain multiplicity can be thought of – without a contradiction – as 'being together' (*zusammenseind*), so that it is possible to consider it as 'one thing', then I call such a multiplicity *consistent* or 'a set'. (In French or Italian one can express this by words '*emsemble*' or '*insieme*').

So the distinction between inconsistent multiplicities called today classes and consistent multiplicities called sets should solve the difficulties. Of course the multiplicity of all ordinal numbers as well as the family of all sets are not sets but just inconsistent multiplicities and consequently there are no ordinal nor cardinal numbers corresponding to them. In this way Cantor eliminated both antinomies (he wrote about that in letters to Dedekind: about the multiplicity of all ordinal numbers in a letter from 28th July 1899 – cf. Cantor 1932, pp. 443–447, and about the multiplicity of all sets in a letter from 31st August 1899 – cf. Cantor 1932, p. 448).

It is worth noticing that Cantor's distinction between sets and classes can be seen as a reference to Leibniz's metaphysics.

The most important part of Cantor's set theory was formed by his considerations about infinite sets. Cantor distinguished several forms of infinity. In the paper *Mittteilungen zur Lehre vom Transfiniten* he wrote (cf. Cantor 1932, p. 378):

> I have distinguished three forms of actual infinity: *first* that which is realized in the highest perfection, in a completely independent being existing outside of the world, *in God* (*in Deo*) and which I call the *absolute infinity* or shortly the *Absolute*; *secondly* that which appears in an dependent and created world, and *third* that which can be comprehended in the thought *in abstracto* as a mathematical quantity, number or ordinal type. In the *both* latter cases in which it appears as a bounded quantity that can be extended and as connected with the finite, I call it the transfinite (*Transfinitum*) and strictly set it against the Absolute.

This distinction of several types of infinity leads to certain distribution of tasks connected with the investigations of it. Cantor writes (cf. Cantor 1932, p. 378):

> As the investigation of the *absolute infinity* together with the determination how much the human mind can say about it belongs to the *speculative theology*, so, on the other side, questions concerning the transfinite belong to *metaphysics* and *mathematics*.

Note that the question on the existence of the actual infinity is in all those cases answered by Cantor affirmatively (what he stated already at the very beginning of *Mittteilungen* ...).

Apart from this (ontological) distinction of various forms of infinity according to the way of existing Cantor considers also another one coming back to Aristotle, namely the distinction between actual and potential infinity. In a letter (included later into *Mitteilungen* ..., cf. Cantor 1932, pp. 400–407) from 28th February 1886 to A. Eulenberg (professor of medicine from Berlin) Cantor writes (cf. Cantor 1932, pp. 400–404):

> If one has before the eyes definitions of actual and potential infinity then the problems you are writing about can be easily solved.
>
> I. About P-I (P-I = potential infinity, *ἄπειρον*) one says first of all in those cases when one has a certain indefinite *variable finite* quantity which increases above all finite bounds (here one can think, e.g., about the so called time counted from a given initial moment) or which can become smaller than any finite bound (what is the case of a so called differential); generally I am talking about P-I in all cases in which a certain *indefinite* quantity is given which can be defined in an infinitely many ways (*die unzählich vieler Bestimmungen fähig ist*).
>
> II. Under A-I (A-I = actual infinity, *ἀφωρισμένον*) one should understand a quantity which on the one hand is invariable, constant and definite in all its parts, which is a real constant (*eine richtige Konstante*) and which simultaneously goes beyond any quantity of the same type. As an example I can quote the totality (*Gesamheit*, *Inbegriff*) of all finite positive integers; this set is an object for itself (*ein Ding an sich*) and forms – independently of the sequence of numbers belonging to it – a certain definite quantity (*Quantum*) which is constant and invariable in all its parts (*ἀφωρισμένον*) and which should be considered as being greater than any finite number. [...] Another example is the totality of all points lying on a certain circle (or on any other given curve).[6]

Answering the question of A. Eulenberg concerning the problem whether it is reasonable to use the name infinity in the case of the potential infinity Cantor wrote (cf. Cantor 1932, p. 404):

> P-I is in fact no real infinity – therefore in my *Grundlagen* I call it *improper* infinity. On the other hand it would be difficult to remove completely the mentioned usage of this concept – the more so as P-I is an easier, nicer and more superficial, less independent concept with which is connected a certain nice illusion of having something proper and really infinite; in fact the P-I posses only a borrowed existence (*eine geborgte Realität*) and the whole time it indicates the A-I thanks to which it is possible. Hence the name of it used by scholastics, namely *συνκατηγορηματικός* is sound.

[6] As we see Cantor is using here the term 'quantity'. It was characteristic for the mathematics of the 19th century. Today it has been eliminated from mathematics and replaced by the concept of set.

This distinction between actual and potential infinity was treated by Cantor as very essential. In a letter to S.G. Vivanti from Mantua from May 1886 (included later in the paper *Mitteilungen* … – cf. Cantor 1932, pp. 409–411) he wrote (see p. 409):

> If one wants to be aware of the origin of widely spread prejudices concerning the actual infinity, of that *horror infiniti* in mathematics then one should take into account first of all the contrast between actual and potential infinity. While the potential infinity denotes nothing more than a certain undefined changing quantity (variable) being all the time finite and taking values which are either smaller than any small or bigger than any big finite limit then the actual infinity refers to a certain fixed in itself (*in sich fest*) constant value which is greater than every finite quantity of the same type. Hence for example the variable taking as values the integers $1, 2, 3, \ldots, \nu$ is a potential infinity while a set $(\nu)$ of all finite integers $\nu$ defined completely in an abstract way by certain law is the simplest example of the actually infinite quantity (*ein aktual-unendliches Quantum*).

Despite essential differences between actual infinity and potential infinity there were in mathematics many misunderstandings concerning them, in particular in cases "when there was given only a certain potential infinity that was taken incorrectly as an actual infinity or *vice versa*, notions which make any sense only from the point of view of the actual infinity were treated as potential infinity" (cf. Cantor 1932, p. 409). With the first case one has to do, e.g., "when one treats a differential as an actually infinitely small while it can be treated only as an auxiliary variable that can take arbitrarily small values – this has been explicitly stated already by both discoverers of the infinitesimal calculus Newton and Leibniz. This mistake can be treated as defeated thanks to the so called method of limits by which French mathematicians under the direction of great Cauchy have taken part with glory" (cf. Cantor 1932, p. 410).

Cantor considered the second indicated mistake as more dangerous. It appeared for example in the theory of irrational numbers which cannot be founded without using the actual infinity in a certain form. The latter is indispensable both in analysis as well as in number theory and algebra. Its indispensability is justified by Cantor in the following way (cf. Cantor 1932, pp. 410–411):

> As there is no doubt that we are forbidden not to have *changing* quantities in the sense of the potential infinity, one can deduce from that the necessity of the actual infinity as follows: To be able to apply such a changing quantity in mathematical considerations one should beforehand exactly know, by a definition, its scope; but the latter cannot be unstable because there will be no constant and firm basis of our considerations; hence this scope is a certain defined actually infinite set.
>
> Consequently any potential infinity, if it should be mathematically useful, assumes the actual infinity.

Since the actual infinity is indispensable for foundations of mathematics, it should be carefully studied. From the characteristics of the actual infinity given above (and its distinction from the potential one) one could deduce that it is a quantity that cannot be increased (*unvermehrbar*). But this is – according to Cantor – an incorrect supposition though it is widely spread in the antique philosophy as well as in scholastic and modern philosophy. One should make (cf. Cantor 1932, p. 405) "a fundamental distinction by distinguishing:

– $\mathrm{II}^a$ actual infinity that can be increased, hence the transfinite (*Transfinitum*),
– $\mathrm{II}^b$ actual infinity that cannot be increased, hence the Absolute."

All mathematical examples of the actual infinity given by Cantor (hence "the totality consisting of *all* points lying on a certain circle (or on any other curve)" mentioned above or the smallest transfinite ordinal number $\omega$) are of the type $\mathrm{II}^a$. There are certain connections between the actual infinity that can be increased, i.e., the transfinite, and the actual infinity that cannot be increased, i.e., the Absolute. Cantor writes (cf. 1932, pp. 405–406):

> The transfinite – with various shapes and forms it can take – indicates in a necessary way the *Absolute*, the "real infinity", a quantity to which nothing can be added or from which nothing can be deducted and which consequently can be treated as the *absolute* quantitative maximum. The latter overcomes in a certain sense the human ability of comprehension and cannot be described mathematically; on the other hand the transfinite not only fills a substantial space of possibility in God's knowledge but is also a rich and constantly expanding domain of ideal research and – in my opinion – is realized also in the world of created things and does exists to a certain degree and in various relations. In this way it expresses the magnificence of the creator strongly than it would be possible in a "finite world". This fact must wait still a long time to be commonly acknowledged and accepted especially by theologians.

Accepting the existence of the actual infinity Cantor decidedly rejected the actually infinitely small quantities. In his later writings, specially in letters, he sharply engaged in polemics with the introduction of them into mathematics, with "this infinitely bacillus of cholera in mathematics" (*infinitär Cholera Bacillus der Mathematik*) as he writes in the letter to S.G. Vivanti from 13th December 1893 (cf. Meschkowski 1962–1966). In the letter to F. Goldscheider, the grammar-school teacher in Berlin, from 13th May 1887 (and in the almost identical letter to K. Weierstrass from 16th May 1887 – both included into *Mitteilungen …*, cf. Cantor 1932, p. 407–409) he wrote (cf. Cantor 1932, p. 407):

> You mention in your letter the question concerning the actually infinitely small quantities. In many places of my works you will find an opinion that they are impossible mental objects (*Gedankendinge*), i.e., inconsistent in themselves.

Cantor attempts to justify this position using his theory of transfinite numbers. In the letters mentioned above he wrote (cf. Cantor 1932, pp. 407–408):

> I mean the following theorem: "Linear numerical quantities $\zeta$ different from zero (i.e., shortly speaking such numerical quantities that can be represented by certain bounded continuous rectilineal segments) which would be smaller than arbitrarily small finite numerical

> quantity, do not exists, that is they are inconsistent with the notion of a rectilineal numerical quantity." My proof is as follows: I am starting from assuming the existence of a rectilineal quantity $\zeta$ which is so small that its $n$th product
>
> $$\zeta \cdot n$$
>
> for any arbitrarily big finite integer $n$ is smaller than one and I am proving now – using the concept of a rectilineal quantity and some theorems of the transfinite number theory – that also
>
> $$\zeta \cdot \nu$$
>
> is smaller that any arbitrarily small finite quantity where $\nu$ denotes an arbitrarily big transfinite ordinal number (i.e., the cardinality (*Anzahl*) or the type of a certain well ordered set) from any arbitrarily high class of numbers. This does not mean that $\zeta$ cannot be made a finite quantity by an arbitrarily big actually infinite product, hence it certainly cannot be an element of finite quantities. Consequently the assumption made at the very beginning is inconsistent with the concept of a rectilineal quantity which has the property that any rectilineal quantity can be thought of as being an integral part of other quantities, in particular of the finite rectilineal quantities. Hence the only thing that can be done is to reject the assumption according to which there would exist such a quantity that for any finite integer $n$ it would be smaller than $\frac{1}{n}$ and in this way our theorem has been proved.

It is rather difficult to agree with this reasoning of Cantor. First he does not make precise the properties that he associates with his "rectilineal numerical quantities". Does he allow nonarchimedian number systems? Secondly, he makes use in his proof of "certain theorems of transfinite number theory" but he does not mention them explicitly.

Today having tools of the model theory and of the nonstandard analysis we know much more about infinitely small quantities and we do not call them "paper quantities" as Cantor did in the letter to S.G. Vivanti from 13th December 1893 mentioned above (cf. Meschkowski 1962–1966, p. 507).

Looking for philosophical justification of his theory of infinite sets Cantor referred to metaphysics and theology. He was convinced that just there one should look for the foundations. In a letter to Th. Esser, a Dominican friar being active in Rome, from 1st February 1896 he wrote (cf. Meschkowski 1967, p. 111 and p. 123):

> The general theory of sets [...] belongs entirely to metaphysics. [...] The founding of principles of mathematics and natural sciences belongs to metaphysics; hence it should treat them as its own children as well as servants and assistants that should not be lost, on the contrary – it should constantly take care of them; as a queen bee residing in a beehive sends thousands of diligent bees to gardens to suck out nectar from flowers and to convert it then under its supervision into excellent honey, so metaphysics – in a similar way – should take care that mathematics and natural sciences bring from the far kingdom of the corporal and spiritual nature bricks to finish its own palace.

Consequently Cantor tried to prove the existence of the transfinite on the ground of the Absolute. This is mentioned, e.g., by cardinal J. Franzelin, a Jesuit, philosopher and theologian, one of the main papal theologians of Vaticanum Primum with whom Cantor corresponded. He wrote answering a letter of Cantor (cf. Cantor 1932, p. 386):

> In a letter to me you are writing [...] that there exists a proof starting from the existence of God and deducing from the highest perfectness of God the possibility of creating an ordered transfinite. Assuming that your *actual* transfinite does not contain any inconsistency, your conclusion on the *possibility of creating* the transfinite deduced from the God's omnipotence is entirely correct.

Cantor believed that the transfinite does not only take anything from the nature of God but – on the contrary – it adds glory to it. The real existence of the transfinite reflects the infinite nature of God's existence. Cantor constructed even two proofs in which, *a priori* and *a posteriori*, resp., showed the existence of transfinite numbers *in concreto*. *A priori*, from the concept of God one can deduce directly, on the base of the perfectness of his nature, the possibility and necessity of the creation of the transfinite.[7] On the other hand constructing the proof *a posteriori* Cantor proved that since it is impossible to explain in a complete way the natural phenomena without assuming the existence of the transfinite *in natura naturata*, hence the transfinite does exists.

Moreover, Cantor was convinced of the great meaning and significance of his set theory for metaphysics and theology. In particular he was of the opinion that it will help to overcome various mistakes appearing in them, for example the mistake of pantheism. In a letter to the Dominican friar Th. Esser from February 1896 he wrote: "Thanks to my works the Christian philosophy has now for the first time in the history the true theory of the infinite" (cf. Dauben 1979, p. 147).

It is worth saying more on the relations between pantheism and Cantor's theory of the infinity. Just the suspicion that Cantor's theory contains elements of pantheism forced catholic theologians, in particular cardinal J. Franzelin mentioned above, to look carefully at his theory. Cantor believed that the infinity exists *in natura naturata*. Franzelin thought that such an opinion was very dangerous and could lead to the pantheism. And the latter, as is known, was acknowledged to be a erroneous doctrine and has been formally condemned by the decree of Pio IX from 1861. Every attempt to correlate God's infinity and a concrete temporal infinity suggested the pantheism. Every actual infinity *in concreto*, *in natura naturata*, could be identified with the infinity of God, *in natura naturans*. Cantor maintaining that his actually infinite transfinite numbers exist *in concreto* seemed to go in the direction of pantheism.

This explains why Franzelin protested against the thesis on the admissibility of the actual infinity *in concreto* maintaining the "possible" infinity only.

[7] This proof was criticized by the cardinal Franzelin mentioned above. In a letter to Cantor he indicates that it is a mistake to deduce the necessity of the creation of something from the assumption of God's perfectness, from his "infinite goodness and magnificence" and from the possibility of the creation act. Such a necessity would in a certain sense limit God's freedom and consequently will diminish his perfectness (cf. Cantor 1932, p. 386).

But Cantor believing in the actual infinity *in concreto* distinguished two types of infinity what reassured the theologians, in particular cardinal Franzelin. In a letter to J. Franzelin of 22nd January 1886 Cantor explained that he added to the distinction between the infinity *in natura naturans* and *in natura naturata* the distinction between *Infinitum aeternum increatum sive Absolutum* (reserved for God and his attributes) and *Infinitum creatum sive Transfinitum* (exemplified for example in the actually infinite number of objects in the Universe). Franzelin was satisfied by those explanations and gave a sort of *imprimatur* to Cantor's works. On this occasion he wrote that the absolute infinity is a proper infinity while the actual infinity (transfinite) should be treated only as the improper infinity. This story explains also what was the reason of distinguishing various types of infinity mentioned above.

Cantor had vivid contacts with catholic philosophers and theologians. This followed not only from common – in a certain sense – interests but also from the fact that just in them he found true and faithful readers and recipients of his conceptions (what strongly and painfully contrasted with the big lack of understanding and aversion he experienced form his colleagues mathematicians). One should mention here the German theologian and philosopher C. Gutberlet, professor of philosophy, apologetics and dogmatics in Fulda, the founder of the journal *Philosophisches Jahrbuch der Görres-Gesellschaft*, leading representative of the neoscholastic thought. He was interested very much in problems of the actual infinity. He thought that the works of Cantor began a new phase in the studies on the infinity. He tried to find in Cantor's works a justification of his own conceptions and the usage of the actually infinite numbers.

Works of Cantor were carefully studied and commented by neoscholastics. One should mention here first of all T. Pesch and J. Hontheim, Benedictines from the abby Maria-Laach in Reihnland as well as the Italian theologian I. Jeiler, the Jesuit and later cardinal J. Franzelin mentioned above and Dominican friar Th. Esser. Cantor listened with care to their opinions and he was very particular about being in accordance with the official catholic doctrine. In a letter to I. Jeiler he wrote in 1896: "Everything concerning this problem (and I am telling you this confidentially) will be of course checked now by Dominican friars in Rome – they correspond with me about this subject. All will be directed by Father Thomas Esser" (cf. Dauben 1979, p. 144). One can wonder why Cantor was so full of fear and did not want to depart from the official doctrine of Roman Catholic Church. He was not Roman Catholic, his father was a Jew and believer of the Judaism. Shortly before Georg's birth he converted to Protestantism. Cantor's mother was Roman Catholic. He himself was brought up in the protestant spirit and in this religion he brought up his own children. Nevertheless he was a very religious man and this can explain his interests in the theological implications of set theory as well as his search for justification of the latter just in theology and metaphysics. Moreover he was convinced about the absolute truth of his set theory because, as he stated, it has been revealed to him. Hence he saw himself not only as a messenger of God transferring exactly the new revealed theory of transfinite numbers but also as an ambassador of God. In a letter to I. Jeiler from 1888 he wrote (cf. Dauben 1979, p. 147):

> I have no doubts towards the truth of the transfinite which I have recognized with the help of God and which I have been studied in its variety for the last twenty years; each year and even each day lead me further in those studies.

The turn of his interests from mathematics to philosophy which took place beginning with the crisis of 1884 was treated by him as a consequence of God's action. In a letter to C. Hermite from 21st January 1894 he wrote (cf. Meschkowski 1967, p. 124):

> I should admit that metaphysics and theology took possession of my soul to such a degree that I have relatively few time left for my first hobby (*für meine erste Flamme*).
>
> If fifteen years ago, even eight years ago, I had been given – in accordance with my wishes – a wilder field of activity, for example at the university in Berlin or Göttingen,[8] I would have probably performed my duties not worse than Fucks, Schwarz, Frobenius, Felix Klein, Heinrich Weber etc. But now I thank God, the best and omniscient, for refusing those wishes because in this way he forced me through deeper penetration of theology to serve him and his holy Roman Catholic Church, to serve better that I would be able to do working in mathematics.

Cantor tried to interpret his results in the religious way. As G. Kowalewski, one of the biographers of Cantor, remembers, the increasing sequence of cardinal numbers was for him "something holy, it formed in a certain sense steps leading to the throne of the Infinite" (cf. Kowalewski 1950). At another place Kowalewski mentiones Cantor's statement connected with the antinomy of the set of all sets (cf. Kowalewski 1950, p. 120):

> In one of the speeches of Solomon on the occasion of the consecration of the temple (1 Kings 8, 27; 2 Chronicles 6, 18) we read: "But will God indeed dwell on the earth? Behold, the heaven and heaven of heavens cannot contain thee; how much less this house that I have builded?"[9] "Heaven of heavens" – does it not remember "the set of all sets"? What Solomon has said can be translated into the language of mathematics as follows: God, the highest infinity can be grasped (*erfasst werden*) neither by a set nor by a set of all sets.

And Kowalewski adds that Cantor "liked such religious thoughts very much".

* * *

Finishing our considerations we can say that Cantor's set theory grew out not only from strictly mathematical considerations but there is a deep philo-

[8]Cantor attempted for a long time to get a position of professor at the university in Berlin or Göttingen. He did not succeed and the whole life was professor at the provincial university in Halle.

[9]Translation according to *The Holy Bible Containing the Old and New Testament. Translated out of the Original Tongues and with the Former Translations Diligently Compared and Revised by His Majesty's Special Command*. London: Trinitarian Bible Society.

sophical background behind it. The concept of the infinite appearing so often in mathematics (and in many cases indispensable) puzzled Cantor. The necessity of explaining the essence of the infinite was the reason and basis of his studies that led him not only towards "dry" technical mathematical considerations but also towards deeper philosophical justifications of the mathematical infinity and his attempts to put it in the context of theological and metaphysical considerations. The connections between those scientific problems and studies with his deepest outlook on life (*Weltanschauung*) is also very interesting and should be stressed.

# 2

# LEIBNIZ'S AND KANT'S PHILOSOPHICAL IDEAS VS. HILBERT'S PROGRAM[1]

The aim of this paper is to indicate some connections between Leibniz's and Kant's philosophical ideas on the one hand and Hilbert's and Gödel's philosophy of mathematics on the other. We shall be interested mainly in issues connected with Hilbert's program and Gödel's incompleteness theorems.

Hilbert's program – which gave rise to one of the important domains of mathematical logic, i.e., to proof theory, and to one of the three main theories in the contemporary philosophy of mathematics, i.e., to formalism – arose in a crisis situation in the foundations of mathematics on the turn of the nineteenth century.[2]

Main controversy centered around the problem of the legitimacy of abstract objects, in particular of the actual infinity. Some paradoxes were discovered in Cantor's set theory but they could be removed by appropriate modifications of the latter. The really embarrassing contradiction was discovered a bit later by Russell in Frege's system of logic.

Hilbert's program was on the one hand a protest against proposals of overcoming those difficulties and securing the edifice of mathematics by restricting the subject and methods of the latter (cf. Brouwer's intuitionism as well as proposals of L. Kronecker, H. Poincaré, H. Weyl) and on the other an attempt to justify the classical (infinite) mathematics and to save its integrity by showing that it is secure.[3] His attitude can be well characterized by his famous (and often

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[1] Originally published under the title "Leibniz's and Kant's Philosophical Ideas and the Development of Hilbert's Program" in *Logique et Analyse* 179–180 (2002), 421–437. Reprinted with kind permission of the editor of the journal *Logique et Analyse*.

[2] It is not clear who introduced the name 'crisis of the foundations of mathematics' (*Grundlagenkrise der Mathematik*) but it was Hermann Weyl who popularized it through his lecture "Über die neue Grundlagenkrise in der Mathematik" held in Zurich – cf. (Weyl 1921).

[3] Detlefsen writes that "Hilbert did want to preserve classical mathematics, but this was not for him an end in itself. What he valued in classical mathematics was its efficiency (including its psychological naturalness) as a mean of locating the truths of real or finitary mathematics. Hence, any

In: Roman Murawski, *Essays in the Philosophy and History of Logic and Mathematics* (*Poznań Studies in the Philosophy of the Sciences and the Humanities*, vol. 98), pp. 29–39. Amsterdam/New York, NY: Rodopi, 2010.

quoted) sentence from (1926): "No one should be able to drive us from the paradise that Cantor created for us" ("Aus dem Paradies, das Cantor uns geschaffen hat, soll uns niemand vertreiben können").

The problem was stated for the first time by Hilbert in his lecture at the Second International Congress of Mathematicians held in Paris in 1900 (cf. Hilbert 1901). Among twenty three main problems which should be solved he mentioned there as Problem 2 the task of proving the consistency of axioms of arithmetic (by which he meant number theory and analysis). He has been returning to the problem of justification of mathematics in his lectures and papers (especially in the twenties) where he proposed a method of solving it.[4] One should mention here his lecture from 1901 held at the meeting of Göttingen Mathematical Society in which he spoke about the problem of completeness and decidability. Hilbert asked there, in E. Husserl's formulation: "Would I have the right to say that every proposition dealing only with the positive integers must be either true or false on the basis of the axioms for positive integers?" (cf. Husserl 1891, p. 445). In a series of lectures in the twenties, Hilbert continued to make the problems more precise and simultaneously communicated partial results obtained by himself and his students and fellow researchers: Paul Bernays, Wilhelm Ackermann, Moses Schönfinkel, John von Neumann. One should mention here Hilbert's lectures in Zurich (1917), Hamburg (1922) (cf. Hilbert 1922), Leipzig (1922), Münster (1925) (cf. Hilbert 1926), second lecture in Hamburg (1927) (cf. Hilbert 1927) and the lecture "Problems der Grundlegung der Mathematik" at the International Congress of Mathematicians held in Bologna (1928) (cf. Hilbert 1929). In the latter Hilbert set out four open problems connected with the justification of classical mathematics which should be solved: (1) to give a (finitistic) consistency proof of the basic parts of analysis (or second-order functional calculus), (2) to extend the proof for higher-order functional calculi, (3) to prove the completeness of the axiom systems for number theory and analysis, (4) to solve the problem of completeness of the system of logical rules (i.e., the first-order logic) in the sense that all (universally) valid sentences are provable. In this lecture Hilbert claimed also – wrongly, as it would turn out – that the consistency of number theory had been already proved.[5]

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alternative to classical mathematics having the same benefits of efficiency would presumably have been equally welcome to Hilbert" (cf. Detlefsen 1990, p. 374).

[4] A good account of the development of Hilbert's views can be found in Smoryński (1988); see also Peckhaus (1990) where detailed analysis of Hilbert's scientific activity in the field of the foundations of mathematics in the period 1899–1917 can be found, as well as Detlefsen (1986) and Ketelsen (1994).

[5] Only after Gödel published his incompleteness theorems in 1931 did Hilbert come to realize that Ackermann's proof, which he meant here, did not establish the consistency of all of number theory. In fact, Ackermann showed in (1924–1925) only the consistency of a fragment of number theory. Cf. also Ackermann (1940). Other results of that type were also obtained by J. von Neumann (1927) and J. Herbrand (1931).

Hilbert's attempts to clarify and to make more precise the program of justifying classical mathematics were accompanied by a philosophical reflection on mathematics. One can see here the turn to idealism,[6] in particular to Kant's philosophy. In fact Hilbert's program was Kantian in character. It can be seen first of all in his paper "Über das Unendliche" (1926). He wrote there:

> Finally, we should recall our true theme and draw the net result of our reflections for the infinite. That net result is this: we find that the infinite is nowhere realized. It is neither present in nature nor admissible as a foundation in that part of our thought having to do with the understanding (*in unserem verstandesmäßigen Denken*) – a remarkable harmony between Being and Thought. We gain a conviction that runs counter to the earlier endeavors of Frege and Dedekind, the conviction that, if scientific knowledge is to be possible, certain intuitive conceptions (*Vorstellungen*) and insights are indispensable; logic alone does not suffice. The right to operate with the infinite can be secured only by means of the finite.
>
> The role which remains for the infinite is rather that of an idea – if, following Kant's terminology, one understands as an idea a concept of reason which transcends all experience and by means of which the concrete is to be completed into a totality [...].[7]

In Kant's philosophy, ideas of reason, or transcendental ideas, are concepts which transcend the possibility of experience but on the other hand are an answer to a need in us to form our judgements into systems that are complete and unified. Therefore we form judgements concerning an external reality which are not uniquely determined by our cognition, judgements concerning things in themselves. To do that we need ideas of reason.

Kant claimed that space and time as forms of intuition (*Formen der reinen Anschauung*) suffice to justify and to found the notion of potential infinity (the actual infinity was not considered by him). Hilbert indicated a mistake in this approach. Since the actual infinity cannot be justified by purely logical means, Hilbert treated it as an ideal element. Hence his solution to the problem of the actual infinity was in fact of a Kantian character.

In likening the infinite to a Kantian idea, Hilbert suggests that it is to be understood as a regulative rather than a descriptive device. Therefore sentences concerning the infinite, and generally expressions which Hilbert called ideal propositions, should not be taken as sentences describing externally existing entities. In fact they mean nothing in themselves, they have no truth-value and they cannot be the content of any genuine judgement. Their role is rather regulative than descriptive. But on the other hand they are necessary in our thinking. Hence their similarity to Kant's ideas of reason is evident – they play the similar cognitive role. We do not claim that the ideal elements by Hilbert are the same as (are identical with) ideas of reason by Kant. We claim only that they play the same role, i.e., they enable us to preserve the rules of reason (*Verstand*) in a simple,

[6] Detlefsen says in this context about instrumentalism – cf. his (1986).
[7] English translation after Detlefsen (1993a).

uniform and simultaneously general form. It is achieved just by extending the domain by ideal elements (ideas) (cf. Majer 1993a; 1993b).

We use ideas of reason and ideal elements in our thinking because they allow us to retain the patterns of classical logic in our reasoning. But the operations of the classical logic can no longer be employed semantically as operations on meaningful propositions – there is nothing in the externally existing reality that would correspond to ideal elements and ideas of reason, in fact they are free creations of our reason and have only "symbolic" meaning. Their meaning can be determined only by an analogy – they cannot be understood as given by intuition (*Anschauung*). Therefore the operations of the classical logic should be understood only syntactically, as operations on signs and strings of sings. Hilbert wrote in (1926):

> We have introduced the ideal propositions to ensure that the customary laws of logic again hold one and all. But since the ideal propositions, namely, the formulas, insofar as they do not express finitary assertions, do not mean anything in themselves, the logical operations cannot be applied to them in a contentual way, as they are to the finitary propositions. Hence, it is necessary to formalize the logical operations and also the mathematical proofs themselves; this requires a transcription of the logical relations into formulas, so that to the mathematical signs we must still adjoin some logical signs, say
>
> | & | $\vee$ | $\rightarrow$ | $-$ |
> |---|---|---|---|
> | and | or | implies | not |
>
> and use, besides the mathematical variables, $a, b, c, \ldots$, also logical variables, namely variable propositions $A, B, C, \ldots$

Hence the abstracting from meaning of expressions is connected with Hilbert's attempt to preserve the laws of classical logic as laws governing mathematical thinking and reasoning. It is also connected with the distinction between real and ideal propositions according to which real propositions play the role of Kant's judgements of the understanding (*Verstand*) and the ideal propositions the part of his ideas of pure reason. In (1926) Hilbert wrote:

> Kant taught – and it is an integral part of his doctrine – that mathematics treats a subject matter which is given independently of logic. Mathematics, therefore can never be grounded solely on logic. Consequently, Frege's and Dedekind's attempts to so ground it were doomed to failure.
>
> As a further precondition for using logical deduction and carrying out logical operations, something must be given in conception, viz., certain extralogical concrete objects which are intuited as directly experienced prior to all thinking. For logical deduction to be certain, we must be able to see every aspect of these objects, and their properties, differences, sequences, and contiguities must be given, together with the objects themselves, as something which cannot be reduced to something else and which requires no reduction. This is the basic philosophy which I find necessary not just for mathematics, but for all scientific thinking, understanding and communicating. The subject mater of mathematics is, in accordance with this theory, the concrete symbols themselves whose structure is immediately clear and recognizable.

According to this Hilbert distinguished between the unproblematic, finitistic part of mathematics and the infinitistic part that needed justification. Finitistic mathematics deals with so called real propositions, which are completely meaningful because they refer only to given concrete objects. Infinitistic mathematics on the other hand deals with so called ideal propositions that contain reference to infinite totalities.

By Hilbert, analogously as it was by Kant, ideal propositions (and ideal elements) played an auxiliary role in our thinking, they were used to extend our system of real judgements. Hilbert believed that every true finitary proposition had a finitary proof. Infinitistic objects and methods enabled us to give easier, shorter and more elegant proofs but every such proof could be replaced by a finitary one. This is the reflection of Kant's views of the relationship between the ideas of reason and the judgements of the understanding (cf. Kant 1787, p. 383).

Kant's and Hilbert's ideas on the nature of mathematics and the character of its propositions described above resemble also some ideas of Leibniz. Leibniz's ideas on the nature of mathematics can be characterized as simultaneously platonic and nominalistic. His nominalism was rather ontological than linguistical. He claimed that we can point out mathematical objects such as, for example, geometrical figures, only by our reason. They do not exist anywhere in things of the external world. The mathematics of the Nature does not apply to the substance, to what is ontologically primitive. To the latter applies mathematics of ideas and possibilities, i.e., God's real mathematics.

Let us return to Hilbert's program. According to that the infinitistic mathematics can be justified only by finitistic methods because only they can give it security (*Sicherheit*). Hilbert's proposal was to base mathematics on finitistic mathematics via proof theory (*Beweistheorie*).[8] It was planned as a new mathematical discipline in which one studies mathematical proofs by mathematical methods. Its main goal was to show that proofs which use ideal elements in order to prove results in the real part of mathematics always yield correct results. One can distinguish here two aspects: consistency problem and conservation problem.

The consistency problem consists in showing by finitistic methods that the infinitistic mathematics is consistent;[9] the conservation problem consists in show-

[8]Later Hilbert named it metamathematics (*Metamathematik*). This name was used by him for the first time in his lecture "Neubegründung der Mathematik" (1922). It is worth noting that the very term 'Metamathematik', though in another meaning, appeared already in the nineteenth century in connection with discussions on non-Euclidean geometries. It was constructed in the analogy to the word 'Metaphysik' (metaphysics) and had a pejorative meaning. For instance, F. Schultze in (1881) said about "*die metamathematischen Spekulationen über den Raum*" (metamathematical speculations about the space). B. Erdmann and H. von Helmholtz contributed to the change of the meaning of this term to a positive one.

[9]Note that consistency of the mathematical domain extended by ideal elements (which have

ing by finitistic methods that any real sentence which can be proved in the infinitistic part of mathematics can be proved also in the finitistic part, i.e., that infinitistic mathematics is conservative over finitistic mathematics with respect to real sentences and, even more, that there is a finitistic method of translating infinitistic proofs of real sentences into finitistic ones. Both those aspects are interconnected.

Hilbert's proposal to carry out this program consisted of two steps. To be able to study seriously mathematics and mathematical proofs one should first of all define accurately the notion of a proof. In fact, the concept of a proof used in mathematical practice is intuitive, loose and vague, it has clearly a subjective character. This does not cause much trouble in practice. On the other hand if one wants to study mathematics as a science – as Hilbert did – then one needs a precise notion of proof. This was provided by mathematical logic. In works of G. Frege and B. Russell (who used ideas and achievements of G. Peano) one finds an idea (and its implementations) of a formalized system in which a mathematical proof is reduced to a series of very simple and elementary steps, each of which consisting of a purely formal transformation on the sentences which were previously proved. In this way the concept of mathematical proof was subjected to a process of formalization. Therefore the first step proposed by Hilbert in realization of his program was to formalize mathematics, i.e., to reconstitute infinitistic mathematics as a big, elaborate formal system (containing classical logic, infinite set theory, arithmetic of natural numbers, analysis). An artificial symbolic language and rules of building well-formed formulas should be fixed. Next axioms and rules of inference (referring only to the form, to the shape of formulas and not to their sense or meaning) ought to be introduced. In such a way theorems of mathematics become those formulas of the formal language which have a formal proof based on a given set of axioms and given rules of inference. There was one condition put on the set of axioms (and rules of inference): they ought to be chosen in such a way that they suffice to solve any problem formulated in the language of the considered theory as a real sentence, i.e., they ought to form a complete set of axioms with respect to real sentences.

The second step of Hilbert's program was to give a proof of the consistency and conservativeness of mathematics. Such a proof should be carried out by finitistic methods. This was possible since the formulas of the system of formalized mathematics are strings of symbols and proofs are strings of formulas, i.e., strings of strings of symbols. Hence they can be manipulated finitistically. To prove the consistency it suffices to show that there are not two sequences of formulas (two formal proofs) such that one of them has as its end element a formula $\varphi$ and the other $\neg\varphi$ (the negation of the formula $\varphi$). To show conservativeness

only "symbolic" meaning given by analogy and not by intuition (*Anschauung*)) corresponds to the regulative role of the ideas of reason by Kant.

it should be proved that any proof of a real sentence can be transformed into a proof not referring to ideal objects.

Having formulated the program of justification of the classical (infinite) mathematics Hilbert and his students set out to realize it. And they scored some successes – cf., e.g., Ackermann (1924–1925; 1940) or von Neumann (1927). But soon something was to happen that undermined Hilbert's program.

In 1930 Kurt Gödel proved two theorems, called today Gödel's incompleteness theorems, which state that (1) arithmetic of natural numbers and all richer formal systems are essentially incomplete provided they are consistent and that (2) no consistent theory containing arithmetic of natural numbers proves its own consistency (cf. Gödel 1931a).

Gödel's results struck Hilbert's program. We shall not consider here the problem whether they rejected it.[10] We shall rather ask what were the reactions and opinions of Hilbert and Gödel. It turns out that their proposals and solutions were strongly connected with some ideas of Leibniz.

Having learned about Gödel's result (i.e., the first incompleteness theorem) Hilbert "was angry at first, but was soon trying to find a way around it" (cf. Smoryński 1988). He proposed to add to the rules of inference a simple form of the $\omega$-rule:

$$\frac{\varphi(0), \varphi(1), \varphi(2), \ldots, \varphi(n), \ldots \qquad (n \in \mathbf{N})}{\forall x\, \varphi(x)}.$$

This rule allows the derivation of all true arithmetical sentences but, in contrast to all rules of the first-order logic, it has infinitely many premisses.[11] In Preface to the first volume of Hilbert and Bernays' monograph *Grundlagen der Mathematik* (1934, 1939) Hilbert wrote:

> [...] the occasionally held opinion, that from the results of Gödel follows the non-executability of my Proof Theory, is shown to be erroneous. This result shows indeed only that for more advanced consistency proofs one must use the finite standpoint in a deeper way than is necessary for the consideration of elementary formalism.

To be able to indicate some connections between those ideas of Hilbert and some ideas of Leibniz, let us recall that according to Leibniz theorems are either primitive truths, i.e., axioms of a given theory, or propositions (called derived truths) which can be reduced to the primitive ones by means of a proof. A proof consists either of a finite number of steps (in this case one says about finitistic truth or about finitely analytic sentences) or of an infinite number of steps (one deals then with infinitistic truths or with infinitely analytic sentences). Primitive

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[10] This problem is discussed, e.g., in Murawski (1994; 1999a).

[11] In fact the rule proposed by Hilbert in his lecture in Hamburg in December 1930 (cf. Hilbert 1931) had rather informal character (a system obtained by admitting it would be semi-formal). Hilbert proposed that whenever $A(z)$ is a quantifier-free formula for which it can be shown (finitarily) that $A(z)$ is correct (*richtig*) numerical formula for each particular numerical instance $z$, then its universal generalization $\forall x A(x)$ may be taken as a new premise (*Ausgangsformel*) in all further proofs.

truths are necessary and finitely (directly) analytic, derived truths are either necessary (though not identical), i.e., finitely analytic, or contingent, i.e., infinitely analytic. On the other hand Leibniz claimed that necessary truths possess finite proofs while contingent ones have only infinite proofs. So the difference between them is rather of a practical character and not of a substantial character. A being with unbounded calculating possibilities would be able to decide all the truths directly, hence any truth would be for him necessary. Here is the source and reason for Leibniz's idea of a real logical calculus, *ars combinatoria* (only its fragments are known to us in the form of finitistic systems of logic). This calculus must be infinitistic. So Leibniz allowed infinite elements in reasonings and proofs. Consequently Hilbert's proposal to allow the $\omega$-rule is compatible with Leibniz's ideas and has in fact the Leibnizian character.

An explicit influence of Leibniz on Gödel's reactions on the new situation in logic and the foundations of mathematics after incompleteness results can be also seen. Before we present and discuss some details note that after 1945 Gödel's interests were concentrated almost exclusively on the philosophy of mathematics and on the philosophy in general. He studied works of Kant and Leibniz as well as phenomenological works of Husserl (especially in the fiftieth). In his *Nachlaß* several notes on the works of those philosophers and on their views were found. Gödel claimed that it was just Leibniz who had mostly influenced his own scientific thinking and activity (Gödel studied Leibniz's works already in the thirties). Hao Wang writes that "Gödel's major results and projects can be viewed as developments of Leibniz's conceptions along several directions" (cf. Wang 1987, p. 261). Gödel accepted main ideas of Leibniz's monadology, he was interested in a realization of a modified form of *characteristica universalis* (Gödel's incompleteness theorems indicated that the idea cannot be fully realized). Both Leibniz and Gödel were convinced of the meaning and significance of the axiomatic method. Gödel's results indicated the necessity of some changes and modifications in Leibniz's program, though Gödel was still looking for axiomatic principles for metaphysics from which the whole of knowledge could be deduced (or which at least would be a base of any knowledge). He was searching among others for a method of analyzing concepts which would induce methods allowing to obtain new results. Add also that Gödel first got the idea of his proof of the existence of God in reading Leibniz (cf. Wang 1987, p. 195).

Having presented the connections between Leibniz and Gödel in general, let us turn now to problems related to the incompleteness theorems. Observe at the beginning that in his first philosophical paper "Russell's Mathematical Logic" (1944) Gödel has turned among others to the question whether (and in which sense) the axioms of *Principia Mathematica* can be considered to be analytic. And he answered that if analyticity is understood as reducibility by explicit or contextual definitions to instances of the law of identity then even arithmetic is not analytic because of its undecidability. He wrote in (1944, p. 150):

> [...] analyticity may be understood in two senses. First, it may have the purely formal sense that the terms occurring can be defined (either explicitly or by rules for eliminating them from sentences containing them) in such a way that the axioms and theorems become special cases of the law of identity and disprovable propositions become negations of this law. In this sense even the theory of integers is demonstrably non-analytic, provided that one requires of the rules of elimination that they allow one actually to carry out the elimination in a finite number of steps in each case.[12]

The inspiration of Leibniz can be easily seen here (compare Leibniz's finitely analytic truths). On the other hand if infinite reduction, with intermediary sentences of infinite length, is allowed (as would be suggested by Leibniz's theory of contingent propositions) then all the axioms of *Pricipia* can be proved analytic, but the proof would require "the whole of mathematics". Gödel wrote in (1944, pp. 150–151):

> Leaving out this condition by admitting, e.g., sentences of infinite (and non-denumerable) length as intermediate steps of the process of reduction, all axioms of *Principia* (including the axioms of choice, infinity and reducibility) could be proved to be analytic for certain interpretations [...] But this observation is of doubtful value, because the whole of mathematics as applied to sentences of infinite length has to be presupposed in order to prove this analyticity, e.g., the axiom of choice can be proved to be analytic only if it is assumed to be true.

What concerns problems directly connected with the incompleteness results one should note that already in (1931a) Gödel wrote explicitly:

> I wish to note expressly that Theorem XI (and the corresponding results for M and A) do not contradict Hilbert's formalistic viewpoint. For this viewpoint presupposes only the existence of a consistency proof in which nothing but finitary means of proof is used, and it is conceivable that there exist finitary proofs that cannot be expressed in the formalism of P (or M or A).[13]

And in the footnote $48^a$ (evidently an afterthought) to (1931a) Gödel wrote:

> As will be shown in Part II of this paper, the true reason for the incompleteness inherent in all formal systems of mathematics is that the formation of ever higher types can be continued into the transfinite [...] while in any formal system at most denumerably many of them are available. For it can be shown that the undecidable propositions constructed here become decidable whenever appropriate higher types are added (for example, the type $\omega$ to the system P). An analogous situation prevails for the axiom system of set theory.[14]

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[12] Because this would imply the existence of a decision procedure for all arithmetical propositions. Cf. *Turing 1937*. [Gödel's footnote]

[13] Theorem XI states that if P (the system of arithmetic of natural numbers used by Gödel in (1931a) and based on the system of *Principia*) is consistent then its consistency is not provable in P; M is set theory and A is classical analysis. English translation according to Heijenoort (1967, p. 615).

[14] English translation taken from Heijenoort (1967, p. 610). Note that as one of the confirmations of Gödel's thesis expressed here can serve the fact that if, e.g., T is an extension of Peano arithmetic

At the Vienna Circle meeting on 15th January 1931 Gödel argued that it is doubtful, "whether all intuitionistically correct proofs can be captured in a *single* formal system. That is the weak spot in Neumann's argumentation".[15]

Gödel suggested that Hilbert's program may be continued by allowing two principles which can be treated as finitistic, namely (1) the principle of transfinite induction on certain primitive recursive well-orderings, and (2) a notion of computable functions of finite type (i.e., of computable functionals), to which the process of primitive recursion can be extended in a natural way.

The first principle (more exactly, the induction up to the ordinal $\varepsilon_0$) was later applied by Gerhard Gentzen to prove the consistency of the arithmetic of natural numbers (cf. Gentzen 1936; 1938). Later many Gentzen style proofs of the consistency of various fragments of analysis and set theory have been given – cf., e.g., the monographs by Schütte (1960) and Takeuti (1975).

The second principle was applied by Gödel in (1958). He considered there the question of how far finitary reasoning might reach. The problem was considered by him also in (1972) (this paper was a revised and expanded English version of (1958)). Gödel claimed here that concrete finitary methods are insufficient to prove the consistency of elementary number theory and some abstract concepts must be used in addition. He wrote (pp. 271–273):

> P. Bernays has pointed out [...] on several occasions that, in view of the fact that the consistency of a formal system cannot be proved by any deductive procedures available in the system itself, it is necessary to go beyond the framework of finitary mathematics in Hilbert's sense in order to prove the consistency of classical mathematics or even of classical number theory. Since finitary mathematics is defined [...] as the mathematics of *concrete intuition*, this seems to imply that *abstract concepts* are needed for the proof of consistency of number theory. [...] By abstract concepts, in this context, are meant concepts which are essentially of the second or higher level, i.e., which do not have as their content properties or relations of *concrete objects* (such as combinations of symbols), but rather of *thought structures* or *thought contents* (e.g., proofs, meaningful propositions, and so on), where in the proofs of propositions about these mental objects insights are needed which are not derived from a reflection upon the combinatorial (space-time) properties of the symbols representing them, but rather from a reflection upon the *meanings* involved.

And in the footnote b to (1972) Gödel added:

> What Hilbert means by 'Anschauung' is substantially Kant's space-time intuition confined, however, to configurations of a finite number of discrete objects. Note that it is Hilbert's insistence on *concrete* knowledge that makes finitary mathematics so surprisingly weak and excludes many things that are just as incontrovertibly evident to everybody as finitary number theory. E.g., while any primitive recursive definition is finitary, the general principle of primitive recursive definition is not a finitary proposition, because it contains the abstract concept of function. There is nothing in the term 'finitary' which would suggest a restriction

PA and a predicate $S$ of the language L(T) is a satisfaction predicate for the language L(PA) with the appropriate properties then T proves consistency of PA (cf., e.g., Murawski 1997; 1999b).

[15] Quotation after Sieg (1988).

> to concrete knowledge. Only Hilbert's special interpretation of it makes this restriction. (p. 272)

However he was convinced that a precise definition of concrete finitary method would have to be given in order to establish with certainty the necessity of using abstract concepts. In (1946) he explicitly called for an effort to use progressively more powerful transfinite theories to derive new arithmetical theorems or new theorems of set theory. He wrote (p. 151):

> Let us consider, e.g., the concept of demonstrability. It is well known that, in whichever way you make it precise by means of a formalism, the contemplation of this very formalism gives rise to new axioms which are exactly as evident and justified as those with which you started, and that this process of extension can be iterated into the transfinite. So there cannot exist any formalism which would embrace all these steps; but this does not exclude that all these steps (or at least all of them which give something new for the domain of propositions in which you are interested) could be described and collected together in some non-constructive way. In set theory, e.g., the successive extensions can most conveniently be represented by stronger and stronger axioms of infinity. It is certainly impossible to give a combinational and decidable characterization of what an axiom of infinity is; but there might exist, e.g., a characterization of the following sort: An axiom of infinity is a proposition which has a certain (decidable) formal structure and which in addition is true. Such a concept of demonstrability might have the required closure property, i.e., the following could be true: Any proof for a set-theoretic theorem in the next higher system above set theory (i.e., any proof involving the concept of truth which I just used) is replaceable by a proof from such an axiom of infinity. It is not impossible that for such a concept of demonstrability some completeness theorem would hold which would say that every proposition expressible in set theory is decidable from the present axioms plus some true assertion about the largeness of the universe of all sets.

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The above considerations indicated some connections between the philosophy of Leibniz and Kant on the one hand and ideas of Hilbert and Gödel concerning the philosophy of mathematics on the other. We showed that the essential assumptions of Hilbert's program (i.e., the distinction between real and ideal propositions and the conception of the role and meaning of ideal elements in the mathematical knowledge) are connected with some distinctions made by Leibniz and Kant (ideas of reason). On the other hand Hilbert's and Gödel's proposals how to continue the program of justification of (infinitary) mathematics after the discovery of the incompleteness phenomenon indicate some influence of Leibniz, namely the admittance, at least in certain contexts, of some of the infinitistic methods and principles.

# 3

# TRUTH VS. PROVABILITY. PHILOSOPHICAL AND HISTORICAL REMARKS[1]

**1.** Since Plato, Aristotle and Euclid the axiomatic method was considered as the best method to justify and to organize mathematical knowledge. The first mature and most representative example of its usage in mathematics were *Elements* of Euclid. They established a pattern of a scientific theory and in particular a paradigm in mathematics. Since Euclid till the end of the nineteenth century mathematics was developed as an axiomatic (in fact rather a quasi-axiomatic) theory based on axioms and postulates. Proofs of theorems contained several gaps – in fact the lists of axioms and postulates were not complete, one freely used in proofs various "obvious" truths or referred to the intuition. Consequently proofs were only partially based on axioms and postulates. In fact proofs were informal and intuitive, they were rather demonstrations and the very concept of a proof was of a psychological (and not of a logical) nature. Note that almost no attention was paid to making precise and to specifying of the language of theories – in fact the language of the theories was simply the imprecise colloquial language. One should also note here that in fact till the end of the nineteenth century mathematicians were convinced that axioms and postulates should be simply true statements, hence sentences describing the real state of affairs (in mathematical reality). It seems to be connected with Aristotle's view that a proposition is demonstrated (proved to be true) by showing that it is a logical consequence of propositions already known to be true. Demonstration was conceived here of as a deduction whose premises are known to be true and a deduction was conceived of as a chaining of immediate inferences.

Add that the Euclid's approach (connected with Platonic idealism) to the problem of the development of mathematics and the justification of its state-

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ments (which found its fulfilment in the Euclidean paradigm), i.e., justification by deduction (by proofs) from explicitly stated axioms and postulates, was not the only approach and method which was used in the ancient Greek (and later). The other one (call it heuristic) was connected with Democritus' materialism. It was applied for example by Archimedes who used not only deduction but any methods, such as intuition or even experiments (not only mental ones), to solve problems. Though the Euclidean approach won and dominated in the history, one should note that it formed rather an ideal and not the real scientific practice of mathematicians. In fact rigorous, deductive mathematics was rather a rare phenomenon. On the contrary, intuition and heuristic reasoning were the animating forces of mathematical research practice. The vigorous but rarely rigorous mathematical activity produced "crises" (for example the Pythagoreans' discovery of the incommensurability of the diagonal and side of a square, Leibniz's and Newton's problems with the explanation of the nature of infinitesimals, Fourier's "proof" that any function is representable in a Fourier series, antinomies connected with Cantor's imprecise and intuitive notion of a set).

Basic concepts underlying the Euclidean paradigm have been clarified on the turn of the nineteenth century. In particular the intuitive (and rather psychological in nature) concept of an informal proof (demonstration) was replaced by a precise notion of a formal proof and of a consequence. Several events and achievements contributed to the revision of the Euclidean paradigm, in particular the origin and the development of set theory (G. Cantor), arithmetization of analysis (A. Cauchy and K. Weierstrass, R. Dedekind), axiomatization of the arithmetic of natural numbers (G. Peano), non-Euclidean geometries (N.I. Lobachewsky, J.Bolayi, C.F. Gauss), axiomatization of geometry (M. Pasch, D. Hilbert), the development of mathematical logic (G. Boole, A. de Morgan, G. Frege, B. Russell). Beside those "positive" factors there was also a "negative" factor, viz., the discovery of paradoxes in set theory (C. Burali-Forti, G. Cantor, B. Russell) and of semantical antinomies (G.D. Berry, K. Grelling). They forced the revision of some basic ideas and stimulated in particular metamathematical investigations.

One of the directions of those foundational investigations was the program of David Hilbert and his *Beweistheorie*. Note at the very beginning that "this program was never intended as a comprehensive philosophy of mathematics; its purpose was instead to legitimate the entire corpus of mathematical knowledge" (cf. Rowe 1989, p. 200). Note also that Hilbert's views were changing over the years, but always took a formalist direction.

**2.** Hilbert sought to justify mathematical theories by means of formal systems, i.e., using the axiomatic method. He viewed the latter as holding the key to a systematic organization of any sufficiently developed subject. This idea was very well stated already in a letter of 29th December 1899 to G. Frege where

Hilbert explained his motives of axiomatizing the geometry and wrote (cf. Frege 1976, p. 67):

> I was forced to construct my systems of axioms by a necessity: I wanted to have a possibility to understand those geometrical propositions which in my opinion are the most important results of geometrical researches: that the Parallel Postulate is not a consequence of other axioms, and similarly for the Archimedean one, etc.[2]

In "Axiomatisches Denken" (1918, p. 405) Hilbert wrote:

> When we put together the facts of a given more or less comprehensive field of our knowledge, then we notice soon that those facts can be ordered. This ordering is always introduced with the help of a certain *network of concepts* in such a way that to every object of the given field corresponds a concept of this network and to every fact within this field corresponds a logical relation between concepts. The network of concepts is nothing else than the *theory* of the field of knowledge.[3]

By Hilbert the formal frames were contentually motivated. First-order theories were viewed by him together with suitable non-empty domains, *Bereiche*, which indicated the range of the individual variables of the theory and the interpretations of the nonlogical vocabulary. But Hilbert, as a mathematician, was not interested in establishing precisely the ontological status of mathematical objects. Moreover, one can say that his program was calling on people to turn their mathematical and philosophical attention away from the problem of the object of mathematical theories and turn it toward a critical examination of the methods and assertions of theories. On the other hand he was aware that once a formal theory has been constructed, it can admit various interpretations. Recall here his famous sentence from a letter to G. Frege quoted already above:

> Yes, it is evident that one can treat any such theory only as a network or schema of concepts besides their necessary interrelations, and to think of basic elements as being any objects. If I think of my points as being any system of objects, for example the system: love, law, chimney-sweep [...], and I treat my axioms as [expressing] interconnections between those objects, then my theorems, e.g., the theorem of Pythagoras, hold also for those things. In other words: any such theory can always be applied to infinitely many systems of basic elements.[4]

[2] "Ich bin zu der Aufstellung meines Systems von Axiomen durch die Not gezwungen: ich wollte die Möglichkeit zum Verständnis derjenigen geometrischen Sätze geben, die ich für die wichtigsten Ergebnisse der geometrischen Forschungen halte: dass das Parallelenaxiom keine Folge der übrigen Axiome ist, ebenso das Archimedische etc."

[3] "Wenn wir die Tatsachen eines bestimmten mehr oder minder umfassenden Wissensgebiete zusammenstellen, so bemerken wir bald, daß diese Tatsachen einer Ordnung fähig sind. Diese Ordnung erfolgt jedesmal mit Hilfe eines gewissen *Fachwerkes von Begriffen* in der Weise, daß dem einzelnen Gegenstande des Wissensgebietes ein Begriff dieses Fachwerkes und jeder Tatsache innerhalb des Wissensgebietes eine logische Beziehung zwischen den Begriffen entspricht. Das Fachwerk der Begriffe ist nicht Anderes als die *Theorie* des Wissensgebietes."

[4] "Ja, es ist doch selbsverständlich eine jede Theorie nur ein Fachwerk oder Schema von Begriffen nebst ihren nothwendigen Beziehungen zu einander, und die Grundelemente können in beliebiger Weise gedacht werden. Wenn ich unter meinen Punkten irgendwelche Systeme von Dingen, z.B.

The essence of the axiomatic study of mathematical truths was for him to clarify the position of a given theorem (truth) within the given axiomatic system and the logical interconnections between propositions.[5]

Hilbert sought to secure the validity of mathematical knowledge by syntactical considerations without appeal to semantic ones. The basis of his approach was the distinction between the unproblematic, "finitistic" part of mathematics and the "infinitistic" part that needed justification. Finitistic mathematics deals with so called real propositions, which are completely meaningful because they refer only to given concrete objects. Infinitistic mathematics on the other hand deals with so called ideal propositions that contain reference to infinite totalities. Hilbert proposed to base mathematics on finitistic mathematics via proof theory (*Beweistheorie*). The latter was planned as a new mathematical discipline in which one studies mathematical proofs by mathematical methods. Its main goal was to show that proofs which use ideal elements (in particular actual infinity) in order to prove results in the real part of mathematics always yield correct results. One can distinguish here two aspects: consistency problem and conservation problem. The consistency problem consists in showing (by finitistic methods, of course) that the infinitistic mathematics is consistent; the conservation problem consists in showing by finitistic methods that any real sentence which can be proved in the infinitistic part of mathematics can be proved also in the finitistic part. One should stress here the emphasis on consistency (instead of correctness).

To realize this program one should formalize mathematical theories (even the whole of mathematics) and then study them as systems of symbols governed by specified and fixed combinatorial rules. The advantage of this approach was the fact that references to ideal objects were replaced by reasonings of a purely finitary character, reasonings applied not to mathematical entities themselves but to the symbols of a formal language in which the concepts had been axiomatized, i.e., by syntactical considerations without appeal to the semantic ones. Another advantage was – as Bernays remarked – the fact that the problems and difficulties

das System: Liebe, Gesetz, Schornsteinfeger [...] denke und dann nur meine sämtlichen Axiome als Beziehungen zwischen diesen Dingen annehme, so gelten meine Sätze, z.B. der Pythagoras auch von diesen Dingen. Mit anderen Worten: eine jede Theorie kann stets auf unendliche viele Systeme von Grundelementen angewandt werden."

[5]He wrote in (1902–1903, p. 50): "By the axiomatic study of any mathematical truth I understand a study whose aim is not to discover new or more general propositions with the help of given truths, but a study whose purpose is to determine a position of a given theorem within the system of known truths and their logical connections in such a way that one can clearly see which assumptions are necessary and sufficient to justify the considered truth." ("Unter der axiomatischen Erforschung einer mathematischen Wahrheit verstehe ich eine Untersuchung, welche nicht dahin zieht, im Zusammenhang mit jener Wahrheit neue oder allgemeinere Sätze zu entdecken, sondern die vielmehr die Stellung jenes Satzes innerhalb des Systems der bekannten Wahrheiten und ihren logischen Zusammenhang in der Weise klarzulegen sucht, dass sich sicher angeben lässt, welche Voraussetzungen zur Begründung jener Wahrheit notwendig und hinreichend sind.")

in the foundations of mathematics can be transferred from the epistemological-philosophical to the properly mathematical domain.

The formal axiomatic system should satisfy three conditions: it should be complete, consistent and based on independent axioms. The consistency of a given system was the criterion for mathematical truth and for the very existence of mathematical objects.[6] It was also presumed that any consistent theory would be categorical, that is, would (up to isomorphism) characterize a unique domain of objects. This demand was connected with the completeness.

The meaning and understanding of completeness by Hilbert plays a crucial rôle from the point of view of our subject. Note at the beginning that in the *Grundlagen der Geometrie* completeness was postulated as one of the axioms (the axiom was not present in the first edition, but was included first in the French translation and then in the second edition of 1903). In fact the axiom V(2) stated that: "Elements of geometry (i.e., points, lines and planes) form a system of things does not admit any extension provided all the mentioned axioms are preserved" (1903, p. 31).[7] In Hilbert's lecture at the Congress at Heidelberg in 1904 (cf. 1905b) one finds such an axiom for the real numbers. Later there appears completeness as a property of a system. In lectures "Logische Principien des mathematischen Denkens" (1905a, p. 13) Hilbert explains the demand of the completeness as the demand that the axioms suffice to prove all "facts" of the theory in question. He says: "We will have to demand that all other facts of the given field are consequences of the axioms."[8] On the other hand one can say that Hilbert's early conviction as to the solvability of every mathematical problem – expressed for example in his 1900 Paris lecture (cf. Hilbert 1901) and repeated in his opening address "Naturerkennen und Logik" (cf. Hilbert 1930b) before the Society of German Scientists and Physicians in Königsberg in September 1930 – can be treated as informal reflection of his belief in completeness of axiomatic theories. In Paris Hilbert said (cf. 1901, p. 298):

[6]Cf. Hilbert's letter to G. Frege of 29th December 1899 where he claimed that: "If the arbitrary given axioms do not contradict one another with all their consequences, then they are true and the things defined by the axioms exist." ("Wenn sich die willkürlich gesetzten Axiome nicht einander widersprechen mit sämtlichen Folgen, so sind sie wahr, so existieren die durch die Axiome definierten Dinge.") (cf. Frege 1976, p. 66).

[7]"Die Elemente (Punkte, Geraden, Ebenen) der Geometrie bilden ein System von Dingen, welches bei Aufrechterhaltung sämtlicher genannten Axiome keiner Erweiterung mehr fähig ist."

In last editions of *Grundlagen*, beginning with the seventh edition from 1930, Hilbert replaced this axiom by the axiom of lineare completeness stating that: "Points of a line form a system which admits no extension provided the linear order of the line (Theorem 6), the first congruence axioms and Archimedean axioms (i.e., axioms I1–2, II, III1, V1) are preserved." ("Die Punkte einer Geraden bilden ein System, welches bei Aufrechterhaltung der linearen Anordnung (Satz 6), des ersten Kongruenzaxioms und des Archimedischen Axioms (d.h. der Axiome I1–2, II, III1, V1) keiner Erweiterung mehr fähig ist.")

[8]"Wir werden verlangen müssen, dass alle übrigen Thatsachen des vorgelegten Wissensbereiches Folgerungen aus den Axiomen sind."

> The conviction of the solvability of any mathematical problem is for us a strong motive in this work; we hear the whole time the call: *There is a problem, look for a solution. You can find it by a pure thinking; there is no Ignorabimus in the mathematics*.[9]

And in Königsberg he said (cf. 1930b, p. 962):

> For the mathematician there is no Ignorabimus, and, in my opinion, not at all for natural science either. [...] The true reason why [no one] has succeeded in finding an unsolvable problem is, in my opinion, that there is *no* unsolvable problem. In contrast to the foolish Ignorabimus, our credo avers: We must know, We shall know.[10]

In his 1900 Paris lecture Hilbert spoke about completeness in the following words (see Hilbert 1901, second problem, pp. 299–300):

> When we are engaged in investigating the foundations of a science, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of that science. The axioms so set up are at the same time the definitions of those elementary ideas; and no statement within the realm of the science whose foundations we are testing is held to be correct unless it can be derived from those axioms by means of a finite number of logical steps.[11]

One can assume that the phrase 'exact and complete description' (*genaue und vollständige Beschreibung*) is equivalent to the requirement that this description is complete in the sense that it allows to decide decide the truth or falsity of every statement of the given theory. Semantically such completeness follows from categoricity, i.e., from the fact that any two models of a given axiomatic system are isomorphic; syntactically it means that every sentence or its negation is derivable from the given axioms. Hilbert's own axiomatizations were complete in the sense of being categorical. But notice that they were not first-order, indeed his axiomatization of geometry from *Grundlagen* as well as his axiomatization of arithmetic published in 1900 were second-order. Each of those system had a second-order Archimedean axiom and both had a "completeness axiom" stating

[9] "Diese Überzeugung von der Lösbarkeit eines jeden mathematischen Problems ist uns ein kräftiger Ansporn während der Arbeit; wir hören in uns den steten Zuruf: *Da ist das Problem, suche die Lösung. Du kannst sie durch reines Denken finden; denn in der Mathematik gibt es kein Ignorabimus!*"

[10] "Für den Mathematiker gibt es kein Ignorabimus, und meiner Meinung nach auch für die Naturwissenschaft überhaupt nicht. [...] Der wahre Grund, warum es [niemand] nicht gelang, ein unlösbares Problem zu finden, besteht meiner Meinung nach darin, daß es ein unlösbares Problem überhaupt nicht gibt. Statt des törichten Ignorabimus heiße im Gegenteil unsere Losung: Wir müssen wissen, Wir werden wissen."

[11] "Wenn es sich darum handelt, die Grundlagen einer Wissenschaft zu untersuchen, so hat man ein System von Axiomen aufzustellen, welche eine genaue und vollständige Beschreibung derjenigen Beziehungen enthalten, die zwischen den elementaren Begriffen jener Wissenschaft stattfinden. Die aufgestellten Axiome sind zugleich die Definitionen jener elementaren Begriffe, und jede Aussage innerhalb des Bereiches der Wissenschaft, deren Grundlage wir prüfen, gilt uns nur dann als richtig, falls sie sich mittels einer endlichen Anzahl logischer Schlüsse aus den aufgestellten Axiomen ableiten läßt."

that the structure under consideration was maximal with respect to the remaining axioms.[12]

The demand discussed here would imply that a system of axioms complete in this sense is possible only for sufficiently advanced theories. On the other hand Hilbert called for complete systems of axioms also for theories being developed. In "Mathematische Probleme" (1901, p. 295) he wrote:

> [...] wherever mathematical concepts emerge from epistemological considerations or from geometry or from theories of science, mathematics acquires the task of investigating the principles lying at the basis of these concepts and defining [...] these through a simple and complete system of axioms.[13]

One should also add here that Hilbert admitted the possibility that a mathematical problem may have a negative solution, i.e., that one can show the impossibility of a positive solution on the basis of a considered axiom system. In "Mathematische Probleme" (1901, p. 297) he wrote:

> Occasionally it happens that we seek the solution under insufficient hypotheses or in an incorrect sense, and for this reason we do not succeed. The problem then arises: to show the impossibility of the solution under the given hypotheses, or in the sense contemplated [...] and we perceive that old and difficult problems [...] have finally found fully satisfactory and rigorous solutions, although in another sense than that originally intended. It is probably *this* important fact along with other philosophical reasons that give rise to the conviction [...] that every definite mathematical problem must necessarily be susceptible of an exact settlement, either in the form of an actual answer to the question asked, or by a proof of the impossibility of its solution and therewith the necessary failure of all attempts.[14]

In Hilbert's lectures from 1917–1918 (cf. Hilbert 1917–1918) one finds completeness in the sense of maximal consistency, i.e., a system is complete if and only if for any non-derivable sentence, if it is added to the system then the system becomes inconsistent.[15]

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[12] Note that this axiom was not even properly a second-order axiom.

[13] "[...] wo immer von erkenntnistheoretischer Seite oder in der Geometrie oder aus den Theorien der Naturwissenschaft mathematische Begriffe auftauchen, erwächst der Mathematik die Aufgabe, die diesen Begriffen zugrunde liegenden Prinzipien zu erforschen und dieselben durch einfaches und vollständiges System von Axiomen [...] festzulegen."

[14] "Mitunter kommt es vor, daß wir die Beantwortung unter ungenügenden Voraussetzungen oder in unrichtigem Sinne erstreben und infolgedessen nicht zum Ziele gelangen. Es entsteht dann die Aufgabe, die Unmöglichkeit der Lösung des Problems unter den gegebenen Voraussetzungen und in dem verlangten Sinne nachzuweisen. [...] alte schwierige Probleme [...] eine völlig befriedigende und strenge Lösung gefunden haben. Diese merkwürdige Tatsache neben anderen philosophischen Gründen ist es wohl, welche in uns eine Überzeugung entstehen lässt [...] daß ein jedes bestimmte mathematische Problem einer strengen Erledigung notwendig fähig sein müsse, sei es, daß es gelingt die Beantwortung der gestellten Frage zu geben, sei es, daß die Unmöglichkeit seiner Lösung and damit die Notwendigkeit des Mißlingens aller Versuche dargetan wird."

[15] Hilbert wrote in (1917–1918, p. 152): "Let us now turn to the question of *completeness*. We want to call the system of axioms under consideration complete if we always obtain an inconsistent system of axioms by adding a formula which is so far not derivable to the system of basic formulas." ("Wenden wir uns nun zu der Frage der *Vollständigkeit*. Wir wollen das vorgelegte Axiomensys-

In his lecture at the International Congress of Mathematicians in Bologna in 1928 Hilbert stated two problems of completeness, one for the first-order predicate calculus (completeness with respect to validity in all interpretations, hence the semantic completeness) and the second for a system of elementary number theory (formal completeness, in the sense of maximal consistency, i.e., Post-completeness, hence the syntactical completeness) (cf. Hilbert 1930a).

The emphasis on the finitary and syntactical methods together with the demand of (and belief in) the completeness of formal systems seems to be by Hilbert the source and reason of the fact that, as Gödel put it (cf. Wang 1974, p. 9), "[...] formalists considered formal demonstrability to be an *analysis* of the concept of mathematical truth and, therefore were of course not in a position to *distinguish* the two". Indeed, the informal concept of truth was not commonly accepted as a definite mathematical notion at that time.[16] Gödel wrote in a crossed-out passage of a draft of his reply to a letter of the student Yossef Balas: "[...] a concept of objective mathematical truth as opposed to demonstrability was viewed with greatest suspicion and widely rejected as meaningless" (cf. Wang 1987, 84–85). It is worth comparing this with a remark of R. Carnap. He writes in his diary that when he invited A. Tarski to speak on the concept of truth at the September 1935 International Congress for Scientific Philosophy, "Tarski was very sceptical. He thought that most philosophers, even those working in modern logic, would be not only indifferent, but hostile to the explication of the concept of truth". And indeed at the Congress "[...] there was vehement opposition even on the side of our philosophical friends" (cf. Carnap 1963, pp. 61–62).

All these explain in some sense why Hilbert preferred to deal in his metamathematics solely with the forms of the formulas, using only finitary reasoning which were considered to be save – contrary to semantical reasonings which were non-finitary and consequently not save. Non-finitary reasonings in mathematics were considered to be meaningful only to the extent to which they could be interpreted or justified in terms of finitary metamathematics.[17]

On the other hand there was no clear distinction between syntax and semantics at that time. Recall for example that, as indicated earlier, the axiom systems came by Hilbert often with a built-in interpretation. Add also that the very notions necessary to formulate properly the difference syntax–semantics were not available to Hilbert.

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tem vollständig nennen, falls durch die Hinzufügung einer bisher nicht ableitbaren Formel zu dem System der Grundformeln stets ein widerspruchsvolles Axiomensystem entsteht.")

[16]Note that there was at that time no precise definition of truth – this was given in (1933a) by A. Tarski.

[17]Cf. Gödel's letter to Hao Wang dated 7th December 1967 – see Wang (1974, p. 8).

**3.** The problem of the completeness of the first-order logic, i.e., the fourth problem of Hilbert's Bologna lecture, was also posed as a question in the book by Hilbert and Ackermann *Grundzüge der theoretischen Logik* (1928). It was solved by Kurt Gödel in his doctoral dissertation (1929; cf. also 1930b) where he showed that the first-order logic is complete, i.e., every true statement can be derived from the axioms. Moreover he proved that, in the first-order logic, every consistent axiom system has a model. More exactly Gödel wrote that by completeness he meant that "every valid formula expressible in the restricted functional calculus [...] can be derived from the axioms by means of a finite sequence of formal inferences" (1929, p. 61). And added that this is equivalent to the assertion that "Every consistent axiom system [formalized within that restricted calculus] [...] has a realization" (1929, p. 61) and to the statement that "Every logical expression is either satisfiable or refutable" (1929, p. 61) (this is the form in which he actually proved the result). The importance of this result is, according to Gödel, that it justifies the "usual method of proving consistency" (1929, p. 61).

In fact the completeness theorem shows in a sense an equivalence of truth and demonstrability, an equivalence of semantical and syntactical approach. It shows that the logical methods admitted by the notion of derivability are appropriate and sufficient. One should notice here that the notion of truth in a structure, central to the very definition of satisfiability or validity, was nowhere analyzed in either Gödel's dissertation or his published revision of it. There was in fact a long tradition of use of the informal notion of satisfiability (compare the work of Löwenheim, Skolem and others).[18]

Some months later, in 1930, Gödel solved three other problems posed by Hilbert in Bologna by showing that arithmetic of natural numbers and all richer the-

[18] In Gödel's doctoral dissertation (1929, p. 69) one finds the following explanation concerning the considered problem: "Let $S$ be a system of functions $f_1, f_2, \ldots, f_k$ (all defined in the same universal domain), and of individuals (belonging to the same domain), $a_1, a_2, \ldots, a_l$, as well as propositional constants, $A_1, A_2, \ldots, A_m$. We say that this system, namely

$$S = (f_1, f_2, \ldots, f_k; a_1, a_2, \ldots, a_l; A_1, A_2, \ldots, A_m),$$

*satisfies* the logical expression if it yields a proposition that is true (in the domain in question) when it is substituted in the expression. From this we see at once what we must understand by *satisfiable in a certain domain*, by *satisfiable* alone (there is a domain in which the expression is satisfiable), by *valid in a certain domain* (the negation is not satisfiable), and by *valid* alone." (Wir sagen von einem System (sämtlich in demselben Denkbereich definierter) Funktionen, $f_1, f_2, \ldots, f_k$, und (ebenfalls demselben Denkbereich angehörenden) Individuen, $a_1, a_2, \ldots, a_l$, sowie Aussagen, $A_1, A_2, \ldots, A_m$ – von diesem System

$$S = (f_1, f_2, \ldots, f_k; a_1, a_2, \ldots, a_l; A_1, A_2, \ldots, A_m)$$

sagen wir, daß es den logischen Ausdruck *erfülle*, wenn es in denselben eingesetzt einen (in dem betreffenden Denkbereich) wahren Satz ergibt. Daraus erfolgt sich ohneweiteres, was unter *erfüllbar in einem bestimmten Denkbereich, erfüllbar* schlechthin (= es gibt einen Denkbereich, in dem der Ausdruck erfüllbar ist), allgemein giltig in einem bestimmten Denkbereich (= Negaton nicht erfüllbar), *allgemein giltig* schlechthin verstanden werden soll.)

ories are essentially incomplete (provided they are consistent) (cf. Gödel 1931a). It is interesting to see how Gödel arrived at this result. Hao Wang, on the basis of his discussions with Gödel, reports this in the following way (see 1981, p. 654):

> [Gödel] represented real numbers by formulas [...] of number theory and found he had to use the concept of truth for sentences in number theory in order to verify the comprehension axiom for analysis. He quickly ran into the paradoxes (in particular, the Liar and Richard's) connected with truth and definability. He realized that truth in number theory cannot be defined in number theory and therefore his plan [...] did not work.

Gödel himself wrote on his discovery in a draft reply to letter dated 27th May 1970 from Yossef Balas, then a student at the University of Northern Iowa (cf. Wang 1987, pp. 84–85). Gödel indicated there that it was precisely his recognition of the contrast between the formal definability of provability and the formal undefinability of truth that led him to his discovery of incompleteness. One finds also there the following statement (cf. Wang 1987, p. 84):

> [...] long before, I had found the *correct* solution of the semantic paradoxes in the fact that truth in a language cannot be defined in itself.

On the base of this quotation it is sometimes argued that Gödel obtained the result on the undefinability of truth independently of A. Tarski (cf. Tarski 1933a; German translation – 1936, English translation – 1956). One should add that Tarski proving the undefinability of truth had the precise definition of this concept whereas Gödel used only an intuitive (and consequently imprecise) notion of truth. Hence there arises a problem: in which sense one can say that Gödel obtained the result of the undefinability of truth.[19]

Note also that Gödel was convinced of the objectivity of the concept of mathematical truth. In a latter to Hao Wang (cf. Wang 1974, p. 9) he wrote:

> I may add that my objectivist conception of mathematics and metamathematics in general, and of transfinite reasoning in particular, was fundamental also to my other work in logic. How indeed could one think of *expressing* metamathematics *in* the mathematical systems themselves, if the latter are considered to consist of meaningless symbols which acquire some substitute of meaning only *through* metamathematics [...] it should be noted that the heuristic principle of my construction of undecidable number theoretical propositions in the formal systems of mathematics is the highly transfinite concept of 'objective mathematical truth' as *opposed* to that of 'demonstrability' (cf. M. Davis, *The Undecidable*, New York 1965, p. 64 where I explain the heuristic argument by which I arrive at the incompleteness results), with which it was generally confused before my own and Tarski's work.

In this situation one should ask why Gödel did not mention the undefinability of truth in his writings. In fact, Gödel even avoided the terms 'true' and 'truth' as well as the very concept of being true (he used the term 'richtige Formel' and not the term 'wahre Formel'). In the paper "Über formal unentscheidbare Sätze ..."

[19] For the problem of the priority in proving the undefinability of the concept of truth, see Woleński (1991) and Murawski (1998a).

(1931) the concept of a true formula occurs only at the end of Section 1 where Gödel explains the main idea of the proof of the first incompleteness theorem (but again the term 'inhaltlich richtige Formel' and not the term 'wahre Formel' appears here). Indeed, talking about the construction of a formula which should express its own unprovability invokes the interpretation of the formal system. At the very end of the introductory section one finds the following remarks (see Gödel 1931a, 175–176; English translation: Gödel 1986, p. 151):

> The method of proof just explained can clearly be applied to any formal system that, first, when interpreted as representing a system of notions and propositions, has at its disposal sufficient means of expression to define the notions occurring in the argument above (in particular, the notion 'provable formula') and in which, second, every provable formula is true in the interpretation considered. The purpose of carrying out the above proof with full precision in what follows is, among other things, to replace the second of the assumptions just mentioned by a purely formal and much weaker one.[20]

[Add that the "purely formal and much weaker" assumption mentioned by Gödel was the assumption of the $\omega$-consistency, i.e., the assumption that for any formula $\varphi(x)$ with one free variable, if in the considered theory the sentences

$$\varphi(0), \varphi(1), \varphi(2), \ldots, \varphi(n), \ldots \quad (n \in \mathbf{N})$$

are provable then the formula $\exists x \neg\varphi(x)$ is not provable in it.]

On the other hand the term 'truth' occurred in Gödel's lectures on the incompleteness theorems at the Institute for Advanced Study in Princeton in the spring of 1934.[21] He discussed there, among other things, the relation between the existence of undecidable propositions and the possibility of defining the concept 'true (false) sentence' of a given language in the language itself. Considering the relation of his arguments to the paradoxes, in particular to the paradox of "The Liar", Gödel indicates that the paradox disappears when one notes that the notion 'false statement in a language $B$' cannot be expressed in $B$. Even more, the paradox can be considered as a proof that 'false statement in $B$' cannot be expressed in $B$. In the footnote 25 (added to the version published in Davis 1965, p. 64) Gödel wrote:

> For a closer examination of this fact see A. Tarski's papers published in: *Trav. Soc. Sci. Lettr. de Varsovie*, Cl. III, No. 34, 1933 (Polish) (translated in: *Logic, Semantics, Metamathematics. Papers from 1923 to 1938 by A. Tarski*, see in particular p. 247 ff.) and in Philosophy and Phenom. Res. 4 (1944), p. 341–376. In these two papers the concept of truth relating

[20] "Die eben auseinandergesetzte Beweismethode läßt sich offenbar auf jedes formale System anwenden, das erstens inhaltlich gedeutet über genügend Ausdrucksmittel verfügt, um die in der obigen Überlegung vorkommenden Begriffe (insbesondere den Begriff 'beweisbare Formel') zu definieren, und in dem zweitens jede beweisbare Formel auch inhaltlich richtig ist. Die nun folgende exakte Durchführung des obigen Beweises wird unter anderem die Aufgabe haben, die zweite der eben angeführten Voraussetzungen durch eine rein formale und weit schwächere zu ersetzen."

[21] Notes of Gödel's lectures taken by S.C. Kleene and J.B. Rosser were published in Davis's book in 1965 (cf. Gödel 1934).

to sentences of a language is discussed systematically. See also: R. Carnap, *Mon. Hefte f. Math. u. Phys.* 4 (1934), p. 263.

The reasons for the incompleteness results were also explicitly mentioned in Gödel's reply to a letter of A.W. Burks. This reply is quoted in von Neumann's *Theory of Self-Reproducing Automata*, 1966, pp. 55–56. Gödel wrote:

> I think the theorem of mine which von Neumann refers to is not that on the existence of undecidable propositions or that on the length of proofs but rather the fact that a complete epistemological description of a language A cannot be given in the same language A, because the concept of truth of sentences of A cannot be defined in A. It is this theorem which is the true reason for the existence of the undecidable propositions in the formal systems containing arithmetic. I did not, however, formulate it explicitly in my paper of 1931 but only in my Priceton lectures of 1934. The same theorem was proved by Tarski in his paper on the concept of truth published in 1933 in *Act. Soc. Sci. Lit. Vars.*, translated on pp. 152–278 of *Logic, Semantics and Metamathematics*.

What were the reasons of avoiding the concept of truth by Gödel? An answer can be found in a crossed-out passage of a draft of Gödel's reply to a letter of the student Yossef Balas (mentioned already above). Gödel wrote there (cf. Wang 1987, p. 85):

> However in consequence of the philosophical prejudices of our times 1. nobody was looking for a relative consistency proof because [it] was considered axiomatic that a consistency proof must be finitary in order to make sense, 2. a concept of objective mathematical truth as opposed to demonstrability was viewed with greatest suspicion and widely rejected as meaningless.

Hence it leads us to the conclusion formulated by S. Feferman in (1984, p. 112) in the following way:

> [. . . ] Gödel feared that work assuming such a concept [i.e., the concept of mathematical truth – R.M.] would be rejected by the foundational establishment, dominated as it was by Hilbert's ideas. Thus he sought to extract results from it which would make perfectly good sense even to those who eschewed all non-finitary methods in mathematics.

Though he tried to avoid concepts not accepted by the foundational establishment, Gödel's own philosophy of mathematics was in fact Platonist. He was convinced that (cf. Wang 1996, p. 83):

> It was the anti-Platonic prejudice which prevented people from getting my results. This fact is a clear proof that the prejudice is a mistake.

Note that A. Tarski was free of such limitations. In fact in the Lvov-Warsaw School no restrictive initial preconditions were assumed before the proper investigation could start. The main demands were clarity, anti-speculativeness and scepticism towards many fundamental problems of traditional philosophy. The principal method that should be used was logical analysis. The Lvov-Warsaw School was not so radical in its criticism of metaphysics as the Vienna Circe (see, for example, Woleński 1989 and 1995a).

Tarski pointed out on many occasions that mathematical and logical research should not be restricted by any general philosophical views. In particular he wrote in (1930a, p. 363):

> In conclusion it should be noted that no particular philosophical standpoint regarding the foundations of mathematics is presupposed in the present work.

And in (1954, p. 17) he wrote:

> As an essential contribution of the Polish school to the development of metamathematics one can regard the fact that from the very beginning it admitted into metamathematical research all fruitful methods, whether finitary or not.

Hence Tarski, though indicating his sympathies with nominalism, freely used in his logical and mathematical studies the abstract and general notions that a nominalist seeks to avoid.

It is known that Tarski showed not only the undefinability but – and this is his main merit here – he gave the precise inductive definition of satisfiability and truth. In connection with this one should ask whether Gödel saw the necessity to give an analysis of the concept of truth (note that in his doctoral dissertation *Über die Vollständigkeit des Logikkalküls* (1929) and in his paper "Die Vollständigkeit der Axiome des logischen Funktionenkalküls" (1930) on the completeness of the first-order predicate calculus the notion of the validity was understood in an informal way what was in fact a long tradition – cf. Löwenheim and Skolem). The answer is affirmative. Indeed, in a letter to R. Carnap of 11th September 1932 he wrote (quotation after Köhler 1991, p. 137):

> On the basis of this idea I will give in the second part of my work a definition of [the concept] 'true' and I am of the opinion that one cannot do it in another way and that the higher calculus of induction cannot be grasped semantically [i.e., at that time – syntactically].[22]

Köhler explains in (1991) that "II. Teil meiner Arbeit" means here the joint project of Gödel together with A. Heyting to write a survey of the current investigations in mathematical logic for Springer-Verlag (Berlin). Heyting wrote his part while the part by Gödel was never written (the reasons were his problems with the health). One can assume that Gödel planned to develop there a theory of truth based on the set theory.

Gödel's theorem on the completeness of first-order logic and his discovery of the incompleteness phenomenon together with the undefinability of truth vs. definability of formal demonstrability showed that formal provability cannot be treated as an analysis of truth, that the former is in fact weaker than the latter. It was also shown in this way that Hilbert's dreams to justify classical mathematics by means of finitistic methods cannot be fully realized. Those results

[22] "Ich werde auf Grund dieses Gedankens im II. Teil meiner Arbeit eine Definition für 'wahre' geben und ich bin der Meinung, daß sich die Sache anders nicht machen läßt und daß man den höheren Induktionenkalkül nicht semantisch [d.h. damals syntaktisch!] auffassen kann."

together with Tarski's definition of truth (in the structure) and Carnap's work on the syntax of a language led also to the establishing of syntax and semantics in the 1930s.

On the other hand it should be added that Gödel shared Hilbert's "rationalistic optimism" (to use Hao Wang's term) insofar as informal proofs were concerned. In fact Gödel retained the idea of mathematics as a system of truth, which is complete in the sense that "every precisely formulated yes-or-no question in mathematics must have a clear-cut answer" (Gödel 1961, p. 379). He rejected however – in the light of his incompleteness theorem – the idea that the basis of these truths is their derivability from axioms. In his Gibbs lecture of 1951 Gödel distinguishes between the system of all true mathematical propositions from that of all demonstrable mathematical propositions, calling them, respectively, mathematics in the objective and subjective sense. He claimed also that it is objective mathematics that no axiom system can fully comprise.

**4.** Gödel's incompleteness theorems and in particular his recognition (before Tarski) of the undefinability of the concept of truth indicated a certain gap in Hilbert's program and showed in particular, roughly speaking, that (full) truth cannot be established (achieved) by provability and, generally, by syntactic means. The former can be only approximated by the latter. Hence there arose a problem: how should one extend Hilbert's finitistic point of view?

Hilbert in his lecture in Hamburg in December 1930 (cf. Hilbert 1931) proposed to admit a new rule of inference to be able to realize his program. This rule is similar to the $\omega$-rule, but it has rather informal character and a system obtain by admitting it would be semi-formal. In fact Hilbert proposed that whenever $A(z)$ is a quantifier-free formula for which it can be shown (finitarily) that $A(z)$ is a correct (richtig) numerical formula for each particular numerical instance $z$, then its universal generalization $\forall x A(x)$ may be taken as a new premise (Ausgangsformel) in all further proofs.

In Preface to the first volume of Hilbert and Bernays' monograph *Grundlagen der Mathematik* (1934) Hilbert wrote (p. V):

> [. . . ] the occasionally held opinion, that from the results of Gödel follows the non-executability of my Proof Theory, is shown to be erroneous. This result shows indeed only that for more advanced consistency proofs one must use the finite standpoint in a deeper way than is necessary for the consideration of elementary formalism.[23]

Gödel pointed in many places that new axioms are needed to settle both undecidable arithmetical and set-theoretic propositions. In the footnote $48^a$ (evi-

[23]"[. . . ] die zeitweilig aufgekommene Meinung, aus gewissen neueren Ergebnissen von Gödel folge die Undurchführbarkeit meiner Beweistheorie, als irrtümlich erwiesen ist. Jenes Ergebnis zeigt in der Tat auch nur, daß man für die weitergehenden Widerspruchsfreiheitsbeweise den finiten Standpunkt in einer schärferen Weise ausnutzen muß, als dieses bei der Betrachtung der elementaren Formalismen erforderlich ist."

dently an afterthought) to (1931a) he wrote (English translation taken from Heijenoort 1967, p. 610):

> As will be shown in Part II of this paper, the true reason for the incompleteness inherent in all formal systems of mathematics is that the formation of ever higher types can be continued into the transfinite [...] while in any formal system at most denumerably many of them are available. For it can be shown that the undecidable propositions constructed here become decidable whenever appropriate higher types are added (for example, the type $\omega$ to the system P). An analogous situation prevails for the axiom system of set theory.[24]

In (193?), handwritten notes in English, evidently for a lecture, one finds the following words of Gödel:

> [...] number-theoretic questions which are undecidable in a given formalism are always decidable by evident inferences not expressible in the given formalism. As to the evidence of these new inferences, they turn out to be exactly as evident as those of the given formalism. So the result is rather that it is not possible to formalize mathematical evidence even in the domain of number theory, but the conviction about which Hilbert speaks [i.e., the conviction of the solvability of every well formulated mathematical problem – R.M.] remains entirely untouched. Another way of putting the result is this: It is not possible to mechanize mathematical reasoning, i.e., it will never be possible to replace the mathematician by a machine, even if you confine yourself to number-theoretic problems. (p. 164)

In (1931c, p. 35) he stated that "[...] there are number-theoretic problems that cannot be solved with number-theoretic, but only with analytic or, respectively, set-theoretic methods".[25] And in (1933, p. 48) he wrote: "there are arithmetic propositions which cannot be proved even by analysis but only by methods involving extremely large infinite cardinals and similar things". In (1951, p. 318) Gödel stated that:

> In order to prove the consistency of classical number theory (and *a fortiori* of all stronger systems) certain *abstract* concepts (and the directly evident axioms referring to them) must be used, where 'abstract' means concepts which do not refer to sense objects, of which symbols are a special kind. [...] Hence it follows that *there exists no rational justification of our precritical belief concerning the applicability and consistency of classical mathematics (nor even its undermost level, number theory) on the basis of a syntactical interpretation.*

In (1961) Gödel proposed "cultivating (deepening) knowledge of the abstract concepts themselves which lead to the setting up of these mechanical systems" (p. 383). In (1972) (this paper was a revised and expanded English version of

[24] "Der wahre Grund für die Unvollständigkeit, welche allen formalen Systemen der Mathematik anhaftet, liegt, wie im II. Teil dieser Abhandlung gezeigt werden wird, darin, daß die Bildung immer höcherer Typen sich ins Transfinite fortsetzen läßt (vgl. Hilbert 1926, S. 184), während in jedem formalen System höchstens abzählbar viele vorhanden sind. Man kann nämlich zeigen, daß die hier aufgestellten unentscheidbaren Sätzen durch Adjunktion passender höcherer Typen (z.B. des Types $\omega$ zum System $P$) immer entscheidbar werden. Analoges gilt auch für das Axiomensystem der Mengenlehre."

[25] "[...] es [gibt] zahlentheoretische Probleme, die sich nicht mit zahlentheoretischen sondern nur mit analytischen bzw. mengentheoretischen Hilfsmitteln lösen lassen."

(1958)) Gödel claimed that concrete finitary methods are insufficient to prove the consistency of elementary number theory and some abstract concepts must be used in addition. He wrote (pp. 271–273):

> Since finitary mathematics is defined [...] as the mathematics of *concrete intuition*, this seems to imply that *abstract concepts* are needed for the proof of consistency of number theory. [...] By abstract concepts, in this context, are meant concepts which are essentially of the second or higher level, i.e., which do not have as their content properties or relations of *concrete objects* (such as combinations of symbols), but rather of *thought structures* or *thought contents* (e.g., proofs, meaningful propositions, and so on), where in the proofs of propositions about these mental objects insights are needed which are not derived from a reflection upon the combinatorial (space-time) properties of the symbols representing them, but rather from a reflection upon the *meanings* involved.

In the paper (1946) Gödel explicitly called for an effort to use progressively more powerful transfinite theories to derive new arithmetical theorems. He wrote there:

> Let us consider, e.g., the concept of demonstrability. It is well known that, in whichever way you make it precise by means of a formalism, the contemplation of this very formalism gives rise to new axioms which are exactly as evident and justified as those with which you started, and that this process of extension can be iterated into the transfinite. So there cannot exist any formalism which would embrace all these steps; but this does not exclude that all these steps (or at least all of them which give something new for the domain of propositions in which you are interested) could be described and collected together in some non-constructive way. (p. 151)

These remarks correspond with the words of R. Carnap who wrote in (1934a, p. 274):

> [...] *all that is mathematical can be formalized; yet the whole of mathematics cannot be grasped by one system* but an infinite series of still richer and richer languages is necessary.[26]

One can compare the above remarks with those of Turing from his paper (1939). In the introduction to this paper Turing wrote (p. 162):[27]

> The well-known theorem of Gödel (1931a) shows that every system of logic is in a certain sense incomplete, but at the same time it indicates means whereby from a system $L$ of logic a more complete system $L'$ may be obtained. By repeating the process we get a sequence $L, L_1 = L', L_2 = L_1', \ldots$ each more complete than the proceeding. A logic $L_\omega$ may then be constructed in which the provable theorems are the totality of theorems provable with the help of logics $L, L_1, L_2, \ldots$ Proceeding in this way we can associate a system of logic with any constructive ordinal. It may be asked whether such a sequence of logics of this kind is complete in the sense that to any problem $A$ there corresponds an ordinal $\alpha$ such that $A$ is solvable by means of the logic $L_\alpha$.

[26] "[...] *alles Mathematische ist formalisierbar; aber die Mathematik ist nicht durch Ein System erschöpfbar*, sondern erfordert eine Reihe immer reicherer Sprachen."

[27] See also Feferman (1962; 1988) where Turing's idea and its development are discussed.

Also Zermelo proposed to allow infinitary methods to overcome restrictions revealed by Gödel. According to Zermelo the existence of undecidable propositions was a consequence of the restriction of the notion of proof to finitistic methods (he said here about “finitistic prejudice”). This situation could be changed if one used a more general “scheme” of proof. Zermelo had here in mind an infinitary logic, in which there were infinitely long sentences and rules of inference with infinitely many premises. In such a logic, he insisted, “*all* propositions are decidable!”[28] He thought of quantifiers as infinitary conjunctions or disjunctions of unrestricted cardinality and conceived of proofs not as formal deductions from given axioms but as metamathematical determinations of the truth or falsity of a proposition. Thus syntactic considerations played no rôle in his thinking.

**5.** Above the process of the development of the consciousness of the difference between provability and truth in mathematics has been analyzed. The rôle of Gödel’s incompleteness theorems in this process was stressed and the attempts to overcome the limitations disclosed by those theorems by admitting new infinitary methods (instead of finitary ones only) in the concept of a mathematical proof was indicated. To close this considerations let us note that the very distinction between provability and truth in mathematics presupposes some philosophical assumptions. In fact for pure formalists and for intuitionists there exists no truth/proof problem. For them a mathematical statement is true just in case it is provable, and proofs are syntactic or mental constructions of our own making. In the case of a platonist (realist) philosophy of mathematics the situation is different. One can say that platonist approach to mathematics enabled Gödel to state the problem and to be able to distinguish between proof and truth, between syntax and semantics.[29]

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[28]Note that that time was not yet ripe for such an infinitary logic. Systems of such a logic, though in a more restricted form than demanded by Zermelo, and without escaping incompleteness, were constructed in the mid-fifties in works of Henkin, Karp and Tarski (cf. Barwise 1980 and Moore 1980).

[29]Note that, as indicated above, Hilbert was not interested in philosophical questions and did not consider them.

# 4

# PHILOSOPHY OF MATHEMATICS IN THE 20TH CENTURY. MAIN TRENDS AND DOCTRINES[1]

The aim of the paper is to present the main trends and tendencies in the philosophy of mathematics in the 20th century. To make the analysis more clear we distinguish three periods in the development of the philosophy of mathematics in this century: (1) the first thirty years when three classical doctrines: logicism, intuitionism and formalism were formulated, (2) the period from 1931 till the end of the fifties – period of stagnation, and (3) from the beginning of the sixties till today when new tendencies putting stress on the knowing subject and the research practice of mathematicians arose.

## 1. The First 30 Years – the Rise of Classical Conceptions

Philosophy of mathematics and the foundations of mathematics entered the 20th century in the atmosphere of a crisis.[2] It was connected mainly with the problem of the status of abstract objects. In set theory created by Georg Cantor in the last quarter of the 19th century antinomies were discovered. Some of them were already known to Cantor – e.g., the antinomy of the set of all sets or the antinomy of the set of all ordinals (the latter was independently discovered by C. Burali-Forti). He was able to eliminate them by distinguishing between classes and sets. New difficulties appeared when B. Russell discovered the antinomy of irreflexive classes (called today Russell's antinomy) in the system of logic of G. Frege

[1]Originally published in: J. Malinowski and A. Pietruszczak (eds.), *Essays in Logic and Ontology*, pp. 331–347 (2006). Amsterdam–New York: Editions Rodopi. 

[2]It is usually called the second crisis and distinguished from the first one which took place in the antiquity and was connected with the discovery of incommensurables by Pythagoreans.

In: Roman Murawski, *Essays in the Philosophy and History of Logic and Mathematics* (*Poznań Studies in the Philosophy of the Sciences and the Humanities*, vol. 98), pp. 59–73. Amsterdam/New York, NY: Rodopi, 2010.

(to which the latter wanted to reduce the entire mathematics). Besides those antinomies one discovered also so called semantical ones (G.D. Berry, K. Grelling) connected with semantical notions such as reference, sense and truth.

In this situation there was a need to find a new solid and safe foundations for mathematics. Attempts to provide such a foundations and to overcome the crisis led, among others, to the rise of new directions and conceptions in the philosophy of mathematics, namely of logicism, intuitionism and formalism. One used here of course achievements of earlier mathematicians, especially of those living in the 19th century. In particular important were the very rise of set theory (G. Cantor), the idea of the arithmetization of analysis (A. Cauchy, K. Weierstrass, R. Dedekind), axiomatization of the arithmetic of natural numbers (G. Peano), non-Euclidean geometries (N.I. Lobachevsky, J. Bolayi and C.F. Gausss) and the complete axiomatization of systems of geometry (M. Pasch, D. Hilbert) and, last but not least, the rise and the development of mathematical logic (G. Boole, A. De Morgan, G. Frege, B. Russell).

New doctrines in the philosophy of mathematics looking for solid, sure and safe foundations of mathematics saw them first of all in mathematical logic and set theory. The most important proposals are called logicism, intuitionism and formalism.

### 1.1. *Logicism*

Logicism was founded by Gottlob Frege (1848–1925) and developed by Bertrand Russell (1872–1970) and Alfred North Whitehead (1861–1947). It grew out from the tendency to arithmetize the analysis – this tendency was characteristic for foundational studies in the 19th century. The aim of it was to show that the theory of real numbers underlying the analysis can be developed on the basis of the arithmetic of natural numbers. The realization of this task led to a new one, namely to find foundations for the arithmetic of natural numbers, i.e., to found it on a simpler, more elementary theory. This task was undertaken just by G. Frege who tried to reduce arithmetic to logic (cf. Frege 1884, 1893, 1903). Unfortunately the system of logic used by him turned out to be inconsistent what was discovered by B. Russell in 1901 (Russell discovered that one can construct in this system the antinomy of irreflexive classes called today Russell's antinomy). In this situation Russell together with A.N. Whitehead undertook anew the task of reducing arithmetic to a simpler theory. They introduced so called ramified theory of types and showed that the whole mathematics can be reduced to it. This was the beginning of a mature form of logicism. Its main thesis says that (the whole) mathematics is reducible to logic, i.e., mathematics is only a part of logic and logic is an epistemic ground of all mathematics. This thesis can be formulated as the conjunction of the following three theses: (1) all mathematical concepts (in particular all primitive notions of mathematical theories) can be explicitly defined by purely logical notions, (2) all mathematical theorems can be deduced (by logical deduction) from logical axioms and definitions, (3) this deduction is based on a logic common for all mathematical theories, i.e., the justification of theorems in all mathematical theories refers to the same basic principles which form one logic common for the whole of mathematics (here is also included a thesis that any argumentation in mathematics should be formalized). Hence theorems of mathematics have uniquely determined contents and it is a logical contents. Moreover, mathematical theorems are analytic (similarly as logical theorems).

Russell and Whitehead showed in their monumental work *Principia Mathematica* (vol. I – 1910, vol. II – 1912, vol. III – 1913) how to reduce the whole of mathematics to the system of logic given by the ramified theory of types. Some difficulties by the attempts to prove certain properties of natural numbers forced the necessity of introducing principles of a non-logical character, in particular the axiom of infinity, reducibility axiom and the axiom of choice (they have in fact a set-theoretical character). This led to the modern version of logicism which states that the whole of mathematics is reducible to logic and set theory.

### 1.2. *Intuitionism*

Second doctrine of the philosophy of mathematics formulated at the beginning of the 20th century was intuitionism founded by the Dutch mathematician Luitzen Egbertus Jan Brouwer (1881–1966). It used some ideas which appeared by Leopold Kronecker (1823–1891), Henri Poincaré (1854–1912) and by a group of French mathematicians called the Paris group of intuitionism or French semi-intuitionists (R.L. Baire, E. Borel, H.L. Lebesgue and the Russian mathematician N.N. Lusin).

Brouwer presented his philosophical views for the first time in his doctoral dissertation *Over de Grondslagen de Wiskunde* (1907) and developed them in the papers (Brouwer, 1912 and 1949). The main aim of him (and of the intuitionism) was to avoid inconsistencies in mathematics. Brouwer proposed means to do that which turned out to be very radical and led in the effect to the deep reconstruction of the whole of mathematics.

The main fundamental thesis of Brouwer's intuitionism is the rejection of platonism in the philosophy of mathematics, i.e., of the thesis about the existence of mathematical objects which is independent of time, space and human mind. The proper ontological thesis is the conceptualism. According to Brouwer mathematics is a function of the human intellect and a free activity of the human mind, it is a creation of the mind and not a theory or a system of rules and theorems. Mathematical objects are mental constructions of an (idealized) mathematician. Mathematics is based on the fundamental intuition of the a priori time (one can see here the connection with the philosophy of I. Kant). This intuition is a basis for the mental construction of natural numbers. Moreover this construction is the basic of mathematical activity from which all other mathematical activity springs. One of the consequences of this is the thesis that arithmetical statements are synthetic a priori judgements.

One should reject the axiomatic-deductive method as a method of developing and founding mathematics. It is not sufficient to postulate only the existence of mathematical objects (as it is done in the axiomatic method) but one must first construct them. One must also reject the actual infinity. An infinite set can be understood only as a law or a rule of forming more and more of its elements, but they will never exist as forming an actual totality. Hence there are no uncountable sets and no cardinal numbers other than $\aleph_0$.

The conceptualistic thesis of intuitionism implies also the rejection of any nonconstructive proofs of the existential theses, i.e., of proofs giving no constructions of the postulated objects. In fact in intuitionism 'to be' equals 'to be constructed'. Brouwer claimed that just nonconstructive proofs were the source of all troubles and mistakes in mathematics. Since such proofs are usually based on the law of the excluded middle ($p \vee \neg p$) as well as on the law of double negation ($p \equiv \neg\neg p$), intuitionists could not any longer use the classical logic. Moreover they claimed that logic is neither a basis for mathematics nor a starting point of it. Brouwer said that logic is based on mathematics, that it is secondary and dependent on mathematics and not vice versa, i.e., logic is a basis for mathematics as the logicists assert.

A mathematical theorem is a declaration that a certain mental construction has been completed. All mathematical constructions are independent of any language (the only role of a language is to enable the communication of mathematical constructions to others). Hence there is no language (formal or informal) which would be safe for mathematics and would protect it from inconsistencies. It is a mistake to analyze the language of mathematics instead of analyzing the mathematical thinking.

The ideas of Brouwer were popularized by his student Arend Heyting (1898–1980). He attempted to explain them in a language usually used in the reflection on mathematics – note that Brouwer expressed his ideas in a language far from the standards accepted by mathematicians and logicians and therefore not always understandable. It seems that without those attempts of Heyting the ideas of Brouwer would soon disappear. Heyting constructed also the first formalized system of the intuitionistic propositional calculus, i.e., of the propositional calculus satisfying principles of the intuitionism. This system has been never accepted by Brouwer.

### 1.3. *Formalism*

The third classical doctrine in the philosophy of mathematics of the 20th century is formalism created by David Hilbert (1862–1943). Hilbert was of the opinion that the attempts to justify and found mathematics undertaken hitherto, especially by the intuitionism, were unsatisfactory because they led to the restriction of mathematics and to the rejection of various parts of it, in particular those considering infinity (cf. Hilbert 1926). He undertook an attempt to justify the actual infinity by finitistic methods referring only to concrete, finite objects being the starting point of mathematics. Those objects are natural numbers meant as numerals (systems of signs). They are given immediately and clearly. Actual infinity on the other hand is only an idea of reason in Kant's sense, hence a notion for which there is no real basis because it transcends every experience. Hilbert proposed to justify the mathematics using the notion of an actual infinity (in Hilbert's terminology – infinitistic mathematics) by finitistic methods. He wanted to do it via proof theory (*Beweistheorie*). To do this one should formalize the whole mathematics, i.e., to represent it as a formalized system (or a set of formalized systems) and then to study such systems as systems of symbols only (hence as systems of concrete and visible objects clearly and immediately given) governed by certain rules referring only to the form and not to the sense or meaning of formulas. This can be done by finitistic and consequently safe methods. In particular one should show in this way the consistency and conservativeness of infinitistic mathematics. This was the task of proof theory.

One should note here that formalization was for Hilbert only an instrument used to prove the correctness of (infinitistic) mathematics. Hilbert did not treat mathematical theories as games on symbols or collections of formulas without any contents. Formalization was only a methodological tool in the process of studying the properties of the preexisting mathematical theories.

## 2. Between 1931 and the End of the Fifties

In 1931 a paper by a young Austrian mathematician Kurt Gödel (1906–1978) was published (cf. Gödel 1931a). It turned out to be revolutionary and played a crucial rôle in the foundations and philosophy of mathematics. Gödel proved in it that arithmetic of natural numbers and all formal systems containing it are essentially incomplete provided they are consistent (and based on a recursive, i.e., effectively recognizable set of axioms). He announced also a theorem stating that (roughly speaking) no such theory can prove its own consistency, i.e., to prove the consistency of a given theory $T$ containing arithmetic one needs methods and assumptions stronger than those of the theory $T$. Hence in particular one cannot prove the consistency of an infinitistic theory by finitistic methods.

In this way Gödel's incompleteness theorems indicated certain epistemological limitation of axiomatic method. In particular they showed that one cannot completely axiomatize neither the arithmetic of natural numbers nor other richer theories (what was required by Hilbert's program) and that there are no absolute inconsistency proofs for mathematical theories.[3]

Since one of the main aims of doctrines in the philosophy of mathematics formulated at the beginning of the century was to show that mathematics is consistent and that it is free of antinomies, one sees that Gödel's results were in fact a shock for them.[4] Indeed after 1931 one can observe certain stagnation in the philosophy of mathematics which lasted till the end of the fifties. There were formulated new conceptions but they were not so significant as logicism, intuitionism and formalism. One should mention here first of all works by Willard Van Orman Quine (1908–2000), Ludwig Wittgenstein (1889–1951) and Kurt Gödel.

### 2.1. *Quine's Philosophy of Mathematics*

Quine's philosophy of mathematics can be characterized as holistic. According to it mathematics should be considered not in separation from other sciences but as an element of the collection of theories explaining the reality (cf. Quine 1948–1949; 1951a; 1951b). Mathematics is indispensable there, in particular in physical theories, hence its objects do exist.[5] In this way Quine attacked the anti-realist and anti-empiricist approaches to the philosophy of mathematics. This cleared the way for empiricist approaches (see below). Quine accepted only one way of existence. One does not find by him physical, mathematical, intensional, conceptual etc. existence but simply existence. He rejected also the possibility of dividing a scientific theory into an analytical (hence purely conventional) part and a synthetic one (dealing with the reality).

### 2.2. *Wittgenstein's Philosophy of Mathematics*

Wittgenstein's ideas concerning mathematics can be reconstructed from his remarks made at various periods – hence they are not uniform, moreover they are even inconsistent (cf. Wittgenstein 1953; 1956). They grew out from his philosophy of language as a game. He was against logicism, and especially against Russell's attempts to reduce mathematics to logic. He claimed that by such reductions the creative character of a mathematical proof disappears. A mathematical proof cannot be reduced to axioms and rules of inference, because it is in

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[3]For Gödel's theorems and the discussion of their philosophical and methodological consequences see, e.g., Murawski (1999a).

[4]One can find opinions that Gödel's theorems have definitely deprived mathematics of certainty, absolute truth and necessity (cf. Kline 1980).

[5]This argumentation is known in the literature as Quine's-Putnam indispensability argument.

fact a rule of constructing a new concept. Logic does not play so fundamental role in mathematics as logicism claims – its role is rather auxiliary. Mathematical knowledge is independent and specific in comparison with logic. Mathematical truths are a priori, synthetic and constructive. Mathematicians are not discovering mathematical objects and their properties but creating them. Hence mathematical knowledge is of a necessary character. One can easily see here the connections of Wittgenstein's philosophy of mathematics and Kant's ideas as well as the ideas of intuitionists.

### 2.3. *Gödel's Philosophy of Mathematics*

Gödel formulated his philosophical ideas concerning mathematics especially in connection with some problems of set theory (cf. Gödel 1944; 1947). His philosophy of mathematics can be characterized as platonism. He claimed that mathematical objects exist in the reality independently of time, space and the knowing subject. He stressed the analogy between logic and mathematics on the one hand and natural sciences on the other. Mathematical objects are transcendental with respect to their representation in mathematical theories. The basic source of mathematical knowledge is intuition[6] though it should not be understood as giving us the immediate knowledge. It suffices to explain and justify simple basic concepts and axioms. Mathematical knowledge is not the result of a passive contemplation of data given by intuition but a result of the activity of the mind which has a dynamic and cumulative character. Data provided by the intuition can be developed by a deeper study of mathematical objects and this can lead to the adoption of new statements as axioms. Those new axioms can be justified from outside, i.e., by their consequences (whether they enable us to solve problems which are unsolvable so far, whether they make possible to simplify the proofs or to obtain new interesting corollaries). Gödel admits here consequences both in mathematics itself as well as in physics. This is, according to him, the second (besides intuition) criterion of the truth of mathematical statements.

As stated above, Gödel presented and explained his philosophical ideas mainly in connection with set theory, in particular with the problem of continuum. As a realist he was convinced that the continuum hypothesis has a determined logical value, i.e., it is true or false (though we are not able now to decide which one). So Gödel assumed that there exists an absolute universe of sets which we try to describe by axioms of set theory. The fact that we can neither prove nor disprove the continuum hypothesis indicates that the axioms does not describe this universe in a complete way. Hence the necessity of adopting new axioms in mathematics, in particular in set theory. Gödel suggested that new infinity ax-

[6] Gödel never explained what he meant by intuition. Hence various interpretations are possible. Since 1959 he was interested in Husserl's phenomenology. This probably influenced his theory of mathematical intuition.

ioms postulating the existence of new large cardinals should be considered. He claimed that such axioms can imply not only some corollaries concerning the continuum hypothesis but also some new results in arithmetic of natural numbers.

### 2.4. *The Influence of the Philosophy of Mathematics on the Foundations of Mathematics*

Above we indicated the significance and the influence of mathematical logic on the rise and development of doctrines in the philosophy of mathematics, in particular in the case of the classical conceptions, i.e., logicism, intuitionism and formalism. But also another dependence should be noted, namely the influence of philosophical doctrines and conceptions on foundational studies. This was the case especially after 1931.

Under the influence of the logicism and platonism Alfred Tarski (1901–1983) founded and developed set-theoretical semantics. It gave rise to the very important part of the foundations of mathematics, namely to model theory. Hilbert's formalism stimulated researches in the proof theory, hence in the finitistic metamathematics. Though Gödel's results indicated that Hilbert's program of justification of mathematics cannot be realized in the original form but soon appeared new results and ideas which showed that this can be partially done. Proof theory became an independent discipline in the foundations of mathematics. Under the influence of intuitionism various systems of constructive mathematics as well as various constructivistic trends were developed.

Constructivism is a common name for various doctrines the main thesis of which is the demand to restrict mathematics to the consideration of constructive objects and to constructive methods only. Hence constructivism is a normative attitude the aim of which is not to build appropriate foundations for and to justify the existing mathematics but rather to reconstruct the latter according to the accepted principles and to reject all the methods and results which do not fulfil them. The constructivistic tendencies appeared in the last quarter of the nineteenth century as a reaction against the intensive development of highly abstract methods and concepts in mathematics inspired by Cantor's set theory.

There are various constructivist programs and schools. They differ by their interpretation and understanding of the concept of constructivity. One of the most developed schools is intuitionism discussed above (see Section 1.2). Others are finitism, ultraintuitionism (called also ultrafinitism or actualism), predicativism, classical and constructive recursive mathematics, Bishop's constructivism.

One should also note that intensive studies on intuitionistic mathematics and logic took place. Recently those researches became strong impulse from the computer science since it turned out that intuitionistic logic provides a very useful tool in theoretical informatics.

## 3. Philosophy of Mathematics after 1960

In the early sixties certain renaissance of interests in the philosophy of mathematics can be observed. The old doctrines: logicism, intuitionism and formalism, still prevail but there appear some new conceptions based on another presuppositions and having another aims.

Formalism and intuitionism did not abandon in principle the ideas of their founders though the development of mathematical logic and the foundations of mathematics influenced them and forced some modifications of the original theses.

As indicated above, Gödel's incompleteness results struck Hilbert's formalistic program and showed that it cannot be fully realized. In this situation two ideas appeared: One suggested to extend the scope of the admissible methods and allow general constructive ones instead of only finitistic methods (the idea was explicitly formulated for the first time by Paul Bernays) – this led to the so called generalized Hilbert's program. The second idea was based on the following reasoning: If the entire infinitistic mathematics cannot be reduced to and justified by finitistic mathematics then one can ask for which part of it is that possible. In other words: how much of infinitistic mathematics can be developed within formal systems which are conservative over finitistic mathematics with respect to real sentences? This question constitutes the relativized version of the program of Hilbert. Recently results of the so called reverse mathematics developed mainly by H. Friedman and S.G. Simpson contributed very much to this program.[7] In fact they showed that several interesting and significant parts of classical mathematics are finitistically reducible. This means that Hilbert's program can be partially realized.

Under the influence of intuitionism several constructivistic doctrines were developed – we wrote about them above.

Logicism appears as so called pluralistic logicism and is represented by H. Mehlberg and H. Putnam. According to it the main task in mathematics is the construction of proofs in axiomatic systems. By the deduction theorem if a sentence $\varphi$ is a theorem of an axiomatic theory $T$ then there exist axioms $\varphi_1, \varphi_2, \ldots, \varphi_n$ of this theory such that the formula $\varphi_1 \wedge \varphi_2 \wedge \ldots \wedge \varphi_n \longrightarrow \varphi$ is a theorem of logic. Hence mathematical theories are nothing more than a source of logical laws. It is absolutely inessential what axioms one adopts, there do

[7] Detailed description of the results of the reverse mathematics and of their meaning for the philosophy of mathematics is given in Murawski (1994) (one can also find there an extensive bibliography).

not exist better and worse theories – important are only the logical interconnections between axioms and theorems. Hence for example there is (from the point of view of the pure mathematics) no difference between Euclidean or non-Euclidean geometry.[8]

Researches in mathematical logic and the foundations of mathematics indicated the crucial role played in mathematics by set theory. Therefore so much attention was devoted to it not only in foundational studies but also in the philosophical context. This attitude was strengthened by results of Gödel (1939) and Paul Cohen (1963) who showed that two most discussed principles of set theory: the axiom of choice and the continuum hypothsis are consistent and independent of other axioms of Zermelo-Fraenkel set theory. Hence various mutually inconsistent axiomatic set theories are possible and consequently there is no unique set-theoretical base for mathematics. The situation is even more complicated: Zermelo-Fraenkel axiomatization of set theory is not the only possible, there are also other axiomatic theories based on another principles – e.g., Gödel-Bernays set theory (with sets and classes), Quine's system of *New Foundations*, theory of semi-sets of P. Vopěnka and his alternative theory of sets (based on the ideas of Bolzano instead of those of Cantor). All this led anew to the discussion between formalism and realism in set theory (and consequently in mathematics).

By the formalistic approach there is no necessity of justifying the accepted axioms (one can in fact adopt any consistent axioms) – cf. Cohen (1971). The realistic one pretends that axioms provide the description of the real world of sets. The latter was represented, e.g., by Gödel (see above). In the eighties a naturalized version of Gödel's ideas has been developed by Penelope Maddy (cf. Maddy 1980; 1990a; 1990b; 1997). Gödel thought we can intuit abstract sets, Maddy claims that we can see sets of concrete objects whose members are before our eyes. We perceive sets of concrete physical objects by perceiving their elements (physical objects). Sets are located in the space-time real world. In this way we can know "simple" sets, i.e., hereditarily finite sets. More complicated sets are treated by Maddy as theoretical objects in physics – they and their properties can be known by metatheoretical considerations. One sees that in this conception Gödel's mathematical intuition has been replaced by the usual sensual perception. The advantage of Maddy's approach is that it unifies the advantages of Gödel's platonism enabling us to explain the evidence of certain mathematical facts with Quine's realism taking into account the role that mathematics plays in scientific theories.

[8]One can see certain similarity of this idea and Aristotle's thesis that the necessity cannot be found in any single statement about mathematical objects but in hypothetical statements saying that if a certain proposition is true then a certain other proposition is also true. Hence (using today's terminology) we can say that for Aristotle the necessity of mathematics was that of logically necessary hypothetical propositions. Similar ideas one finds also by Russell before 1900, later he has changed his mind.

The most characteristic feature of the development of the philosophy of mathematics in the second half of the 20th century is the anti-foundational tendency and the empiricist approach. They were the reaction to the limitations and one-sidedness of the classical views which are giving one-dimensional static picture of mathematics as a science and are trying to provide indubitable and infallible foundations for mathematics. They treat mathematics as a science in which one automatically and continuously collects true proved propositions. Hence they provide only one-sided reconstructions of the real mathematics in which neither the development of mathematics as a science nor the development of mathematical knowledge of a particular mathematician would be taken into account.

New conceptions challenge the dogma of foundations and try to reexamine the actual research practices of mathematicians and those using mathematics and to avoid the reduction of mathematics to one dimension or aspect only. They want to consider mathematics not only in the context of justification but to take into account also the context of discovery.

One of the first attempts in this direction was the conception of Imre Lakatos (1922–1974). He attempted to apply some of Popper's ideas about the methods of natural science to episodes from the history of mathematics.[9]

Lakatos claims that mathematics is not an indubitable and infallible science – on the contrary, it is fallible. It is being developed by criticizing and correcting former theories which are never free of vagueness and ambiguity. One tries to solve a problem by looking simultaneously for a proof and for a counterexample. New proofs explain old counterexamples, new counterexamples undermine old proofs. By proofs Lakatos means here usual non-formalized proofs of actual mathematics. In such a proof one uses explanations, justifications, elaborations which make the conjecture more plausible, more convincing. Lakatos does not analyze the idealized formal mathematics but the informal one actually developed by "normal" mathematicians, hence mathematics in process of growth and discovery. His main work *Proofs and Refutations* (1963–1964) is in fact a critical examination of dogmatic theories in the philosophy of mathematics, in particular of logicism and formalism. Main objection raised by Lakatos is that they are not applicable to actual mathematics. Lakatos claims that mathematics is a science in Popper's sense, that it is developed by successive criticism and improvement of theories and by establishing new and rival theories. The role of "basic statements" and "potential falsifiers" is played in the case of formalized mathematical theories by informal theories (cf. Lakatos 1967).

Another attempt to overcome the limitations of the classical theories of philosophy of mathematics is the conception of Raymond L. Wilder (1896–1982).

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[9] The reference to the history of mathematics is one of the characteristic features of new trends in the philosophy of mathematics.

He presented his ideas in many papers and lectures – complete version of them can be found in two of his books (1968; 1981). Wilder's main thesis says that mathematics is a cultural system. Mathematics can be seen as a subculture, mathematical knowledge belongs to the cultural tradition of a society, mathematical research practice has a social character. Thanks to such an approach the development of mathematics can be better understood and the general laws of changes in a given culture can be applied in historical and philosophical investigations of mathematics. It also enables us to see the interrelations and influences of various elements of the culture and to study their influence on the evolution of mathematics. It makes also possible to discover the mechanisms of the development and evolution of mathematics. Wilder's conception is therefore sometimes called evolutionary epistemology. He has proposed to study mathematics not only from the point of view of logic but also using methods of anthropology, sociology and history. Wilder maintains that mathematical concepts should be located in the Popper's "third world". Mathematics investigates no timeless and spaceless entities. It cannot be understood properly without regarding the culture in the framework of which it is being developed. In this sense mathematics shares many common features with ideology, religion and art. A difference between them is that mathematics is science in which one justifies theorems by providing logical proofs and not on the basis of, say, general acceptance.

The thesis that mathematical knowledge is a priori, absolutely certain and indubitable as well as infallible was criticized also in the quasi-empiricism of Hilary Putnam. He claimed (cf. Putnam 1975) that mathematical knowledge is not a priori, absolute and certain, that it is rather quasi-empirical, fallible and probable, much like natural sciences. He argues that quasi-empirical mathematics is logically possible and that ordinary mathematics has been quasi-empirical all along. In (1967) Putnam proposed a modal picture of mathematics according to which mathematics does not study any particular objects themselves but rather possibilities involving any objects whatsoever. Hence mathematics studies the consequences of axioms and asserts also the possibility of its axioms having models. The introduction of modalities opened the door to new epistemology of mathematics.

The new anti-foundational trends in the philosophy of mathematics described above should not be treated as competitive with respect to old theories. They should be rather seen as complements of logicism, intuitionism and formalism. One is looking here not for indubitable, unquestionable and irrefutable foundations of mathematics, one tries not to demonstrate that the actual mathematics can be reconstructed as an infallible and consistent system but one attempts to describe the actual process of building and constructing mathematics (both in individual and historical aspect).

The quasi-empirical or even empirical approach to mathematics became recently a new dimension through the broader and broader application of computers. This phenomenon puts new challenges to the philosophy of mathematics.

Computers are applied in mathematics in at least six different ways: (1) to do numerical calculations, (2) to solve (usually approximatively) algebraical or differential equations and systems of equations, to calculate integrals, etc. (3) in automated theorem proving, (4) in checking mathematical proofs, (5) in proving theorems (one speaks in such situations about proofs with the help of computers), (6) to do experiments with mathematical objects. Special interests and controversies awake applications of the type (5). One of the main problems considered in this context is the problem of the epistemological status of theorems obtained in this way and consequently – in the case of accepting such theorems as legitimated mathematical theorems – the problem of the character of mathematical knowledge.

This problem is usually discussed in the context of the four-colour theorem (cf. for example Tymoczko 1979). This theorem – proved in 1976 – is the first mathematical theorem whose proof is essentially based on the application of a computer program. We have no proof of this theorem not using computer. Moreover, some of the deciding ideas used in the proof were obtained and improved with the help of computer experiments, by a "dialogue" with the computer. Hence the four-colour theorem is the first theorem of the new type, i.e., it has (at least so far) no traditional proof and its only proof we know is based on a(n) (computer) experiment (more exactly: it uses computer computations). So one should ask whether the mathematical knowledge can be still classified as an a priori knowledge and one should choose between the following two options: (1) to extend the scope of admissible proof methods and admit also the usage of computers (hence a certain experiment) in proving theorems or (2) to decide that the four-colour theorem has not been proved so far and does not belong to the domain of mathematical knowledge. Observe that the first option implies the thesis that mathematical knowledge is not an a apriori knowledge any more but has in fact a quasi-empirical character. The thesis of the quasi-empirical character of mathematical knowledge can be found, e.g., by R. Hersh, Ph. Kitcher and E.R. Swart (cf. Swart 1980). They argue that mathematics always admitted empirical elements and had in fact an empirical character. On the other hand one attempts also to defend the a priori character of mathematics despite the usage of computers in it by arguing that proofs using computers can be transformed into traditional proofs by adding new axioms or that a computer is in fact a mathematician and it knows the result proved deductively or that procedures similar to applying computer programs have been used in mathematics for a long time, hence the applications used in the proof of the four-colour theorem are in fact nothing essentially new (cf. for example Levin 1981, Krakowski 1980 or Detlefsen and Luker 1980).

It is worth noting here that not only the usage of computers is the source of new challenges in the philosophy of mathematics but also the theoretical computer science, in particular the computation theory, raises new problems and simultaneously provides new tools to analyze known results and to give new interpretations of them. As an example can serve here the information theory and its application to the epistemology of mathematics (cf. Chaitin 1982 and 1998). In particular the analysis of Gödel's incompleteness theorems from the point of view of the information theory leads to the conclusion that mathematical knowledge has a quasi-empirical character and is in fact similar to sciences, in particular in the case of the criteria of accepting new axioms and methods.

Considering new conceptions in the philosophy of mathematics one must also mention structuralism.[10] It can be characterized as a doctrine claiming that mathematics studies structures and that mathematical objects are featureless positions in these structures. As forerunners of such views one can see R. Dedekind, D. Hilbert, P. Bernays and N. Bourbaki. The latter is in fact a pseudonym of a group of (mainly French) mathematicians who undertook in the thirties the task of a systematization of the whole of mathematics (the result of their investigations was the series of books under the common title *Éléments de mathématique*). Their work referred to Russell's idea of reconstructing mathematics as one system developed on a firm (logical) basis. For Bourbakists the mathematical world is the world of structures. The very notion of a structure was explained by them in terms of set theory. They distinguished three principal types of mathematical structures: algebraic, order and topological structures.

The idea of treating mathematics as a science about structures is being developed nowadays by Michael Resnik, Stewart Shapiro and Geoffrey Hellman. Resnik claims (cf. Resnik 1981; 1982; see also Resnik 1997) that mathematics can be viewed as a science of patterns with its objects being positions in patterns. The identity of mathematical objects is determined by their relationships to other positions in the given structure to which they belong. He does not postulate a special mental faculty used to acquire knowledge of patterns (they are not seen through a mind's eyes). We go through a series of stages during which we conceptualize our experience in successively more abstract terms. This process do not necessarily yield necessary truths or a priori knowledge. Important is here our tendency to perceive things as structured. The transition from experience to abstract structures depends upon the culture in which it takes place. Add that the transition from simple patterns to more complicated ones and the development of pure theories of patterns rely upon deductive evidence.

S. Shapiro claims (cf. Shapiro 1989; 1991) that there is a strict connection between objects of mathematics and objects of natural sciences. An explanation of it can be provided in his opinion just by structuralism according to which mathematics studies not objects *per se* but structures of objects. Hence objects of

[10]The presentation and analysis of structuralistic doctrines can be found in the doctoral dissertation by I. Bondecka-Krzykowska – cf. Bondecka-Krzykowska (2002); see also (2007).

mathematics are only “places in a structure”. The advantage of such an approach is that it enables us to explain the phenomenon of applicability of abstract mathematical theories in natural sciences as well as the interrelations between various domains of mathematics itself. It enables also a holistic approach to mathematics and science.

G. Hellman argues (cf. Hellman 1989) that one can interpret mathematics (in particular arithmetic and analysis) as nominalistic theories concerned with certain logically possible ways of structuring concrete objects. He uses by such interpretations second-order logic and modal operators (hence his approach is sometimes called modal-structural).

## 4. Conclusions

The overview of the development of the philosophy of mathematics in the 20th century given above shows on the one hand the strong influence of and interconnections between mathematical logic and the foundations of mathematics on the one hand and the philosophy of mathematics and on the other. It indicates also the dominating (especially in the second half of the century) tendency to provide not only a reconstruction of the existing mathematics and to show in this way that it is safe and consistent (what was the aim of the classical doctrines developed at the beginning of the century) but to study the real research practice of mathematicians. The latter leads to the development of quasi-empirical trends and doctrines in the philosophy of mathematics. Those tendencies – reconstruction of mathematics on a safe base on the one hand and the study of the mathematical practice on the other – should not be treated as competitive but as complementary. They attempt to show and to investigate new aspects of the phenomenon of mathematics not covered by the others.

# 5

# ON NEW TRENDS IN THE PHILOSOPHY OF MATHEMATICS[1]

The aim of this paper is to present some new trends in the philosophy of mathematics. The theory of proofs and counterexamples of I. Lakatos, the conception of mathematics as a cultural system of R.L. Wilder as well as the conception of R. Hersh and the intensional mathematics of N.D. Goodman and S. Shapiro will be discussed. All those new trends and tendencies in the philosophy of mathematics try to overcome the limitations of the classical theories (logicism, intuitionism and formalism) by taking into account the actual practices of mathematicians.

Since the beginning of the sixties a renaissance of interests in the philosophy of mathematics can be observed. It is characterized, on the one hand, by the dominance of the classical theories like logicism, intuitionism, formalism and platonism developed at the turn of the century, and, on the other, by emergence of some new conceptions. The latter are reactions to the limitations and one-sidedness of the classical views which are in fact results of certain reductionist tendencies in the philosophy of mathematics and consequently are of explicitly monistic character.

Logicism claims that the whole mathematics can be reduced to logic, or, in a modern version, to set theory. Hence mathematics is nothing more than logic, or set theory.

Intuitionism rejects the existence of an objective mathematical reality and says that mathematical knowledge can be founded on the activity of human mind and that this activity can be directly known. Hence the subject of mathematics is the mental activity of mathematicians. A. Heyting, the follower of

[1]Originally published in: E. Orłowska (ed.), *Logic at Work. Essays Dedicated to the Memory of Helena Rasiowa*, pp. 15–24 (1999). Heidelberg–New York: Physica-Verlag, A Springer-Verlag Company. 

In: Roman Murawski, *Essays in the Philosophy and History of Logic and Mathematics* (*Poznań Studies in the Philosophy of the Sciences and the Humanities*, vol. 98), pp. 75–84. Amsterdam/New York, NY: Rodopi, 2010.

L.E.J. Brouwer, the founder of intuitionism, wrote: "In fact, mathematics, from the intuitionistic point of view, is a study of certain functions of the human mind [...]." (cf. Heyting 1966, p. 10).

Formalism (at least in the radical version of H.B. Curry, cf. 1951) – Hilbert, the founder of formalism, was not so radical) reduces mathematics to certain play on meaningless symbols. Mathematics is simply a study of formal systems expressed in artificial formal languages and based on certain formal rules of inference.

Platonism,[2] founded by Plato and taking various forms in the history of mathematics, is always alive and is in fact the philosophy of most mathematicians (if they do thing of philosophy at all and express their views). Platonism claims that the subject of mathematics are certain timeless and spaceless entities being independent of mathematicians. The nature and existence of them was understood and explained in various ways. What is important here is the fact that a mathematician who constructs mathematics was not taken into account. Mathematics was an absolute knowledge about certain absolute objects.

This attitude is characteristic for all classical theories in philosophy of mathematics. They are giving us one-dimensional static pictures of mathematics as a science and are trying to construct indubitable and infallible foundations for mathematics. They treat mathematics as a science in which one automatically and continuously collects true proved propositions. The complexity of the phenomenon of mathematics is lost in this way – neither the development of mathematics as a science nor the development of mathematical knowledge of a particular mathematician are taken into account. The classical theories provide us only with one-sided reconstructions of the real mathematics. The reason for that can be seen in the fact that they were created at the turn of the century in an atmosphere of a crisis in the foundations of mathematics which was the result of the discovery of antinomies in set theory. What one needed then mostly were unquestionable and indubitable foundations on which the "normal" pursued mathematics could be founded. One supposed here of course that mathematics should be an infallible and indubitable science and that in fact it is such a science.

Logicism claims that mathematics can be reduced to logic (or: set theory); intuitionism says that mathematics can be based on the intuition of natural numbers and the latter can be founded on the intuition of a priori time; formalism sees the resource in formal languages to which all mathematical theories should be reduced. As a result one receives idealized pictures of mathematics being really practised.

All this, together with the growing interests in the history of mathematics, led to the rise of new more adequate theories in philosophy of mathematics. Its characteristic feature is the fact that the actual practices of real mathematicians

[2]This name was introduced by P. Bernays.

are taken into account and, secondly, that the reduction of mathematics to one dimension or aspect only is avoided.

## 1. Lakatos and the Conception of Proofs and Refutations

One of the first attempts in this direction was the conception of Imre Lakatos.[3] It was developed under the influence of the philosophy of Karl Popper and was published in the work "Proofs and Refutations. The Logic of Mathematical Discovery" (cf. Lakatos 1963–1964).

Lakatos claims that mathematics is not an indubitable and infallible science – just the opposite, it is fallible. It is being developed by criticizing and correcting former theories which are never free of vagueness and ambiguity. There is always a dangerous that an error or an oversight can be discovered in them. One tries to solve a problem by looking simultaneously for a proof and for a counterexample. Lakatos wrote: "Mathematics does not grow through a monotonous increase of the number of indubitably established theorems, but through the incessant improvement of guesses by speculation and criticism, by the logic of proofs and refutation" (1976, p. 7). New proofs explain old counterexamples, new counterexamples undermine old proofs. By proofs Lakatos means here usual non-formalized proofs of actual mathematics. In such a proof one uses explanations, justifications, elaborations which make the conjecture more plausible, more convincing. Lakatos does not analyze the idealized formal mathematics but the informal one actually developed by "normal" mathematicians, hence mathematics in process of growth and discovery. The work *Proofs and Refutations* is in fact a critical examination of dogmatic theories in philosophy of mathematics, in particular of logicism and formalism. Main objection raised by Lakatos is that they are not applicable to actual mathematics. He also tries to show that the Popperian philosophy of mathematics is possible.

Lakatos claims that mathematics is a science in Popper's sense, that it is developed by successive criticism and improvement of theories and by establishing new and rival theories. But what are in the case of mathematics the "basic state-

[3]Imre Lakatos (1922–1974), proper name Lipschitz, born in Hungary, studied mathematics, physics and philosophy in Debrecin. In 1944 changed his name to Imre Molnár (for safety under Germans) and later (when he found a shirt bearing the monogram I.L.) – to Imre Lakatos. After the war he was an active communist and a high official of the Ministry of Education in Budapest. Arrested in 1950 he spent three years in prison. After release he earned his living by translating mathematical books into Hungarian. In 1956 he left Hungary and emigrated to England where he came under the influence of the philosophy of Karl Popper. He wrote his doctoral dissertation devoted to the history of Euler-Descartes formula $V - E + F = 2$ which says that in any simple polyhedron the number of vertices $V$ minus the number of edges $E$ plus the number of faces $F$ is equal 2. His fame began with the work "Proofs and Refutations" which was published as a four-part series in the *British Journal for the Philosophy of Science* (1963–1964) and later appeared as a book (1964).

ments" and "potential falsifiers"? One finds no answer in *Proofs and Refutations*. A partial answer can be found in his paper "A Renaissance of the Empiricism in the Recent Philosophy of Mathematics?" (cf. Lakatos 1967). It is said there that informal theories are potential falsifiers for formalized mathematical theories. For example: constructing a system of axioms for set theory one takes into account how and to what extent those axioms reflect and confirm an informal theory used in actual research practice. But what are the objects of informal theories, what do they speak about? What are we talking about when we talk about numbers, triangles or other objects? One finds various answers to this question in the history. Lakatos does not take here a definite attitude. He writes only: "The answer will scarcely be a monolithic one. Careful historico-critical case-studies will probably lead to a sophisticated and composite solution" (1967, p. 35). And just in the history one should look for a proper answer. Lakatos claims that the separation of the history of mathematics from the philosophy of mathematics (what was done by formalism) is one of the greatest sins of formalism. In the Introduction to *Proofs and Refutations* he wrote (paraphrasing Kant): "The history of mathematics, lacking the guidance of philosophy , had become blind, while philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become empty" (1976, p. 4).

Lakatos indicated that the classical theories in the philosophy of mathematics are not adequate with respect to actual research practice and proposed a new model being closer to that practice. It is only a pity that he did not succeed to realize his program of reconstructing the philosophy of mathematics in the framework of his epistemology. One should note that Lakatos' scheme of proofs and refutations does not explain the development of all mathematical theories – for example the origin and the development of the theory of groups can be explained as a result of certain unification tendencies. Lakatos was conscious of those limitations. Nevertheless it is doubtlessly his merit that he proposed a picture of mathematics as a science which is alive and which cannot be closed in a framework of a formal system, a picture in which a mathematician developing mathematics is taken into account and in which the actual research practice – and not only idealized reconstructions – is considered.

## 2. Mathematics as a Cultural System

Another attempt to overcome the limitations of the classical theories of the philosophy of mathematics is the conception of Raymond L. Wilder.[4] Wilder pro-

[4]Raymond L. Wilder (1896–1982) studied mathematics at the University of Texas where he also received his doctor degree in topology (the supervisor of his thesis was R.L. Moore). Professor of mathematics at Ohio State University, University of Michigan and University of California, Santa Barbara.

posed to treat mathematics as a cultural system. A source of this conception can be probably seen in his interests in anthropology (his daughter, Beth Dillingham, was an anthropologist and professor of anthropology at the University of Cincinnati).

Wilder presented his ideas in many papers and lectures. A complete version of it can be found in two of his books: *Evolution of Mathematical Concepts. An Elementary Study* (1968) and *Mathematics as a Cultural System* (1981).

The main thesis of Wilder says that mathematics is a cultural system. Mathematics can be seen as a subculture,[5] mathematical knowledge belongs to the cultural tradition of a society, mathematical research practice has a social character. The word 'culture' means here "a collection of elements in a communication network" (cf. Wilder 1981, p. 8). Thanks to such an approach the development of mathematics can be better understood and the general laws of changes in a given culture can be applied in historical investigations of mathematics. It enables also to see the reciprocal relations and influences of various elements of the culture and to study their influence on the evolution of mathematics. It makes also possible to discover the mechanisms of the development and evolution of mathematics. Wilder's conception is therefore sometimes called evolutionary epistemology. He has proposed to study mathematics not only from the point of view of logic but also using methods of anthropology, sociology and history.

In the history of mathematics one can spot various phenomena which develop along general patterns. As an example can serve here the phenomenon of multiples, i.e., cases of multiple independent discoveries or inventions. Probably the best known case is the invention of the differential and integral calculus by Leibniz (1676) and Newton (1671) or the invention of non-euclidian geometry by Bolayi (1826–1833), Gauss (about 1829) and Lobachewsky (1836–1840). Other examples are: the invention of logarithms by Napier and Briggs (1614) and by Bürgi (1620), the discovery of the principle of least squares by Legendge (1806) and Gauss (1809), the geometric law of duality by Plücker, Poncelet and Gergonne (early 19th century). Today such simultaneous and independent discoveries and inventions are very often.

Wilder explain this phenomenon in the following way: mathematicians – who should be treated, of course, as social beings – work on problems which in a given culture are considered as being important, i.e., there are various cultural tendencies which suggest that this or that problem should be solved. If one additionally assumes that the abilities and cultural forces are uniformly distributed then one can expect that solutions will appear simultaneously and independently.

[5]The following problem arises here: when can one see mathematics as a subculture and when only as an element of a culture? In Babylon and Egypt, for example, mathematics did not possess the status of a subculture, today it is certainly a subculture. It seems that the crucial thing is here the fact if the given mathematics has its own tradition, if it can be directly and univocally identified inside a given culture and if it has its own laws of development.

It is not an exception but a rule (not only in mathematics but in science in general). “When a cultural system grows to the point where a new concept or method is likely to be invented, then one can predict that not only will it be invented but that more than one of the scientists concerned will independently carry out the invention” (Wilder 1981, p. 23).

The conception of Wilder enables also the study of the evolution of mathematics in a given culture or between various cultures as well as to predict in a certain sense the development of mathematics (the latter is possible if one knows the laws and rules of development in a given cultural system).

The advantages of Wilder’s conceptions can be better seen when compared with other theories. For example, for E.T. Bell (cf. his book *The Development of Mathematics*, 1945) mathematics is a “living stream” with occasional minor tributaries as well as some backwaters. Moslem mathematics was for him a slow spot in the river, a dam useful for conserving the water. It was necessary to conserve the achievements of the Greek mathematics but it brought no new original results.

For O. Spengler (cf. his *Der Untergang des Abendlandes*, 1918, 1922) mathematics is pluralistic, every culture has its own mathematics. And cultures are organisms like men, they go through definite stages of development. Hence, for example, the Greek mathematics became quite different, quite new mathematics under the Moslems. Greek mathematics had been an abstract and speculative activity of a leisure class, Moslem mathematics was a concrete and practical activity of the descendants of nomads who had been shaped by a somewhat harsher environment. It was simply the mathematics of another culture. Spengler would never condemn the Moslems and their mathematics for not continuing the Greek tradition, as Bell did.

For Wilder a culture, and in particular the mathematical culture, is not an organism which goes through various stages of development from youthful to decline and death (as it is the case by Spengler) but a species that evolves. Hence he does not say that the Greek mathematics died with Moslem mathematics thence born, but rather that mathematics moved from one culture to the other and subjected to different cultural forces, altered its course of development. But it was the same mathematics.

Wilder formulates certain laws governing the evolution of mathematics. We give here some examples:

1. New concepts usually evolve in response to hereditary stress (unsolved problems) or to the pressure from the host culture (Wilder calls it environmental stress).
2. An acceptance of a concept presented to the mathematical culture is determined by the degree of its fruitfulness. It will not be rejected because of its origin or because it is “unreal” by certain metaphysical criteria (com-

pare, e.g., the introduction to mathematics of negative integers, of complex numbers or of Cantor's theory of infinite sets).

3. An important and crucial element in the process of accepting new mathematical concepts is the fame or status of its creator. It is especially important in the case of concepts which break with tradition. This same applies also to the invention of new terms and symbols. (Cf. the introduction of $\pi$, $e$ and $i$ by Euler).
4. The acceptance and importance of a concept or theory depend not only on their fruitfulness but also on the symbolic mode in which they are expressed. As an example can serve here the case of Frege and his sophisticated and complicated symbolism – this symbolism was replaced later by better and simpler symbolism due to Peano and Russell but the ideas of Frege survived and were further developed.
5. At any given time there exists a cultural intuition shared by (almost) all members of the mathematical community.
6. Diffusions between cultures or between various fields in a given culture result in the emergence of new concepts and accelerated growth of mathematics.
7. Discovery of inconsistency in existing theories (or in existing conceptual structure of a theory) results in the creation of new concepts. As an example one can mention here the crisis of the ancient Greek mathematics that resulted from the discovery of irrationals and the theory of proportion by Eudoxus as well as antinomies of set theory and the emergence of axiomatic systems of set theory.
8. There are no revolutions in the core of mathematics – they may occur in the metaphysics, symbolism or methodology of mathematics.
9. Mathematical systems evolve only through greater abstraction, generalization and consolidation prompted by hereditary stress.
10. A mathematician is limited by the actual state of the development of mathematics as well as of its conceptual structure. As an example of this rule let us mention here mathematical logic. It was hardly possible to create it in the earlier stages of the development of mathematics because the new attitude towards algebra and its symbols was required. In fact one needed the idea that algebraic symbols do not necessarily represent numbers but that they can represent arbitrary objects of thought satisfying certain operational laws.
11. The ultimate foundation of mathematics is the cultural intuition of the mathematical community. Without that intuition mathematical investigations would be fruitless.
12. As mathematics evolves, hidden assumptions are made explicit and either generally accepted or (partially or fully) rejected. The acceptance usually follows an analysis of the assumption and a justification of it by new meth-

ods of proof. As a classical example can serve here the axiom of choice used by Peano in 1890 in the proof of the existence of a solution of a system of differential equations. In 1902 Beppo-Levi recognized it as an independent proof principle and in 1904 Zermelo used it in his proof of the well-ordering theorem. It was this latter proof that made mathematicians to discuss the axiom carefully and to study its status.

13. Since mathematics has a cultural basis, hence there is no such thing as the absolute in it, everything is relative. In particular any mathematical concept must be related to the cultural basis which engendered the mathematical structure to which it belongs. There is also no unique universal pattern of precision and correctness and one cannot say that mathematics in a given period was not precise enough. There are also no absolute objects studied by mathematics. The only existence that the mathematical objects have is the existence of the cultural objects. Mathematical concepts are created by mathematicians – of course it is not being done in an arbitrary way but on the basis of objects already existing and under the influence of the inner stress (unsolved problems) or of the environmental stress (problems of other sciences or of the praxis). Hence in particular the concept of a number is a cultural object and as such it should be investigated.

Wilder maintains that mathematical concepts should be located in the Popper's "third world". Mathematics investigates no timeless and spaceless entities. It cannot be understood properly without regarding the culture in the framework of which it is being developed. In this sense mathematics shares many common features with ideology, religion and art. A difference between them is that mathematics is science in which one justifies theorems by providing logical proofs and not on the basis of, say, general acceptance.

## 3. The Conception of Hersh

An interesting synthesis of the conceptions by Lakatos and Wilder is the paper by Reuben Hersh[6] "Some Proposals for Reviving the Philosophy of Mathematics" (1979).

He says first of all that the thesis "that mathematics must be a source of indubitable truth" (1979, p. 32) is simply false. This thesis is not confirmed by the real practice of mathematicians. In fact we have in mathematics no absolute certainty – mathematicians make mistakes and later correct them, they are often not sure if a given proof is correct or not. Mathematicians deal in their research

[6]Reuben Hersh is a professor of mathematics at the Universtity of New Mexico. He works in the domain of partial differential equations.

practice with ideas. They are using symbols just to be able to tell about ideas and to communicate their results to others (it is similar to the usage of scores in music). Axioms and definitions serve to describe main properties of mathematical ideas. But there are always properties which are not explicitly described in them. Hersh says that "a world of ideas exists, created by human beings, existing in their shared consciousness. These ideas have properties which are objectively theirs, in the same sense that material objects have their own properties. The construction of proof and counterexample is the method of discovering the properties of these ideas. This is the branch of knowledge which we call mathematics" (1979, p. 47).

## 4. Intensional Mathematics

All attempts to give a new explanation of the phenomenon of mathematics described above are reactions to the classical tendencies in the philosophy of mathematics which tried to build an indubitable and unquestionable foundation for it and which disregarded the real practice of mathematicians. One should also note that there are not only attempts to replace the old theories but also to extend them in such a way that the practice would be regarded. We mean here the so called intensional mathematics. It is an attempt to build dualistic foundations of mathematics and to treat it in a similar way as, e.g., the quantum mechanics where the observer must be taken into account. One proposes to add to the classical mathematics certain epistemological notions. In particular an idealized mathematician (it should be an idealized picture of a mathematician in a collective sense) and an epistemological operator $\Box\Phi$ ($\Phi$ *is knowable*) are introduced. It is assumed that it has the following properties (they are analogous to the properties of the operator of necessity in the modal logic) (cf. Goodman 1984):

1. $\Box\Phi \longrightarrow \Phi$,
2. $\Box\Phi \longrightarrow \Box\Box\Phi$,
3. $\Box\Phi \;\&\; \Box(\Phi \rightarrow \Psi) \longrightarrow \Box\Psi$,
4. from $\vdash \Phi$ infer $\Box\Phi$.

Axiom 1 says that everything knowable is true. Axiom 2 states that anything knowable can be known to be knowable. Axiom 3 claims that if both a conditional and its antecedent are knowable then the consequent is knowable. Rule 4 says that if we actually prove a claim then we know that claim to be true, and so that claim is knowable. One should note that if we take $\Box$ to mean *known* then there arise problems with Axiom 3. In fact Axiom 3 would claim in this case that whenever our mathematician can make an inference, he has already done so. But even an idealized mathematician does not follow out every possible chain of inference. Hence we take $\Box$ to mean *knowable*.

The first system constructed according to the described ideas and taking into account the epistemological aspect was the system of arithmetic due to S. Shapiro (cf. Shapiro 1985a). Later other systems were constructed, e.g., a system of set theory, of type theory, etc. (cf. Shapiro 1985b).

Systems of the indicated type make possible the study of constructive aspects of proofs and classical inferences. They enable also to take into account the ways in which mathematical objects are given. From the philosophical point of view one rejects here the platonism.

It is impossible to give here further technical details. Nevertheless it should be said that systems of intensional mathematics are an interesting attempt to develop classical – as a matter of fact – foundations of mathematics and simultaneously to take into account the real practice of a mathematician. Just this tendency is, as we tried to indicate above – the leading tendency in the modern philosophy of mathematics.

The new trends in the philosophy of mathematics should not be treated as competitive with respect to old theories. They should be rather seen as complements of logicism, intuitionism and formalism. One is looking here not for indubitable, unquestionable and irrefutable foundations of mathematics, one tries not to demonstrate that the real mathematics can be reconstructed as an infallible and consistent system but one attempts to describe the real process of building and constructing mathematics (both in individual and historical aspect).

# 6

# REMARKS ON THE STRUCTURALISTIC EPISTEMOLOGY OF MATHEMATICS[1]

(Co-authored by Izabela Bondecka-Krzykowska)

The paper is devoted to the discussion of structuralistic solutions to principal problems of the epistemology of mathematics, in particular to the problem: how can one get knowledge of abstract mathematical entities and what are the methods of developing mathematical knowledge. Various answers proposed by structuralistic doctrines will be presented and critically discussed and some difficulties and problems indicated.

## 1. Introduction

Mathematical structuralism can be briefly characterized as a view that objects studied by mathematics are structures. Hence the slogan connected with this: mathematics is the science of structures. In the philosophy of mathematic, structuralism is often treated as an alternative to platonism. Its chief motivation and aim is to avoid some ontological and epistemological problems of the latter without the necessity of rejecting realism.[2]

[1] Originally published as a joint paper with I. Bondecka-Krzykowska in *Logique et Analyse* 193 (2006), 31–41. Reprinted with kind permission of the editor of the journal *Logique et Analyse*.

[2] Note that structuralism, as defined above, is not a form of platonism (in a strict sense). In the ontological issues the differences between them seem to be significant. Platonism claims that mathematics is a science about independently existing mathematical objects – they are independent of any human activities, of time and space but also of one another. Structuralism rejects this form of independent existence. It claims that mathematical objects have no important features outside structures they belong to and that all of the features must and can be explained in terms of relations of the structures. Note that the *ante rem* structuralism (see below for an explication of this term) claims that structures exist independently of human activities (therefore it is sometimes called platonistic structuralism) but it does not concern the very objects of mathematics (such as numbers, points, lines, etc.).

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One of the main problems that realism is faced with is the question: how mathematical methods, in particular computing and proving, could generate information about the mathematical realm and whether such a knowledge is legitimate. Almost every realist agrees that mathematical objects are abstract entities, hence the problem reduces to the question: how can we know anything about abstract objects, how can we formulate beliefs about such objects and claim that our beliefs are true? Structuralism attempts to avoid those questions by maintaining that mathematical objects, such as numbers or points, are only positions in appropriate (mathematical) structures and that we cannot possess knowledge about such isolated objects outside the structures. On the contrary, we can get to know only structures or their parts and not single numbers or points. But now a question arises: how can we get knowledge about structures?

In the contemporary philosophy of mathematics various structuralistic conceptions were formulated – they offer also various solutions to this principal epistemological question. Let us mention here at least structuralism of Parsons, Shapiro's axiomatic theory of structures, the theory of patterns developed by Resnik and Hellman's modal structuralism. Those theories propose in particular different answers to the question about how structures can be defined and about the very existence of structures. Generally one can distinguish two main attitudes towards ontological problems in structuralism:

1. *in re* structuralism (called also eliminative structuralism), and
2. *ante rem* structuralism.

The main thesis of the eliminative structuralism (whose examples are Parsons' and Hellman's structuralistic conceptions) is: statements about some kind of objects should be treated as universal statements about specific kind of structures. So in particular all statements about numbers are only generalizations. The *in re* structuralism claims that the natural number structure is nothing more than systems which are its instantiations. If such particular systems were destroyed then there would be also no structure of natural numbers.

Add that eliminative structuralism does not treat structures as objects. It is claimed that talking about structures is only a comfortable form of talking about all systems which are instances of the given structure. Therefore this form of structuralism is called by many authors "structuralism without structures". On the other hand one needs here a basic ontology, a domain of considerations whose objects could take up places in structures *in re*. Such an ontology should be rich enough and we are not interested in the very nature of objects but rather in their quantity. The ontology of the *in re* structuralism requires an infinite base.

The *ante rem* structuralism – for example Shapiro's theory of structures – claims that structures do exist apart from the existence of their particular examples. It is often said that *ante rem* structures have ontological priority with respect to their instantiations.

The different versions of structuralism have – as indicated above – different ontologies. But they have also different epistemologies and propose different answers to the main questions formulated above. With respect to this questions the hard part – from the eliminative perspective – is to understand how can we know anything about systems of abstract objects that exemplify *in re* structures. On the other hand, the *ante rem* structuralism must speculate how do we accomplish the knowledge about structures which exist independently of their instantiations.

## 2. How do We Get Knowledge about Structures?

Structuralism claims that mathematical objects are only positions in structures and that consequently one cannot possess any knowledge about, say, single numbers or points – on the contrary, one can recognize only structures. But how can one get knowledge about structures? The answer to this question depends on the type, more exactly, on the size of the considered structures. So let us distinguish some cases:

- *Small finite structures.* In this case knowledge about structures is apprehended through abstraction from their physical instances via pattern cognition. The process of acquiring beliefs about patterns (structures)[3] can be described as a series of stages: (a) experiencing something as patterned, (b) recognizing structural equivalence relations, (c) level of predicates, (d) supplementing predicates with names for shapes, types and other patterns. It is worth noticing that the abstraction process yields necessary truths or a priori knowledge. Such approach treats mathematics like other sorts of empirical knowledge.
- *Large finite structures.* The method of pattern cognition described above works only for small structures whose instances can be perceived. This idea is not appropriate with respect to structures we have never seen, for example a billion-pattern. In this case another strategy is used.
  A small finite structure, once abstracted, can be seen as forming a pattern itself. Next one *projects* this pattern or those patterns beyond the structures obtained by simple abstraction. Reflecting on finite patterns one realizes that the sequence of patterns goes well beyond those one has ever seen, for example the billion-pattern. Hence we have the first step to knowledge about *ante rem* structures.
- *Countable structures.* The strategy of grasping large finite structures described above can be adopted to the simplest infinite structure, i.e., to the natural number structure. One first observes that finite structures can be

[3]The term 'pattern' appears in papers and books by Resnik and it is used either as a synonym of the term 'structure' or to indicate a physical example of an abstract structure.

treated as objects in their own right. Then a system of such objects with the appropriate order is formed. Finally the structure of this system is being discussed. The important point that should be stressed here is that such strategy fits the *ante rem* structuralism, because in the case of the eliminative structuralism there might not be enough finite structures.[4]
After a given structure has been understood one can discuss and describe other structures in terms of this structure and structures one had known before. For example, the integer structure can be understood as a structure similar to the natural number structure but unending in both directions. The rational number system can be seen as a structure of pairs of natural numbers with the appropriate relations. Another original method of introducing abstract objects was presented by S. Shapiro in (1997). A kind of linguistic abstraction over an equivalence relation on a base class of entities has been used there.

Notice that all the methods of apprehending structures we described above can be applied only to denumerable structures, i.e., to structures with denumerably many places. But what about larger structures?

- *Infinite uncountable structures.* The most powerful but simultaneously most speculative technique of grasping structures is their direct description by an implicit definition (statements used in it are usually called axioms). Such a definition provides a characterization of a number of items in terms of their mutual relations. It can characterize a structure or a possible system. In this way one defines, e.g., natural numbers or real numbers.
- *From old structures to new ones.* There are still other ways of getting knowledge about new structures: one can collect patterns (originally treated in isolation) into a new pattern or "extend" the old ones (compare the definition of integers or the definition of the rational numbers). Mathematics itself also produces new structures and theories by proving, calculating and finding solutions to problems. This can lead to new theories of new mathematical objects such as the theory of equations, proof theory or the computation theory.

## 3. Mathematical Methods and Knowledge of Structures

One of the major problems facing mathematical realism is to explain how do mathematical methods – such as, e.g., computing and proving – generate information about the mathematical realm. One of the possible answers is that

[4]On requirements needed for the ontology of the *ante rem* and *in re* structuralism we wrote above. Add also that Field in (1980) tried to give an argument that there is enough concrete stuff to get the continuum. His ideas have been discussed (but also criticized) by many authors.

mathematicians learn about this realm appealing to structural similarities between abstract mathematical structures and physical computations and diagrams (note that the latter are always finite whereas patterns may be vastly infinite). But mathematicians can and do obtain evidence of higher-level theories also through results belonging to more elementary levels. Resnik tried in (1997) to describe connections between certain elementary mathematical results and physical operations that we can perform.

The examples given by him show that operations on dot templates can generate information about some features of sequences of natural numbers.[5] Of course finite templates can represent only initial segments of an infinite number sequence. Some properties of initial segments can be generalized to the infinite sequence of natural numbers, however this generalization is not always simple and straightforward. Observe also that mathematicians do not work with dots but they are doing computations using Arabic numerals and methods we learned at school. Nevertheless Resnik (1997, p. 236) claims that this is not important because "if we seek a more basic explanation of why they work, we can appeal to theorems of some axiomatic number theory, or alternatively we can explain our current rules in terms of dot arithmetic".

Notice that, unfortunately, we cannot explain the computation of the values of a derivative, a trigonometric expression or a transfinite polynomial by the arithmetic of dots. In those cases there is no straightforward connection between computations and patterns they concern. So rules of such computations are theorems of some axiomatic system describing the pattern.

In practice most proofs of theorems are in fact not proofs within an explicitly formulated axiomatic system. This is no problem when the premises of a given mathematical proof state uncontroversial features of the pattern in question.

On the other hand there is a problem if a proof of a theorem about some simple structure employs facts concerning other, more complex, structures. For example, proofs in elementary number theory can appeal to premises from real or complex analysis. Resnik claims, however, that it is not really a problem because this situation is similar to the situation when one is proving a fact concerning the natural number system by appealing to some features of its initial segment. In our opinion this is not so simple. Indeed, one of the major presuppositions of structuralism is that all facts about mathematical objects should be expressed and explained in the language of the structure they belong to. Hence using facts about a different structure in order to prove a statement about the given one does not fit to it (even if one notice that, e.g., the natural number structure can be treated as a part of the real or complex number structure[6]).

[5]The term 'pattern' (or 'structure') is reserved by Resnik for abstract patterns. The term 'template' is used to refer to concrete devices representing how things are shaped, designed or structured.

[6]To explain this one should recall some facts from model theory, in particular the distinction between being a submodel, being an elementary submodel and being elementarily equivalent.

So we might get information about structures by manipulating templates, one can even prove theorems in such a way. The question is: how can we know that the premises of proofs are true of the pattern. Resnik responds here by saying that they constitute an implicit definition of the pattern. Theorems of a given branch of mathematics are supposed to be true in the structure they are describing, they follow from the clauses defining the structure in question. But one should remember that this claim is connected with the claim that structures of the considered type do exist. The latter existential claim is not a logical consequence of the very definition of the structure. Thus combining structuralism with the doctrine of implicit definitions does not make mathematics analytic.

The problem of existence together with another one, namely the problem of categoricity, appears quite clearly in the situation when the structure is introduced by implicit definitions where one characterizes objects in terms of their interrelations (this is the method mostly used in mathematics, the axiomatic method). Mathematical logic and in particular model theory provide some methods of solving them and indicate simultaneously various connections and interdependencies between structures (models) and languages used. But are they compatible with structuralistic attitude and structuralistic presuppositions? The answer seems negative. In fact the most delicate problem is the existence problem. Can one claim that a structure defined by an implicit definition, hence by a set of axioms, does exist by appealing to the consistency of the axioms and to the completeness theorem (stating that a consistent set of axioms has a model)? No "normal" mathematician is doing so. Furthermore, the proof of the completeness theorem provides a model constructed on terms. From the point of view of a real mathematics this is extremely artificial and unnatural! If such methods were rejected so where from should we know then that structures defined by implicit definitions do exist? What influence would it have on the distinctions between *in re* and *ante rem* structuralism? Would the structuralism *in re* be possible in this situation?

There are also other methods of showing that defined structures do exist – one of them is to construct examples of them in set theory. But the latter has no structuralistic base and is not founded and justified in a structuralistic way.[7] In which sense can one say then that instantiations of defined abstract structures are known?

Another problem is the problem of uniqueness, i.e., the problem whether the implicit definitions, hence the axioms, define the appropriate needed structure in a unique way. Even in the simplest case of the structure of natural numbers there arise big problems. In fact first order arithmetic is not categorical, i.e., it has nonstandard models, hence models different (non-similar, non-isomorphic) to

[7] An attempt to provide structuralistic account of set theory made by Hellman in (1989) is – in our opinion – not satisfactory.

standard, intended one. On the other hand Löwenheim-Skolem theorems show that any theory with an infinite model has also models of any cardinality. Thus nonstandard models of first-order arithmetic can be even uncountable! This is very far from the intended structure of natural numbers! To characterize natural numbers in a categorical way and to obtain a categorical arithmetic one should use second-order logic (which is in fact natural for mathematical research practice). Unfortunately there arises a problem: how second-order variables should be understood in structuralistic terms?[8]

Besides difficulties indicated above there are also other connected with the structuralistic approach that should be considered and solved. The most important is the problem of infinite and more complex structures studied by mathematicians.

One can try to look for a solution using Takeuti's result (cf. Takeuti 1978) which states that more complex mathematical theories such as (parts of) analysis can be translated into number theory if the definitions are explicit. In other words: there exist extensions of number theory[9] which are conservative over it and in which one can develop a sufficiently large portion of analysis provided that one uses only predicative definitions. Though promising this does not give a solution. Indeed, most more complex theories are extensions of number theory obtained by adding implicit definitions, i.e., axioms. So even if, as Takeuti in (1978) shows, "theorems which can be proved in analytic number theory can be proved in Peano arithmetic", this does not solve the problem.

One can also try to argue using results of the so called reverse mathematics initiated by H. Friedman and developed intensively by S.G. Simpson. They show that a significant and important part of mathematics can be developed in fragments of analysis (second-order arithmetic) which are conservative with respect to arithmetic of natural numbers.[10] The problem is here that such reductions and proofs in those systems are quite different than proofs presented in "real" mathematics, in fact they are far from real research practice of mathematicians and are artificial from a point of view of a working mathematician. They can be considered only as (foundational) reconstructions of mathematics and do not explain the real mathematics as it is being done.

Artificial is also an attempt to reduce a more complex theory to number-theoretic structure using completeness theorem or Löwenheim-Skolem theorem. According to them every consistent theory has a countable model, hence a model whose universe is the set of natural numbers and whose relations can be interpreted as relations among natural numbers. Though it gives a reduction of a complex theory to a simple one, namely to the arithmetic of natural numbers, but this

[8] Note that Boolos in (1985) has made an attempt to solve this problem.

[9] In Takeuti (1978) two such systems are described and studied.

[10] A presentation of those results and of their meaning for foundations of mathematics, in particular for the Hilbert's program can be found in Murawski (1999a).

reduction is entirely unnatural from the point of view of the mathematical practice and from what mathematicians are really doing. No “normal” mathematician will accept this as a picture, as a model of his/her research practice.

On the other hand there are results in number theory whose proofs really need much more than Peano arithmetic and consequently much more than the considerations of finite patterns (proposed by structuralists) can give. We mean here results by Kirby, Paris, Harrington and Friedman on true arithmetical sentences which can be proved only using necessary some methods of set theory, i.e., some infinite objects (cf. Friedman 1998 or Murawski 1999a). One can argue – trying to defend the structuralistic doctrine – that those results provide examples coming from metamathematical and not directly mathematical considerations or mathematical practice but it does not help and the problem is not solved.

## 4. Conclusions

The main question considered in this paper was: how can one know anything about abstract mathematical objects which are only positions in structures? Since one cannot get to know isolated mathematical objects, the problem is: how can one recognize structures or their parts?

Structuralists are providing various answers to those questions. Resnik for example claims that mathematicians are getting knowledge about the mathematical realm by appealing to structural similarities between abstract mathematical structures and physical computations and diagrams.

It is often stressed that one gets the knowledge about structures by abstraction from concrete examples of them. This can work fairly well in the case of finite small structures which can be apprehended through abstraction from their physical instances via pattern cognition.[11] It is possible also in the case of finite large structures. But what about infinite structures which do not have any concrete instantiations that could be investigated directly?

Observe that all examples provided and considered by structuralists are usually restricted to natural numbers (and sometimes other number structures). But what about other (really abstract) objects like those studied in more advanced branches of mathematics as functional analysis, topology, etc.? Explanations provided by structuralists are not fully satisfactory in those cases!

In the case of more advanced and more sophisticated structures one can refer in fact to methods of model theory. But does it suffice to explain the full richness of the realm of structures of the real mathematics? On the other hand all restric-

[11] This indicates also the role of pictures and diagram in the process of developing mathematical knowledge (cf. Brown 1999).

tions and specifications of methods and theorems of the theory of models should be taken into account and respected. This concerns in particular the problem of proving categoricity usually connected with the language chosen to describe and characterize the defined structures – we indicated it above on the example of the categoricity of the structure of natural numbers defined as a structure satisfying appropriate axioms (i.e., Peano's axioms).

It should be also added that the usage of methods of mathematical logic and in particular of the model theory can be a source of doubts whether the proposed explanations do concern the real cognitive and epistemic activity of a real mathematician (as it seems to be the case when simple structures are being considered) or provide rather an artificial reconstruction of real mental processes (in the spirit of foundationalist theories in the philosophy of mathematics).

* * *

Structuralism is an interesting proposal in the epistemology of mathematics and provides a reasonable alternative to the platonism (not rejecting realism). But explanations of the process of getting and developing mathematical knowledge given by it are in fact mostly restricted to simple number structures. Above we indicated some difficulties one meets when trying to apply structuralistic approach in the case of more abstract, more complex and more sophisticated parts of mathematics. If structuralism wants to be a doctrine explaining the whole real mathematics (and not only its elementary fragments) then those problems should be solved.

# 7

# FROM THE HISTORY OF THE CONCEPT OF NUMBER[1]

(Co-authored by Thomas Bedürftig)

## 1. Introduction

What are numbers, where do they come from, what is their nature? The ancient Greeks seem to be the first who formulated 2500 years ago the ontological question about the concept of number. Today one is very cautious in this respect and tries to omit the problem. This is the case especially by those from whom one should expect to receive the answer, namely by mathematicians.

Instead of an attempt to answer the question "What are numbers?" we are cautious and adopt a "structuralistic" attitude: we ask how does one count and calculate using numbers, where are they used? This is explained usually by giving appropriate axioms. In this way one describes the structure formed by numbers and avoids the ontological problems. In the sequel by using the term 'number' we mean always 'natural number'.

In university teaching one is taught that the whole of mathematics is founded on logic and set theory. The concept of a natural number can be then defined by using some set-theoretical operations and the empty set. Negative integers, fractions (rational numbers), roots, reals and complex numbers can be constructed on the basis of natural numbers by set-theoretical methods. In this way the ontology of numbers is reduced to the ontology of sets.

What do we get in this way? There are various definitions of natural numbers in set theory. And what are sets? There are many various set theories. Which definition, which set theory should be chosen? Should we refer to our intuition to answer this question? But what is in fact intuition? Does it come from the

[1]Originally published as a joint paper with Th. Bedürftig under the title „Alte und neue Ansichten über die Zahlen – aus der Geschichte des Zahlbegriffs", *Mathematische Semesterberichte* 51 (2004), 7–36.  Translated into English by Th. Bedürftig and R. Murawski. Published with kind permission of Springer Science and Business Media.

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practice of using numbers? Do we all have the same intuition? What do we get by reducing the ontology of numbers to the ontology of sets?

Such a reduction seems to give us an answer to the question "What are numbers?" via a chosen definition in a chosen set theory and via omitting the problem of the ontology of sets. On the other hand at the beginning of the 20th century it turned out that the reduction to pure logic is impossible. The ontological problem cannot be solved by mathematical means only.

So can one expect to receive an answer to the question about the nature and essence of numbers from the philosophy? How did mathematical attempts to find the answer look like and where did they lead?

Considering the history of the concept of number, one should always take into account the philosophical background that influenced the appropriate view. Such philosophical backgrounds can be classified in the form of the following triangle:

*spiritual world* •

*thinking* • • *reality*

The fundamental question is: where are the sources of knowing and of concepts? Conceptions corresponding to the limit points indicated above are classical philosophical conceptions:

- idealism which sees the source of knowing in the spiritual, intelligible world,
- empiricism according to which the reality and the experience are source of all knowledge,
- rationalism which claims that the foundation of our knowledge is the structure of our thinking.

The famous representative of the idealism was Plato. Positivists represent clearly the empiricism and Kant represents what we – partially contrary to the usual classifications – call rationalism.

We shall describe in a chronological order some conceptions and characterize them. At the end we shall attempt to present our own conceptions and to consider the problem of the philosophical relevance of some current trends.

## 2. Antiquity

In the **prehistory** numbers have been developed as instruments used in the everyday life. They were used to solve various practical problems. In the **ancient**

**Egypt** and **Babylon** they served as measures in the astronomy and as symbols with mystical meaning in the astrology. Pythagoreans seem to be the first who asked about the essence and nature of numbers.

**Pythagoras** (about 570 – 500 B.C.) traveled to Egypt and Babylon and learned there arithmetic, astronomy, geometry as well as the mystic of numbers. Later he transformed this knowledge into a philosophical metaphysics. “All is number” was the main philosophical thesis of Pythagoreans. Only this which has a form is knowable – said Philolaos, a pupil of Pythagoras – and form is based on measure and number. For Pythagoreans numbers form a higher spiritual world and a pattern for everything that exists.

This conception is important for at least two reasons:

- It explains the origins of a theory. In fact, Pythagoreans investigated the higher world of number to get knowledge about the material world. It was impossible to justify the laws of numbers on the basis of experience in the material world. They *should* be justified within the world of numbers. Hence the first theory was established: the theory of numbers.
- This number theory is simultaneously mathematics and metaphysics. So the knowledge of things was based on the knowledge about numbers and their interrelations.

Hence at the beginning of the philosophy there is – one can say – mathematics. But the theory of numbers as a philosophical project of a reality has been destroyed when the phenomenon of incommensurability had been discovered.

The meaning of numbers by Pythagoreans was manifold. According to Jamblichos (about 300 B.C.) Pythagoras said that numbers are the “development” of “the creating principles lying in the unity”. This formulation explains why Pythagoreans did not treat the unit as a usual number but saw it as a principle of a unity and as a source of “usual normal numbers”.

The usual numbers have beside their practical meaning also the metaphorical meaning coming back to their mystical and symbolical sources. The even numbers were “feminine”, the odd numbers – “masculine”, five, the sum of first odd and the first even number symbolized the marriage. Ten, the sum of the first four numbers – *τετράκτυς* (tetraktys) – was a “divine” number. They distinguished also *perfect* numbers, i.e., numbers being equal to the sum of their divisors – and formulated a task to provide a formula giving perfect numbers. The unity (one) was interpreted geometrically as a point – the geometrical unit, two – as a line, three – as a surface, four – as a body and the three dimensions of the space.

Numbers were for Pythagoreans not abstractions, they were real forces acting in the nature. “Number” was a principle of all that exists.

One can shortly characterize numbers of Pythagoreans so:

> Numbers are elements (coming from the unit) of the higher world. They are spiritual powers – over things.

Every segment was for Pythagoreans – relatively to a given measure unity – a number or a relation of numbers and consequently geometry was a part of the theory of numbers. The absolute superiority of numbers was finished by the discovery of the phenomenon of incommensurability about 450 B.C. There were no numbers which could give the relation of the side and the diagonal of a regular pentagon (cf. Bigalke 1983). This discovery meant a real philosophical and *Weltanschauung* crisis. Maybe it can be treated as the birth of mathematics that lost its metaphysical function and was separated from philosophy.

“A unit is that by virtue of which each of the things that exist is called one” and “A number is a multitude composed of units” wrote **Euclid** in Book VII of his *Elements*. In this work geometry plays the first role. Euclid described the theory of quantities (developed in the first half of the 4th century by Eudoxos) in Book V of *Elements* before the theory of numbers. The latter is by him a part of the theory of quantities and their relations.

*Ideas* formed by **Plato** (427 – 347 B.C.) – similarly as numbers by Pythagoreans – a higher world. They possessed their own existence and determined the existence of real things. Plato was then a realist – the reality, i.e., the higher reality of ideas and the lower one of material things exist independently of human thinking.

The material things formed a domain of causality, imperfectness and being not sure. The world of ideas was characterized by safety, clarity, necessity and perfectness. At the end of Book IV of *State* Plato considers the classes of objects described in the following table:

| | | |
|---|---|---|
| nonmaterial | ideas | unity, beauty, justice, circularity, ... |
| | mathematical objects | circle, number, relation, proportion ... |
| material | physical objects | wheel, table, house, tree... |
| | pictures of physical objects | picture of a wheel, picture of a tree ... |

Ideas are unique: there is only one idea of unit, of beauty or of circularity. Physical things are, e.g., circular if they participate in the idea of circularity. There are many circle objects. A human being gets the knowledge – means by Plato that – his/her soul recalls the ideas and their interrelations that it saw before its world existence.

The mathematical objects are – according to the interpretation of Plato by Aristotle (*Metaphysics* A 6; cf. Thiel 1982, p. 19) – *between* ideas and physical objects. There are many realizations of them: there are many circles, many numbers. On the other hand the mathematical objects are nonmaterial and therefore possess the property of safety, clarity and necessity.

Plato says often about numbers in a similar way as about ideas. They seem to be of a special kind among mathematical objects. Except the unit (*τό ἕν*) from which by Pythagoreans emerged all numbers, there appears the two, the proportion, *λόγος*, where the numbers of Pythagoreans collapsed. Using that he attempts to explain – again according to Aristotle – in a bit obscure way the origin of numbers in a similar way as the origin of the ideas (cf. Toeplitz 1931, p. 60*ff*).

Numbers by Plato can be characterized in the following way:

> Numbers are non-material intermediators *between* ideas and the material reality.

Human knowledge consists of remembrances of a soul about ideas which it saw before its earthly existence.

> Numbers provide – in a similar way as geometry – the gate to the world of ideas.

A proper method to describe and to investigate the domain of mathematical objects was – according to the ancient Greeks – the axiomatic method., i.e., the reduction of mathematical statements and concepts to axioms and primitive notions. Most probably it was just Plato who essentially contributed to the establishing of this method. The source of mathematics is – according to Plato – not experience but it forms its own mental domain.

**Aristotle** (ca. 384–322 B.C.) developed another conception of mathematical objects than his teacher Plato. For him, e.g., "three" was not an independently existing non-material object but something like a property of material objects: for example, a table has the length of three feet, something lasted three years, etc. Mathematical objects do *not* belong to an independent non-material domain. They emerge by *abstraction* from real objects (*Metaphysics*, Book M 1–3; cf. Thiel 1982, pp. 22–28). But numbers are not detached from things as properties – as one understands today abstraction. On the opposite: they belong inseparably to the essence of things. They did not loose their Pythagorean power to determine things: they are forming, together with other properties, the matter to become particular objects. They are inseparably connected with things. Briefly:

> Numbers are (spiritual) forms – *in* things.

A human being grasps in numbers and forms the essence of things and refers in this way to the spiritual world to which he/she belongs together with his/her knowing soul. Hence the philosophical conception of Aristotle is based in the reality. It cannot be characterized as a point in the diagram indicated above but rather as a vector in the triangle of basic positions – in fact as a vector that leads from the reality to the spiritual world.

One should mention here also Aristotle's contribution to the study of the infinity. He accepted only the *potential* infinity. He rejected the *actual infinity* and his attitude dominated the mathematical as well as the philosophical investigations through the next 2200 years.

### 3. Nicolaus of Kues, Kant, Mill

**Nicolaus of Kues** (1401–1464), one of the last scholastics, was a mathematician and theologian as well as a preparer of the philosophy of modern times. Mathematics play an important role in his philosophy (cf. Radbruch 1999). His conceptions can be placed in the middle of the indicated fundamental positions.

The *spiritual world* is represented – according to Nicolaus von Kues – by God. God created the world according to mathematical principles, numbers and ideas come from Him. This idea appeares again by Gottfried Wilhelm Leibniz (1646–1716): "Dum DEUS calculat et cogitationem exercet, mundus fit".

In *Liber de mente* speaks Nicolaus von Kues about the concept of number. He distinguishes numbers as objects of mathematics – their source is human mind, and numbers that exist in God. The "human" numbers are reflections of "God's numbers". What is measurable in real things is a realization of God's numbers. Numbers in mathematics are only products of human mind and they exists only as ideal objects in the mind.

How does a human being create mathematical objects and numbers, how are God's numbers reflected in human mind? Nicolaus' attempts to answer those questions reveal a *rationalistic* and *empiristic* features of his conception. He indicates a particular feature of human thinking consisting in the ability to simulate – in an own construction – the process of creating due to God. This simulation is based on an abstraction from real things. Nicolaus writes in *Liber de mente*: "[...] there is nothing in the mind that before was not in senses" ("[...] ut nihil sit in ratione, quod prius non fuit in sensu"). So one can try to summarize his conception of a number in the following way:

> Numbers are rational reconstructions of God's principles that are active in the reality.

We should also say some words about **René Descartes** (1596–1650). His unification of algebra and arithmetic with geometry done in *Géométrie* (1637) influenced very much the future views on numbers. Numbers obtained in this way new, still more and more abstract character and lose their connections with quantities what was the case since Euclid's *Elements*.

Quite new elements in the approach to numbers can be found by **Immanuel Kant** (1724–1804). The base of our considerations is here his fundamental work

*Kritik der reinen Vernunft* (Kant 1781, 1787). The main feature of his epistemology was first of all to indicate the *a priori elements* of our thinking and knowledge, i.e., those conditions that proceed any experience, that give appropriate structure to data of experience and that make our knowledge possible. Among them are the forms of pure intuition (*Anschauung*) of space and time as well as the *a priori* structure of the reason given by categories of reason.

Space and time – as forms of pure intuition – are not anything existing in the outer world but they are forms of the human sensuality that order sensations. Space understood in this way provides a base for geometry and the pure intuition of time – the base for arithmetic and for the concept of number. Numbers are – according to Kant – partial *a priori* structures of reason:

> A number is the 'pure scheme' of the mind's concept of quantity, i.e., the imagination that grasps 'the successive addition of one to one'. (Kant 1781, pp. 142–143)

Consequently:

> arithmetical statements are synthetic *a priori*.

'Synthetic' means that concepts are connected with other concepts and in this way new concepts are obtained (they cannot be obtained analytically from the given initial concepts). "Hence our counting is a synthesis with respect to concepts" (Kant 1781, p. 78). Kant explains this on the example of $7 + 5 = 12$. The 12 is obtained as every number by successive addition of 1 – and it is contained neither in (the concept of) 5 nor in (the concept of) 7. Arithmetical statements as synthetic *a priori* are not empirical and – as arithmetical concepts – are objectively valid (Kant 1781, p. XVI). This validity assigns the nobility of knowledge to the (bare) experience.

Another standpoint represented **positivists** of the 19th century, among them **John Stuart Mill** (1806–1873) – it was in opposition to Kant's transcendental idealism (cf. Mill's *System of Logic*, 1843). Positivists attempted to develop their philosophy on the basis of "positive facts". The question about the nature of concepts and philosophical objects in general as well as the search for the first and true reasons were dismissed by them as "metaphysical".

John Stuart Mill attempted to found his theory of cognition on psychology, i.e., on the research of "positive facts" of the consciousness. Starting point were perceptions and the connections of perceptions. The task of *logic* was then to distinguish the constant perceptions from the elusive ones and contingent connections of perceptions from constant ones.

The process of building mathematical and arithmetical concepts was seen as an art of abstraction, i.e., they were obtained by omitting some properties of real objects and by generalizing and idealizing other properties. Numbers were cardinalities as abstractions of sets obtained by successive addition of units. Briefly:

> Numbers have their source and origin in the reality. Numbers are the result of successively appearing perceptions.

The exclusive cognition principle was induction, i.e., an inference from particular observations to a general statement. Arithmetical statements are not implied by definitions of numbers but are based on observed facts. Hence arithmetical and generally mathematical judgements could not be necessarily true. The task of mathematics is – according to Mill – to search for consequences of assumptions and the latter can be – in principle – arbitrary. Only the logical connections between assumptions and their consequences are necessary truths. So the task of mathematics is *not* to look for valid assumptions and on the basis of them to produce valid statements.

Elements of positivism one finds also in the marxist epistemology. Friedrich Engels claimed that (1878, Chapter III, p. 20):

> The concepts of a number and of a figure does come from the real world. Ten fingers with the help of which human beings are counting, hence the first arithmetical operation they learn, can be anything else but certainly not free creations of human reason.

This sentence has been quoted very often, in particular in materials for teachers in the former GDR in order to found the introduction of numbers as cardinal numbers (cf. Ministerium für Volksbildung 1988, p. 5). This quotation looks like a polemics with "bourgeois" conceptions of Cantor and Dedekind – we shall consider them just now.

## 4. Gauss, Cantor and Dedekind

One can say that C.F. Gauss stands at the beginning of modern mathematics. On the other hand his views concerning numbers were rather traditional. New contributions to the mathematical foundations of numbers due to G. Frege, G. Cantor, R. Dedekind and G. Peano appeared almost one century later.

**Carl Friedrich Gauss** (1777–1855) through his manifold mathematical activity changed the mathematical thinking dissolving mathematics from its philosophical ties. This happened in the era dominated by Kant's philosophy. The latter influenced first of all geometry – compare in particular the *a priori* form of the pure intuition of space which was assumed to be Euclidean. Gauss was the first who admitted a non-Euclidean model of geometry in which the axiom of parallels is not fulfilled. He released geometry from the ontological ties and challenged the ontological character of geometrical axioms.

Gauss' views towards numbers seem to be rather traditional and in fact go back to Book V of Euclid's *Elements*.

According to C.F. Gauss, objects of mathematics are "extensive quantities". Among them are "space or geometrical quantities, [...], time, number". He explained that "the proper objects of mathematics are relations between quantities" (Gauss 1973, vol. X, 13. Zur Metaphysik der Mathematik, p. 57–59). Numbers were for him abstractions of relations between quantities:

> "Numbers" indicate "how many times a directly given quantity should be repeated."

Gauss confirmed this opinion (coming from the beginning of the 19th century or even earlier) also at the end of his life (Wagner 1975, p. 155). But he noticed that "the arithmetical approach will gain advantage over the geometrical one" (cf. Wagner 1975, p. 155). The reason is our "method of counting that is much easier than that of the ancients" (cf. Wagner 1975, p. 155).

Though Gauss links his concept of a number to the concept of a quantity, he sees also the possibility of constructing the domain of numbers in a purely arithmetical way. On the one hand he has given the geometrical interpretation of complex numbers but on the other he did not treat the latter as a foundation of them. In a letter to a mathematician, psychologist and philosopher Max Drobisch from 14th August 1834 Gauss wrote: "The interpretation of complex quantities as relations between points on the plane is only an example of application of them and does not describe their proper nature – this should be still done. I presented my theory many times in lectures and I came to the conclusion that it can be easily formulated and contains nothing absurd." (Gauss 1973, vol. X/1, Briefwechsel [zum Fundamentalsatz der Algebra], p. 106). Gauss means here a theory about which also Wolfgang Bolyai asked him when he wrote: "I am awaiting the development of your theory of complex numbers [. . . ]" (a letter of W. Bolyai to Gauss from 18th January 1848, Gauss–Bolyai 1899, p. 129).[2] Since the construction of the domain of numbers begins with the construction of natural numbers, we have here a reference to the problem of the concept of a natural number independent of the concept of a quantity.

Considering **Georg Cantor**'s (1845–1918) views concerning numbers one finds elements of all three philosophical positions indicated at the beginning. He sees the concept of a number as a result of "twofold abstraction act" – he understands it as an "abandonment" from properties of objects and simultaneously as the reflection on the common properties of sets. A result of it is a "general concept" (*Allgemeinbegriff*) (Cantor 1895, p. 281*f*). He wrote:

> Since every single element $m$, if we abstract from its nature, becomes a "unit", the cardinal number $\bar{\bar{M}}$ is a definite aggregate composed of units, and this number has existence in our mind as an intellectual image or projection of the given aggregate $M$. (1932, p. 283)

[2]There are authors who attribute the interpretation of complex numbers as pairs of reals just to Gauss and to the year 1831 and in this way give him the priority before Hamilton (1837) (cf. Kline 1972, p. 776; Bell 1945, p. 179). They refer – without giving the proper sources – to two letters of Gauss and Bolayi from 1837 that cannot be found in (Gauss–Bolyai 1899). – The authors would like to thank Professor Olaf Neumann from University of Jena for indicating the appropriate places in Gauss' writings.

This recalls Euclid's "set consisting of units" and applies not only to finite but also to infinite numbers which Cantor discovered beyond the natural numbers. He called them "transfinite cardinal numbers". It is clear that a concept of a number that admits also infinite numbers necessarily eliminates an attachment to Kant's pure intuition of time. The transfinite cardinal numbers reject definitely Aristotle's dogma against the actual infinity. Cantor introduced cardinals into mathematics as actual infinite realities and defended them against acute mathematical and philosophical resistance and objections from theologians. He look for a support for his conceptions in the history of philosophy and found "points of contact" (Cantor 1883, p. 205) by Plato, Nicolaus von Kues and Bolzano (against Aristotle, Leibniz, Kant, supposedly also Gauss, Kronecker and many others).

Cantor's attitude towards concepts and statements was on the one hand *platonistic*. They are not invented by a mathematician: "Hypotheses non fingo" (Cantor 1895, motto).[3] He/she rather discovers them and describes: "[...] with respect to the contents of my works I was only a reporter and an official (*nur Berichterstatter und Beamter*)" (Cantor, 1884, in a letter to Mittag-Leffler, cf. Fraenkel 1932, p. 480). Hence Cantor defends in his transfinite cardinal numbers no bare subjective imaginations but ideas that are really given and that *cannot* be denied.

Besides the described platonistic realism in which numbers form the "trans-subjective" reality one finds by Cantor also a second rationalistic conception of numbers in which numbers form an "intrasubjective" or "immanent (intrinsic)" reality or – as in the quotation given above – exist "in our mind" (*in unserem Geiste*) (Cantor 1883, p. 181*f*). The intrinsic reality of mathematical concepts is – according to Cantor – a necessary condition of the possibility of a *pure*, or – as he prefers to say – of a *free mathematics*. The freedom of mathematics consists of being free from "commitment [...] to check them [i.e., concepts – remark of the authors] with respect to their transient reality" (1883, p. 182). In opposition to all other sciences, mathematics is "free of any metaphysical ties" (Cantor 1883, p. 182*f*).

Cantor's conception of numbers can be summarized in the following way:

> On the one hand numbers are given as ideal realities – independent of human thinking, and on the other they exist as projections of sets in our thinking and can be obtained by the process of abstraction.

Cantor and **Richard Dedekind** (1831–1916) exchanged lively ideas since

[3] It has been taken from Newton's *Philosophiae naturalis principia mathematica* what Cantor explicitly says. The second motto to the same work indicating Cantor's platonism is: "Neque enim leges intellectui aut rebus damus ad arbitrium nostrum, sed tanquan scribe fideles ab ipsius naturae voce latas et prolatas excipimus et describimus". Here Cantor gives no source. We have ascertained that the motto is a quotation from Francis Bacon's *Scripta in naturali et universali philosophia*, IV, 4: *Prodrimi sive anticipationes philosophiae secundae. Praefatio*.

1872 (cf. Cantor 1991, pp. 29–66; Cantor 1932, pp. 443–450). There seems to be an agreement between them with respect to the problem of the intrinsic existence of mathematical concepts in mind. By Dedekind there appears an additional psychological component. Arithmetical truths are – according to Dedekind – "never *given* by inner consciousness" [emphasis of the authors] but "*gained* by a more or less complete repetition of the individual inferences" (Dedekind 1888, p. V). The reference to mathematical inference steps describes Dedekind in his famous work *Was sind und was sollen die Zahlen?* (1888). And he continues:

> So from the time of birth, continually and in increasing measure we are led to relate things to things and thus to use that faculty of the mind on which the creation of numbers depends. (p. V)

The creation of numbers describes Dedekind in the following way (1888, Section 73):

> If in the consideration of a simply infinite system $N$ set in order by a transformation $\varphi$ we entirely neglect the special character of the elements; simply retaining their distinguishability and taking into account only the relations to one another [...] then are these elements called *natural numbers* [...]. With the reference to this freeing the elements from every other content (abstraction) we are justified in calling numbers a free creation of the human mind.

"Simply infinite systems" describe the infinite process of counting in an infinite set (thought as actually given). Dedekind obtains (in a structuralistic way) from the structure of counting – which he described earlier by giving three appropriate axioms – a concept of a number and he sees just in the abstraction the core of "creating".

More clearly appears the idea of "mental creation" (*geistige Schöpfung*) in his work *Stetigkeit und irrationale Zahlen* (1872). Dedekind realizes that there are gaps in the domain of rational numbers between upper and lower classes forming *cuts*. Those gaps will be closed – according to him – by numbers that we are creating (Dedekind 1872, p. 13; English translation: p. 15):

> Whenever, then, we have to do with a cut $(A_1, A_2)$ produced by no rational number, we create a new, an *irrational* number $\alpha$, which we regard as completely defined by this cut $A_1, A_2)$; we shall say that the number $\alpha$ corresponds to this cut, or that it produces this cut.

Frege and Russell have shown later that the irrational numbers can be identified with cuts (even simpler, with their lower classes). Hence from the mathematical point of view one can avoid in this way the acts of creating. On the other hand, from the psychological point of view, it is difficult to treat cuts or lower classes of them as numbers.

When one considers the concept of a natural number then there are no concrete objects as cuts to which the natural numbers would correspond. The base is now a particular abstract structure and the imagination of abstract unidentified places in a series. This is the core of Dedekind's conception of a number:

Numbers are abstractions of places in an infinite counting series.

It is difficult to understand as “creation” the process of abstracting that creates numbers. We shall try to explain this concept.

Note first of all that to be able to apply a concept one needs a name, a symbol, a picture or a word for it. Dedekind describes the process of counting. One needs there signs (symbols) which represent not only single numbers but also the structure of them. Counting (objects) can be then reduced to a concept of ordering (those objects) with the help of a given particular series that should be “invented”. Elements of such a series can be called numbers – they represent abstract positions in the abstract structure of counting. One sees also how close a number and a symbol in this “structuralistic” concept of number are.

For Dedekind – similarly as for Cantor – infinite sets given as actual infinities were something obvious. He defined the concept of an infinite set and attempted to give an example of a concrete infinite set: “My world of thoughts, i.e., the totality of all things which can be objects of my thoughts, is infinite” (“Meine Gedankenwelt, d.h. die Gesamtheit aller Dinge, welche Gegenstand meines Denkens sein können, ist unendlich”) (Dedekind 1888, Section 66). From the mathematical point of view – inside set theory – such a psychological example is useless. The infinite process used here by Dedekind to build in his mind a chain of thoughts (a thought of a thought of a thought of . . . ) has been later mathematically reformulated by Zermelo and led to the infinity axiom in his system of set theory: There *exist* infinite sets!

**Giuseppe Peano** (1858–1932) attempted – similarly as Dedekind – to describe axiomatically the process of counting. In opposition to Dedekind he used only logical and arithmetical methods. His main aim in the work *Arithmetices principia nova methodo exposita* (1889a) was to develop arithmetic in a formal way using specific logical and arithmetical symbols invented by himself. He did not explain directly his views concerning the nature of numbers. Variables and constants used by Peano in his symbolism for the arithmetic of natural numbers were connected – as all his symbols – with *thoughts*. In 1896 he wrote: “In this way is established a one-to-one correspondence between thoughts and symbols, a correspondence that cannot be found in the colloquial language” (p. 572). Symbols represent by Peano not real ideas or intellectual creations but intellectual objects in the sense of “intrinsic realities” of Cantor.

## 5. Logicism, Intuitionism, Formalism

The modern philosophy of mathematics begins with the attempts to move the task of building a firm foundations of mathematics – and simultaneously the classical philosophy of mathematics – to mathematics itself. It took place in mathematical logic and set theory – in the development of them took part Bolzano, Peano, Frege, Dedekind and Cantor. One wanted to found on them the arithmetic of natural numbers and further also the theory of reals (hence the analysis as well).

There are three main trends in the modern philosophy of mathematics: logicism, intuitionism, formalism. We shall describe what did they claim with respect to natural numbers.

The main representatives of the logicism were **Gottlob Frege** (1848–1925) and **Bertrand Russell** (1872–1970). The idea of Frege – developed further by Russell together with **Alfred North Whitehead** (1861–1947) in the famous *Principia Mathematica* – was to found numbers and statements about numbers on the basis of pure thinking, i.e., on logic alone. Such a system of logic has been developed by Frege in his epochal *Begriffsschrift* (1879). In *Grundlagen der Arithmetik* (1884) attempted Frege to develop arithmetic as a part of logic. This forced a necessity of building a new view on the concept of number. The philosophical attitude of Frege can be characterized as follows:

- antiempiricism: arithmetical propositions are not inductive generalizations,
- anticantianism: arithmetical propositions are not synthetic *a priori*,
- antiformalism: arithmetical propositions are not the result of manipulations on symbols made according to certain rules.

The antiempiricism of Frege implied also his antipsychologism: everything that is psychic was for him necessarily subjective. His aim was "to separate what is psychic from that what is logical as well as subjective from objective" (Frege 1884, p. ix). According to him the arithmetical propositions are *analytic* (because they can be reduced to logical propositions) and consequently they are *a priori*.

Frege treated numbers as cardinal numbers:

Numbers are cardinal numbers.

The latter were classes of equipolent sets and sets were understood as domains of concepts (Frege avoided to speak about sets – he wanted to eliminate all extralogical concepts). Concepts were treated by Frege as elements of pure thinking, hence as objects of logic. Consequently:

numbers are elements of logic,

i.e., of pure thinking. Concepts exist – according to Frege who took here the platonic attitude – independently of space, time and human mind. Note that Frege – contrary to Dedekind and Peano who took the structuralistic position – attempted to explain finally the *ontological status of numbers* by *mathematical* means.

In *Grundgesetze der Arithmetik. I, II* (1893 and 1903) Frege developed arithmetic of natural numbers along those lines. In 1901 Russell discovered in Frege's system of logic the famous Russell's antinomy, i.e., an unsolvable contradiction. Later Russell and Whitehead developed in *Principia Mathematica*, vol. 1, 2, 3 (1910, 1912 and 1913) a new system of logic in which this antinomy could be eliminated. *Principia* are written in a symbolic language built on the basis of ideas of Peano. To prove that for every natural number there exists its successor, Russell and Whitehead had to use the axiom of infinity which in fact has a set-theoretical (and not logical) character. Consequently the reduction of numbers undertook in *Principia* was the reduction not to logic itself but to logic and set theory. And this is the main thesis of the contemporary logicism.

Intuitionism was developed in opposition to logicism on the one hand and – on the other – to the practice of unbounded and careless usage of the infinity in mathematics which was the case by Cantor and formalists (see below). Its characteristic ideas can be seen already by **Leopold Kronecker** (1823–1891) and his pupils in the 1870s and 1880s. In his *Über den Zahlbegriff* (1887) formulated Kronecker the idea of an "arithmetization" of algebra and analysis that should be founded on the intuition of a natural number. His views on natural numbers were soundly summarized in his famous remark made at a scientific meeting in Berlin in 1886: "The natural numbers were made by God, all others is a product of a man" ("Die ganzen Zahlen hat der liebe Gott gemacht, alles andere ist Menschenwerk").

Mathematics is – according to Kronecker – "abstraction of the arithmetical reality" (1887, p. 342). His concept of a series of natural numbers was quite different than that of his contemporaries Dedekind and Frege. This series was in fact only potentially infinite (and not actually infinite), moreover, existential statements on natural numbers were permissible only when their "pure existence", i.e., the constructibility of necessary numbers could be proven. Consequently a definition of a natural number is correct only if it can be decided in a finite number of steps whether any given number fulfills this property (definition) or not.

The proper founder of the intuitionism was **Luitzen Egbertus Jan Brouwer** (1881–1966). **Hermann Weyl** (1885–1955) was during some time an adherent of Brouwer's ideas (in a modified version) and contributed to the propagation of his conceptions. **Arend Heyting** (1898–1980), the pupil of Brouwer, popularized his ideas and attempted to explain them in the language usually used in the reflection on mathematics (note that Brouwer used often a language full of mystical elements). According to intuitionism mathematics is a function of the human intellect and a free activity of the human mind, it is a creation of the mind and not a theory or a system of rules and theorems. The basis of mathematics – and of the whole thinking – is the fundamental pre-intuition of a natural number which is founded on the intuition of *a priori* time. Similarly as by Kant, mathematical propositions are synthetic *a priori* judgements. Brouwer spoke about natural numbers as about "*unsubstantial sensation of time*" (1912, p. 185).

**Paul Lorenzen** (1915–1994) attempted to link the formalism of Hilbert to the intuitionistic critique of Brouwer by replacing the psychologistic way of describing the series of natural numbers by some rules of constructing the sign used by counting. **Christian Thiel** (born 1938) describes them in his book *Philosophie und Mathematik* (1995, p. 114) in the following way:

$$\mathbb{N} \quad \begin{cases} & \Longrightarrow \quad | \\ n & \Longrightarrow \quad n| \end{cases}$$

By those simple rules $\mathbb{N}$ one gets *counting signs*

$|, ||, |||, ||||, \ldots ,$

which were put at the beginning of Hilbert's considerations (see below). Thiel makes one step more and moves from those counting signs to "fictitious objects", to "numbers" (Thiel 1995, p. 135). He says that the counting signs $n$ and $n'$ – belonging to the same or to different systems of counting signs – are "equivalent with respect to counting" if one considers only the steps of constructing $n$ and $n'$, resp., and if the results of constructions of $n$ and $n'$ coincide (Thiel 1995, p. 115). One does not take here into account anything that is specific in a given system of signs and this allows to speak about the number $n$ and to say that $n$ and $n'$ *represent the same number*. This process leads as a "purely logical procedure" (Thiel 1995, p. 131) from counting signs to numbers and to arithmetical statements that hold in all systems of counting signs. Briefly:

> Numbers are fictitious objects which one gets by abstraction from counting signs in various systems of counting signs.

Formalism was in a sense a reaction to the radical intuitionistic critique. **David Hilbert** (1862–1943) claimed that (1926, p. 161 and p. 163; English translation: p. 369 and pp. 370–371:

> [. . . ] the significance of the *infinite* for mathematics had not yet been completely clarified

and

> the definite clarification of the *nature of the infinite* has become necessary, not merely for the special interests of the individual sciences but for the *honor of human understanding* itself.

Formalism is an attempt to clarify and to provide the foundations of mathematics by formalization, i.e., by reducing mathematics to symbols and operations on them. This was the method of realizing *the Hilbert's program*. The formal domain of symbols and sequences of symbols was treated by Hilbert as "finitistic" and he proposed to found on them in a "finitistic" way the "infinitistic" mathematics in which the actual infinity is used. This should be applied especially to analysis and its "infinitistic" concepts. What "finitistic" means has been never precisely explained by Hilbert.

In this "finitistic" foundations of mathematics the important role was played by natural numbers. In the lecture *Über das Unendliche* (1925, published in 1926) (from which also the quotations given above are taken) Hilbert said (p. 171; English translation: p. 377):

> In number theory we have the numerals
>
> $$1, 11, 111, 11111,$$
>
> each numeral being perfectly perceptually recognizable by the fact that in it 1 is always again followed by 1. These numerals, which are the object of our consideration, have no meaning at all in themselves.

Hence numbers as symbols are for Hilbert concrete and directly given and are in fact nothing more than symbols. Briefly:

> Numbers are meaningless symbols.

One finds by Hilbert no further explanation of the nature of numbers. At another place (Hilbert 1922, p. 18) admits Hilbert "his firm philosophical conviction that is essential for the foundation of mathematics – and of all scientific thinking, understanding and communicating – namely: At the beginning [...] there is a symbol". Weyl was later of the same opinion. In 1940 he said (Weyl 1940, p. 443): "The power of science [...] rests upon the combination of *a priori* symbolic construction with systematic experience [...]". Earlier he characterized the status of symbols in the following way (p. 442): "We now come to the decisive step of mathematical abstraction: we forget about what the symbols stand for." Those meaningless symbols are – according to Weyl (1948) – the end of the chain of the reduction of our knowledge: "We are left with our symbols" (from the *Nachlass* of Weyl, quoted according to Beisswanger 1965, p. 146).

The reduction to the domain of finite sequences of symbols should justify and found the concept of the actual infinity in mathematics. The actual infinity is an idealized concept that Hilbert considered to be indispensable and necessary and – on the other hand – neither constructible nor represented by anything in the real world. Note that formalization was for Hilbert a metamathematical method of justifying mathematics and that he never claimed that mathematics *is* a game of symbols (cf. Jahnke 1990, p. 168*ff*).

Symbols used by Hilbert are similar to those used by Lorenzen and Thiel in their operative approach as well as to those used by Weyl – but they played completely different role.

The symbols by Hilbert and Lorenzen were constructed from the signs for units and referred to the archaic signs as scores, balls, fingers used by counting. Those objects represented other objects in the process of counting and consequently they played the role of symbols. A sign is meaningless since it denotes (and means) itself and only itself. It is simultaneously an object and a sign. And at this point the views of Lorenzen and Hilbert differ. Lorenzen manipulates with symbols as objects and gets in this way in his calculus new objects, Thiel introduces then an operation of an arithmetical abstraction on them. Hilbert on the other hand understood symbols as being characterized by their interpretations.

**Kurt Gödel** (1906–1978) showed in his *incompleteness theorems* (1931) that Hilbert's program cannot be realized in his original formulation. He proved that for every consistent theory extending the arithmetic of natural numbers there are sentences which are true (in the intended model) but which cannot be proved (decided) by means of the considered theory. The proof of this theorem uses (in a certain artificial and sophisticated) way natural numbers – with the help of an arithmetization of syntax one can speak inside a theory about this theory itself.

Let us make also a short remark on a current trend in the philosophy of mathematics – namely on structuralism (being developed mainly in the USA). We cannot consider here the technical details – so we shall describe only what is said there about natural numbers. We take into account conceptions developed by **Geoffrey Hellmann** (1989), **Michael Resnik** (1997) and **Stuart Shapiro** (1997).

In the middle of 1950s a group of French mathematicians attempted to order and classify all mathematical fields and disciplines on the basis of some fundamental structures. This is the starting point of structuralism that claims that mathematics is a structure of structures. The smallest units there are the so called "patterns". Mathematical objects, in particular numbers, are positions in such patterns. This recalls the description of numbers given by Dedekind – numbers were there simply positions in particular series. What is new here is the treatment of the nature of numbers – whereas Dedekind spoke about "mental creations" and by Cantor numbers were ideal and simultaneously real objects outside our thinking, so in structuralism they possess only "modal existence". Hellmann suggests to treat mathematics and in particular arithmetic, only as a nominalistic theory the concepts of which are only names and symbols and hence have no meaning. So summing up one can say:

> Numbers are positions in patterns. They have a "modal" existence and no meaning.

The "modal existence" of numbers and of other mathematical objects is shown by using second-order logic and modal operators of second-order modal logic. Sophisticated technical methods are applied here and in this way structuralism becomes a bit artificial (in a certain sense).

## 6. Genetical Positions

Finally we present two further important contributions to the concept of number. Both come from very different fields, from psychology and cultural anthropology – the latter formulated inside the modern philosophical anthropology (Dux *et al*. 1994).

The psychological contribution originates in the school of **Jean Piaget** (1896–1980), the important researcher in psychological genetics. Piaget's concept of number was influenced by the logistical attempts of Frege and Russell. At the same time he reprehended their one-sided cardinal concept of number. He himself explicated the nature of number as a complex and inseparable connection of cardinal classification and ordinal succession (while he considered the latter as a logical operation). He wrote in (1971, p. 208):

> Finite numbers are at the same time cardinal and ordinal numbers; this follows from the nature of number itself, which is a single system of classes and asymmetric relations melted into an operational totality.

From this – mathematically not precise – position Piaget criticized "the often artificial deduction, [...] which separated logical research in a fundamental way from the psychological analysis, though both are qualified for relying on each other as mathematics and experimental physics are" (Piaget 1971, p. 11). In comparison with this we recall the deeply anti-psychological attitude of Frege.

Piaget's conception of number itself is not so meaningful as his idea of the development which is basic for his conception. Fundamentals of psychological development are the biological organization of human beings and their empirical approach to the world which is simultaneously formative and adapting. According to Piaget numbers are in each case outcomes of individual constructions and new creations which arise from practical acts by abstracting conceptions from them. (The idea of development presented in a similarly clear way can be found only in the paper of Dedekind *Was sind und was sollen die Zahlen?* which was obviously unknown to Piaget.) Piaget gained and proved his assumptions about numbers and about the stages of development by his famous experiments in which children were observed in precisely arranged tests.

Piaget's conception of number can be summarized as follows:

> Numbers are finite cardinals possessing also ordinal features. They are individual cognitive constructions having their source in acts performed on concrete objects.

There is something striking in Piaget's conception of number: though stressing ordinal qualities in his conception of number, counting appears in it only as an epiphenomenon. Counting has the subordinated position and rôle in the development of numbers (Piaget 1971, p. 100 [translated by the authors]):

> We must not dwell upon the latter [i.e., counting – annotation by the authors] for the object of this book is the analysis of the formation of the concept of number.

Piaget did not notice counting for itself as a schema of concept formation – as we found it in the conception of Dedekind who described it mathematically as the structure of the natural numbers.

The conception formulated in the framework of the cultural anthropology regards development additionally in a historical way. **Peter Damerow** (born 1939) makes an attempt to locate the concept of number between ontogenesis and histogenesis. His “constitutive basic assumptions” are (Damerow 1994, p. 271)

> first, logical-mathematical concepts are abstracted invariants and transformations which are realized by acts, and
>
> second, such abstractions are passed down by collective external representations [. . . ].

With respect to the first basic assumption Damerow assumed “that the theoretical reconstruction of the development of the concept of number by Piaget in ontogenesis reflects this process in a basically correct way” (p. 255).

Damerow regards ontogenesis and histogenesis as fundamentally different processes: the one is not the analogue of the other. External representations, for instance systems of arithmetical symbols, are snapshots in histogenesis. On the one hand they are results of psychological *constructions* performed by single individuals inside their *social* history. On the other hand they are objects of ontogenetical *reconstruction* made by individuals at any time.

Damerow reflects upon the development of the concept of number in conjunction with so called “arithmetical activities”. These are “operations of comparing, correspondence, aggregation and iteration” (p. 280). In a zeroth “pre-arithmetical level” there are no such activities in the proper sense. Here they are bounded to concrete situations and objects. Collective external representations as symbols and systems of symbols do not exist at all.

In the following “proto-arithmetical level” (p. 285) one can find “counting objects” as objective symbols for real objects – like calculi and notches, then words or symbolic acts in standard “counting successions ” (p. 286) expressing quantities by iteration. In the second level of “symbolic arithmetic” (p. 293) there appear contextual and abstract symbols as representatives of the second order which are made up for symbolization and calculation as well. Technical terms occur interacting with symbols and objects.

Arithmetical techniques build a new area of activities from which the third and last level in the development, the “level of theoretic arithmetic” was achieved (p. 302). This means a field of statements about abstract numbers within deductive systems which are obtained only by mental operations. If deductions need arithmetical interpretations then they are “linguistic”. If they are removed from these in systems of axioms which can be interpreted in optional ways, then they are “formal”.

Damerow regards his approach as a background for the theoretically undeterminable (p. 314; cf. Damerow 2001, p. 51)

> respond to the question about the historical or nonhistorical nature of logical-mathematical and particularly arithmetical thinking.

He speaks about the potential “part of arithmetical thinking [. . . ] traced back to cultural acquirements” and about the potential “extent [. . . ] where structures and processes in arithmetical thinking are historically unchangeable universalia in the nature of homo sapiens”. He regards historical external representations as the key for “historical-cultural comparative” analysis which are able to give answers in this area.

The concept of number in Damerow’s considerations is affected by its commitment to Piaget. It is not easy to outline his complex conception of number:

> Numbers are finite cardinals possessing ordinal features. Numbers are at the same time individual and social cognitive constructions which cultivate or which have been cultivated in the psychological development of individuals and in the historical development of societies. External representations are linking individual, social and historical development. The concept of number includes nonhistorical universal and historical cultural units.

We observe that Damerow, though he postulates fundamental counting structures as necessarily belonging to the concept of number, does not study their origin and early development. Counting structures seems to spring up from systems of operations which are a part of the natural environment of human beings and precede the specific arithmetical activities. Potentially they belong to “logical-mathematical universalia of thinking” .

## 7. The Evolutionary Position

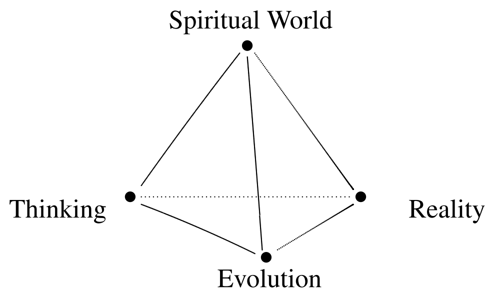
The positions of genetical psychology and cultural anthropology are exceedingly important for didactics of mathematics. It seems that in the philosophy of mathematics scarcely anybody takes note of such positions.

We note that both of these positions trace back to the theory or better to the discovery of evolution which not only altered biology but also radically challenged philosophy. The idea of evolution is a turning point for epistemology. It calls old fundamental positions into question. We will try to characterize shortly this new position. This will also provide us with a look at our object of investigation, namely at the concept of number.

At first we have to extend our picture of the fundamental positions – see below.

If one takes the position of the evolution then formations of thinking collapse – in them thinking attempted to solve its mysteries referring solely to itself. For the first time in the history of philosophy this new position provides the opportunity of looking at the development of thinking just as an external object

– it is done by studying biological and psychological development of human beings.




The “biological” or “evolutionary epistemology” was founded by **Konrad Lorenz** (1903–1989) and is represented today for instance by Gerhard Vollmer and Rupert Riedl. One of its key messages is:

– Life is a process leading to knowledge. This process continuously leads from the evolution of molecules up to civilization.

From this one obtains that:

– Thinking is like a higher organ formed in a “second step of evolution”. Structures of thinking are copies of natural patterns.
– Knowledge and insight (in its classical meaning) is actual reflection of reality inside structures of thinking.

The problem of knowledge (cognition) in the philosophy becomes a biological, anthropological, historical and finally a psychological and sociological problem.

Life is a process of getting knowledge. From the evolutionary point of view the old fundamental positions amalgamate. Thinking, spirit and reality are phenomena of one and the same life. The first job is to study evolution and the development of human beings. This can in principle lead to a solution of the old problems of philosophy. The artificial separation of matter and spirit, subject and object, thinking and being, idea and reality principally disappears.

What does it mean to adopt the position of the biological evolution in relation to the old fundamental positions?

– Evolutionary epistemology is *empiristic*.

For biology belongs to natural sciences. In its own structures thinking is reflecting structures of the world which in this process come first.

– Evolutionary epistemology is *anti-rationalistic*.

Thinking is the product of nature. Therefore it matches with natural objects and procedures. Any rationalistic remainder isolating thinking and knowledge from objects disappears. Objects of the world are directly given to thinking by its evolutionary biography. The so called "*Ding an sich*" (Kant) is a metaphysical nonsense.

– Evolutionary epistemology is *anti-idealistic*.

Evolution is managed by external processes: chance, mutation, accommodation, selection. Higher spiritual principles guiding evolution are unneeded and absurd. In fact there exists for the first time the possibility of monitoring the development of spirit – and of defining it. Attempts of definitions made by Dux (1994) for instance remain necessarily inexact. For example: Spirit is the medium between acquiring and designing environments (cf. p. 175). Or: "Which is being developed as spirituality, in fact is linked to the functionality of the central nervous system" (p. 108).

It is interesting to check the range of the new position on the simple example of numbers. What consequences for numbers, more precisely: for their nonhistoric universal part, has the thesis that they are the product of evolution? We state:

– Certainly numbers are not any longer "made by God". They are mainly products of the evolution and from now on caused genetically.

This position is very popular today.

– Does there exist anything like "number center" or "calculation (computing) center" in human brain, the development of which could be observed along the line of human ancestors?
– Do number genes exist?

The second question is undecided, the first one is temporarily answered in a negative way.

Evolution is a process proceeding in the course of time, it is a random process. The following question arises:

– Did the development of numbers proceed in the random way too? Can one imagine that if the genetical and anthropological development of human beings had proceeded in a different way, numbers could have been different than 1, 2, 3, … ?

One would rather think that the answer is: No. So where is the source of numbers in this case? This question is connected with the limits of the theory of evolution:

– Up to which point one can follow numbers and their origins through pre-

history and early history, evolution or cosmological progression, respectively?
- To ask the question about the evolution of numbers means also to ask the question about their temporality. This question may be absurd if the concept of number cannot be separated from time.
- Are numbers objects that must be thought of being independent of the evolution?
- Finally: is it necessary to accept a spiritual origin or a given structure of evolution. Are numbers elements of this structure?

Are numbers ideas, forms, divine thoughts? Though the position of evolution is revolutionary and very important, the simple question on the evolution of numbers seems to refer back to the good old philosophy.

## 8. Our Conception

At first we try to survey and to order the broad spectrum of conceptions about numbers leading from mystic till insignificance. Doing this we strongly simplify and concentrate on few and basic aspects of the conceptions. The following diagram should not be treated as a classification. We will find some conceptions in pretendedly conflicting places. This diagram should be seen as an attempt.

Read the following table in this way: For example "numbers are in the conception of *Dedekind* (see blocks inside) on the one hand *structurally* (see entering column) founded, on the other hand *nonhistoric, universal* (see entering row)".

| | historic | nonhistoric, universal |
|---|---|---|
| transcendent | — | Pythagoras, Plato, Nicolaus of Kues, Aristotle |
| transcendental | — | Kant, intuitionism |
| abstractions | Mill, Damerow | Euclid, Aristotle, Cantor, Piaget, Frege |
| formal | — | Frege, Hilbert, Peano |
| semiotic | Damerow | Hilbert, Weyl, Lorenzen, Thiel |
| structural | — | Dedekind, Peano |
| genetic | Damerow | Dedekind, Piaget, Damerow |

We try to find *our own position* in relation to these conceptions.

We not only notice the genetic positions but also integrate these positions in the case of looking at the *structure of natural numbers*.

Like Dedekind we firstly regard numbers as *counting numbers* (to use a didactic expression). These are

> digits in elementary counting structures,

i.e., elements in very elementary, finite cognitive pattern necessarily connected with internal or external representations. To understand this we have to try to explain the meaning of the process of counting. In Bedürftig and Murawski (2001) we tried to do this in a mathematical way – even regarding the most elementary finite cases.

Even simple counting shows something very fundamental: *the ability of building sequences*. Counting generally is

> the concept of sequence – by a self-conscious, actual sequence.

Behind this there is the ability to put digits or intervals in time – consciously re- and anticipating what is a precondition to conscious actions at all. Such digits may be linked with acts, perceptions, points in space and finally with symbols. This ability is being developed psychologically and historically as well. Systems of symbols are transformed and reconstructed. One can say:

> Counting in standardized sequences of number symbols is the external and collective representation of the concept of sequence.

*In principle* the inner construction of a pattern of counting is independent from abstractions from reality and applications at all. In this context we may speak of *pure counting*. We think that this construction is connected with inner time like Kant's form of pure intuition. Arithmetical matters are *a priori* and analytic for one can reduce them to simplest principles of pure counting. Like Dedekind and Cantor we regard numbers as "mental creations" and "intrinsic realities" arising from psychological constructions of schemata of counting.

*In the real psychological, prehistoric and historic development* are the internal constructions of counting and of numbers from the very beginning generally connected with the interchange with external environment. Such internal constructions *a priori* are instruments of individuals helping to acquire and to master the outside world. Dealing with objects in the outside world the schemata of counting are induced to be formed, organized and differentiated.

In this process the inner constructions of numbers cross abstractions, which give numbers the sense of cardinals, ordinals, quantities or operators (functions). In this way inner numbers get external reflections. They become manifest and need external representations by symbols that influence the inner constructions.

By internal and external representations the counting can refer to itself:

> Numbers count numbers.

This situation is characteristic for the interaction between counting and numbers. It is a fundamental condition of the development of arithmetic.

Our conception is evidently influenced by the genetic positions. But we do not accept the radical position of biologic epistemology which rejects other fundamental positions in philosophy.

Our position is *rationalistic* if it treats the construction of inner schemata of counting. It is *empiristic* by emphasizing external conditions *ab initio* in the formation of these schemata.

We tend to regard numbers treated as *counting numbers* as nonhistoric universalia. Counting numbers arise from systems of acts which come before "arithmetic activities" (according to Damerow, see above). In our opinion the psychological, prehistorical and historical development is also oriented towards numbers as universalia. We are not able to decide this.

What about ideas (Plato) and forms (Aristotle)? Do inner numbers impose a structure to the external world? Does there exist something like "external numbers" independent of thinking, corresponding to inner numbers? Do such external numbers on their part impose a structure upon the material world and determine the development of counting and numbers? Well, these are meta-metaphysical questions. We tend to a view which integrates aristotelian, platonistic and rationalistic elements and accepts numbers as inner and outer forms as well. In the interplay between inner construction, application to external forms and retroaction to inner construction we see impulses to the development of numbers and influences on the interpretation of the external world.

We do not regard numbers as pure symbols or as abstractions from them. The inner construction of schemata of counting depends on representations by internal and external symbols. In addition abstraction from the variety of symbols is needed which completes the concept of number. It appears to us that internal schemata of counting and external representations by symbols are connected to each other. Here we see an important interface between internal and external reality.

Finally we regard numbers not only as abstractions (implying ordinal features), which arise in connection with arithmetical activities (according to Damerow). The reason is that the structural ordinal aspect of numbers, which is represented in the process of counting and which is fundamental to the concept of number, goes back beyond these arithmetical activities. From the very beginning those acts are successions of acts. Therefore the ordinal structure of numbers as cardinals being abstracted from arithmetical activities is not a consequence of abstraction. This structure is a precondition for systems of arithmetical and already for "prearithmetical" systems of acts. The most elementary concept behind this is "counting".

Our conception therefore implies the necessity of widening the meaning of "arithmetical", for counting and "counting numbers" are arithmetical in their proper sense. These enables us to deduce the whole of arithmetic – without any additional ingredients.

## 9. Questions for the Philosophy of Mathematics

We already noticed that in the modern philosophy of mathematics only scant or no attention is given to the genetic conceptions of numbers. There are some reasons for this abstinence.

Metamathematics and philosophy of mathematics study mathematical theories taking infinite sets as granted. The task is to investigate and establish infinite concepts and methods in usual and successful infinitesimal mathematics. Conceptions trying to explain the origin of the elementary concept of number are far from this world of the infinite.

The reflection on the concept of number is usually begun in the philosophy of mathematics with natural numbers. Natural numbers are characterized and based transfinitely. Either a set theory postulating the axiom of infinity (natural numbers form a set) forms the basis or one deals with Peano-arithmetic and its models which necessarily are infinite. This is the starting point. At the same time this is the point where the development of the elementary concept of numbers ends.

In addition to that we find platonistic positions about mathematical concepts. According to them the latter are nonhistoric universalia determining any psychological and historical development. Only their selection and their linguistic occurrence lie in the psychological, social and historical sphere of influence, which is irrelevant to mathematics. Frege's anti-psychological attitude is still actual.

Is the development of the concept of number and of the elementary arithmetic *necessarily* beyond the interest of philosophy of mathematics? We do not think so.

We think that it is not adequate to connect numbers that are in fact finite, with the infinity. In the textbook quoted above we choose a finite basis (NBG set theory without the axiom of infinity) and consider counting and numbers in elementary finite structures (Bedürftig and Murawski 2001, p. 9). Our approach is similar to that of Dedekind (1888).

Models of such finite counting structures are usually not isomorphic as it is the case with models of natural numbers fulfilling Peano axioms and given in a set theoretical version. Instead of isomorphism we get the isomorphic embedding of models of such finite structures (p. 68). We regard this as an adequate alternative.

Finite counting structures allow us to develop elementary arithmetic in a mathematical way going along with psychological development in stages. That is the didactic sense and aim of our approach.

A pure logical axiomatic for finite arithmetical structures similar to Peano arithmetic is not possible. It would be also uninteresting for it would only record arithmetical states and not trace any development of them.

It appears to us that a finite set theoretical characterization of finite numbers and its arithmetical progression is of interest to the philosophy of mathematics. Finite mathematical positions are possible and give the possibility to take into account the development of the concept of number and to discuss the concept of number in a new and advanced way. A well-founded finite basis enables us to follow the structural development of numbers from the finite to the infinite level. One can find examples of this in Chapter 4 of our textbook – they show how we can get a new position facing the phenomenon of infinity (and finiteness). We quote a proposition from this textbook (Bedürftig and Murawski 2001, p. 200):

> THEOREM. *Let* $\langle N, \nu, 1\rangle$ *be an unary structure generated by* 1. *If it satisfies the property:*
>
> > *For any unary structure* $\langle A, \mu, a, p\rangle$ *there exists an homomorphism* $\beta : A \to N$ *with* $\beta(a) = 1$
>
> *then* $\langle N, \nu, 1\rangle$ *is a model of the Peano axioms.*

The colloquial meaning of this theorem is the following: Any counting schema, which is able to enumerate any finite set, has the structure of the natural numbers. The application of numbers as enumerating instruments to determinate quantities involves infinity.

## 10. Concluding Remarks

If we look back at the various conceptions of numbers we realize that there is no and there will never be a final clarification of the nature of numbers. Mathematics alone cannot (and perhaps should not) clarify it. On the other hand mathematics can be developed without clarifying the essence of the concept of number, moreover, we can understand quite well the concepts of our predecessors and claim that our conception is similar to theirs. Does it mean that the philosophical reflection on the foundations of mathematics is irrelevant?

It seems to be the case when one considers the internal process of the development of mathematics in an isolation. On the other hand it is certainly not irrelevant from the point of view of teaching mathematics and when one considers the *meaning* of mathematics as a science. This meaning was understood in a different way in various periods. We tried to indicate the differences:

> *from* the numbers by Pythagoreans and the ideas of Plato that are metaphysical powers in the development of the world
>
> *through* the continuous quantities in the Greek theory of quantities
>
> *to* the actual infinite sets that deconstructed the quantities into elements and eliminated them from mathematics, *and finally*

> *to* meaningless symbols that generate today – as bits and bytes – virtual worlds and net the real world.

Conceptions – discussed at the end of our considerations – that are looking for the genesis of the concept of number, show how the seemingly meaningless symbols are in fact associated with the individual subject, world and history.

# 8

# CHURCH'S THESIS AND ITS EPISTEMOLOGICAL STATUS[1]

The aim of this paper is to present the origin of Church's thesis and the main arguments in favour of it as well as arguments against it. Further the general problem of the epistemological status of the thesis will be considered, in particular the problem whether it can be treated as a definition and whether it is provable or has a definite truth-value.

## 1. Origin and Various Formulations of Church's Thesis

The Church's thesis is one of the widely discussed statements in the recursion theory and computability theory. For the first time formulated in 1936, it still focuses interest of specialists in foundations and philosophy of computer science. It can be stated simply as the equation

$$\mathcal{O} = \mathcal{R},$$

where $\mathcal{O}$ denotes the class of all (effectively) computable functions and $\mathcal{R}$ the class of all recursive functions. So it says that

a function is (effectively) computable if and only if it is recursive.[2]

The central notion we should start with is the notion of an algorithm. By an algorithm we mean an effective and completely specified procedure for solving problems of a given type. Important is here that an algorithm does not require creativity, ingenuity or intuition (only the ability to recognize symbols is as-

[1] Originally published in *Annales Universitatis Mariae Curie-Skłodowska, Sectio AI, Informatica* 2 (2004), 57–70. Reprinted with kind permission of Wydawnictwo Uniwersytetu Marii Curie-Skłodowskiej (Publishing House of Maria Curie-Skłodowska University).

[2] The exact definition of recursive functions and properties of them can be found in various books devoted to the recursion theory – cf., e.g., Murawski (1999; 2000).

In: Roman Murawski, *Essays in the Philosophy and History of Logic and Mathematics* (*Poznań Studies in the Philosophy of the Sciences and the Humanities*, vol. 98), pp. 123–134. Amsterdam/New York, NY: Rodopi, 2010.

sumed) and that its application is prescribed in advance and does not depend upon any empirical or random factors.

A function $f : \mathbb{N}^k \longrightarrow \mathbb{N}$ is said to be effectively computable (shortly: computable) if and only if its values can be computed by an algorithm. So consequently a function $f : \mathbb{N}^k \longrightarrow \mathbb{N}$ is computable if and only if there exists a mechanical method by which for any $k$-tuple $a_1, \ldots, a_k$ of arguments the value $f(a_1, \ldots, a_k)$ can be calculated in a finite number of prescribed steps. Three facts should be stressed here:

- no actual human computability or empirically feasible computability is meant here,
- functions are treated extensionally, i.e., a function is identified with an appropriate set of ordered pairs; consequently the following function
$$f(x) = \begin{cases} 1, & \text{if Riemann's hypothesis is true,} \\ 0, & \text{otherwise,} \end{cases}$$
is computable since it is either the constant function 0 or the constant function 1 (so a classical notion of a function is assumed and not the intuitionistic one – see below),
- the concept of computability is a modal notion ("there exists a method", "a method is possible").

Many concrete algorithms have long been known in mathematics and logic. In any such case the fact that the alleged procedure is an algorithm is an intuitively obvious fact. A new situation appears when one wants to show that there is no algorithm for a given problem – a precise definition of an algorithm is needed now. This was the case of Gödel's incompleteness theorems and Hilbert's program of justification of classical mathematics by finitary methods *via* proof theory. Hence in the thirties of the last century some proposals to precise the notion of an effectively computable function appeared – there were the proposals by Alonzo Church (1936a), Emil Post (1936) and Alan Turing (1936–1937).

A natural problem of adequacy of those proposals arose. The positive answer to this question is known in the literature as Church's thesis (sometimes called also Church-Turing thesis). It was formulated for the first time by A. Church in (1936a) and appeared in the context of researches led by himself and his students (among them was S.C. Kleene) on the $\lambda$-definability. They studied computable functions asking whether they are $\lambda$-computable. In §7 of (1936a) Church wrote:

> We now define the notion, already discussed, of an effectively calculable function of positive integers by identifying it with the notion of a recursive function of positive integers (or of a $\lambda$-definable function of positive integers). This definition is thought to be justified by the considerations which follow, so far as positive justification can ever be obtained for the solution of a formal definition to correspond to an intuitive notion.

The very problem of the adequacy appeared in a conversation of Church and

Gödel – as has been explained by Church in (1936a) in the footnote where he wrote:

> The question of the relationship between effective calculability and recursiveness (which it is proposed to answer by identifying the two notions) was raised by Gödel in conversation with the author. The corresponding question of the relationship between effective calculability and $\lambda$-definablility had previously been proposed by the author independently.

Though published in 1936, the thesis was announced to the mathematical world by Church already on 19th April 1935 at a meeting of the American Mathematical Society in New York City in a ten minute contributed talk. In the abstract received by the Society on 22nd March 1935 (cf. Church 1935) one can read:

> Following a suggestion of Herbrand, but modifying it in an important respect, Gödel has proposed (in a set of lectures at Princeton, N.J., 1934) a definition of the term *recursive function*, in a very general sense. In this paper a definition of *recursive function of positive integers* which is essentially Gödel's is adopted. And it is maintained that the notion of an effectively calculable function of positive integers should be identified with that of a recursive function, since other plausible definitions of effective calculability turn out to yield notions which are either equivalent to or weaker than recursiveness.

Add that the term 'Church's thesis' appeared for the first time in S.C. Kleene's monograph (1952) (on page 317) published in 1952 (earlier, in the paper from 1943 he referred to it as 'Thesis I').

A thesis similar to Church's thesis was independently formulated by Alan Turing in (1936–1937). He introduced there what is called today Turing machines and stated that:

> The computable numbers include all numbers which could naturally be regarded as computable.

Explain that "computable numbers" are those reals whose decimal extension is computable by a Turing machine.

Later other formulations similar (and equivalent) to Church's thesis as well as some paraphrases of it were proposed. Let us mention here the formulation of Kleene (cf. Kleene 1943) where one finds Thesis I stating that:

> Every effectively calculable function (effectively decidable predicate) is general recursive.

A.A. Markov in (1955) formulated a principle which is equivalent to Church's thesis. It is formulated in the language of algorithms (in the precise sense of Markov!) and says that every algorithm in the alphabet $A$ is fully equivalent with respect to $A$ to a normal algorithm over $A$.

As examples of some paraphrases of the considered thesis let us quote Boolos and Jeffrey's (1980) where they write:

> [...] the set of functions computable in our sense [i.e., by a Turing machine – R.M.] is identical with the set of functions that men or machines would be able to compute by whatever

> effective method, if limitations on time, speed, and material were overcome (p. 20)

and

> [...] any mechanical routine for symbol manipulation can be carried out in effect by some Turing machine or other. (p. 52)

Some other paraphrases can be found in L. Kalmár (1959), G. Kreisel (1970) or in D.R. Hofstadter (1979).

## 2. Attempts to Justify Church's Thesis

The natural question that should be asked is the following: is Church's thesis true (correct, sound)?

Let us start by noticing that the thesis is widely accepted today. Within recursion theory a technique (called sometimes "argument by Church's thesis") has been developed. It consists in concluding that a function is recursive, or that a set is decidable, just because there is an algorithm for it. Examples of its applications can be found in the monograph by H. Rogers (1967).

Note also that Church's thesis has a bit different meaning when considered from the constructivist (or intuitionistic) position. Indeed, for a constructivist a formula $\forall x \exists! y \Phi(x, y)$ (hence a definition of a function) states that given an $x$ one *can find* a unique $y$ such that $\Phi(x, y)$. This amounts to an assertion that a certain function is computable. In consequence Church's thesis is the statement that if $\forall x \exists! y \Phi(x, y)$ then there is a *recursive function* $f$ such that $\forall x \Phi(x, f(x))$. Hence for a constructivist Church's thesis is a claim that *all* number-theoretic functions are recursive.

Church's thesis is a statement formulated rather in a colloquial language than in the precise language of mathematics – it is about the class of computable functions defined in the colloquial, pragmatic, pre-theoretic language using the imprecise notion of an algorithm or an effective mechanical method. Hence it seems that *a priori* no precise mathematical proof of this thesis can be expected. The only thing one can do is to try to indicate evidences confirming it (or arguments against it). All such arguments must, at least in part, be of a philosophical character – they cannot be purely mathematical arguments.

Since one cannot give a precise mathematical proof of the thesis of Church, let us look for arguments in favour of it (arguments against it will be indicated in the sequel). Further, since the inclusion $\mathcal{R} \subseteq \mathcal{O}$ is obvious, the essential part of Church's thesis is the inclusion $\mathcal{O} \subseteq \mathcal{R}$. The arguments in favour of it can be divided into three groups:

(A) heuristic arguments (no counterexample arguments),
(B) direct arguments,

(C) arguments based on the existence of various specifications of the notion of computability.

Among arguments of group (A) one can find the following ones:

($A_1$) all particular computable functions occurring (so far) in mathematics were shown to be recursive,

($A'_1$) no example of a computable function not being recursive was given,

($A_2$) it was shown that all particular methods of obtaining computable functions from given computable ones lead also from recursive functions to recursive functions,

($A'_2$) no example of a method leading from computable functions to computable functions but not from recursive functions to recursive ones was given.

One can easily see that the above arguments are in a certain sense of empirical (or quasi-empirical) character.

Arguments of group (B) consist of theoretical analysis of the process of computation and attempts to show in this way that only recursive functions can be computable. Such was the argumentation of Turing in (1936–1937) where a detailed analysis of the process of computing a value of a function was given and where the conclusion was formulated that any possible computation procedure has a faithful analogue in a Turing machine and that, therefore, every computable function is recursive.

Arguments of group (C) are based on the fact that in the second quarter of this century several somewhat different mathematical formulations of computability were given (more or less independently). All of them have been proven to be extensionally equivalent and equal to the class $\mathcal{R}$ of recursive functions. It suggests that their authors have the same intuitions connected with the notion of computability. The equality of all those classes of functions can serve as an argument in favour of the thesis that the class $\mathcal{R}$ comprises all computable functions.

All attempts to define the notion of computability in the language of mathematics can be classified in the following way:

(1) algebraical definitions – they consist of fixing certain initial functions and certain operations on functions. One considers then the smallest class of functions containing the initial functions and closed under the indicated operations;

(2) definitions using abstract mathematical machines – as an example of such a definition can serve the definition of the class of functions computable in the sense of Turing (cf. Turing 1936–1937);

(3) definitions using certain formal systems – an example of a definition of this sort is the definition of functions computable in the sense of Markov (i.e.,

of functions computable by normal Markov's algorithms, cf. Markov, 1955), Herbrand-Gödel-Kleene definition of computable functions (cf. Gödel 1934 and Kleene, 1952), definition of computability by representability in a formal system, the theory of $\lambda$-definability by Church (cf. Church 1941) or the theory of Post normal systems (cf. Post 1943).

Add also the following pragmatic argument due to M. Davis who writes in (2004, p. 207):

> The great success of modern computers as all-purpose algorithm-executing engines embodying Turing's universal computer in physical form, makes it extremely plausible that the abstract theory of computability gives the correct answer to the question "What is a computation?" and, by itself, makes the existence of any more general form of computation extremely doubtful.

Having considered arguments in favour of Church's thesis let us study now arguments against it. One can find various such arguments but, as Shapiro writes in (1981 (p. 354), "[they] seem to be supported only by their authors". We shall discuss here only arguments of Kalmár formulated by him in (1959).

Kalmár gives of course no example of a particular function which is not recursive but for which there exists a mechanical method of calculating its values. He shows only that Church's thesis implies certain peculiar consequences.

Let $F(k, x, y)$ be a ternary partially recursive function universal for binary recursive functions. Let $f(x, y)$ be its diagonalization, i.e., a function defined as:

$$f(x, y) = F(x, x, y).$$

The function $f$ is recursive. Consider now the following example of a nonrecursive function (given by Kleene – cf. Kleene 1936, Theorem XIV).

$$g(x) = \mu y\,(f(x, y) = 0) = \begin{cases} \text{the smallest } y \text{ such that} \\ f(x, y) = 0, \text{ if such } y \text{ exists,} \\ \\ 0, \text{ otherwise.} \end{cases}$$

The function $g$ is nonrecursive, hence by Church's thesis it is also noncomputable. On the other hand, for any natural number $p$ for which there exists a number $y$ such that $f(p, y) = 0$, there is a method of calculating the value of $g(p)$, i.e., of computing the smallest number $y$ such that $f(p, y) = 0$. Indeed, it suffices to compute successively $f(p, 0)$, $f(p, 1)$, $f(p, 2)$, … (each of them can be calculated in a finite number of steps since $f$ is recursive) till we get the value 0. For any number $p$ for which it can be proved (by any correct method) that there exists no $y$ such that $f(p, y) = 0$, we also have a method of calculating the value of $g(p)$ in a finite number of steps – it suffices simply to prove that there exists no number $y$ such that $f(p, y) = 0$ which requires a finite number of steps and leads to the result $g(p) = 0$. Hence assuming that the function $g$ is not computable and using *tertium non datur* (note that it was already used in the definition of $g$) one comes to the conclusion that there are natural numbers $p$ such that, on the one hand, there is no number $y$ with the property $f(p, y) = 0$ and, on the other hand, this fact cannot be proved by any correct means. So Church's thesis implies the existence of absolutely undecidable propositions which can be decided! An example of such a proposition is any sentence of the form $\exists y[f(p, y) = 0]$ where $p$ is a number for which there is no $y$ such that $f(p, y) = 0$. Hence it is an absolutely undecidable proposition which we can decide, for we know that it is false!

In the above considerations we used a somewhat imprecise notion of provability by any correct means. This imprecision can be removed by introducing a particular formal system (more exactly: a system of equations) and showing that the considered sentence cannot be proved in any consistent extension of this system.

But the situation is even more peculiar. Church's thesis not only implies that the existence of false sentences of the form

$$\exists y[f(p, y) = 0]$$

is absolutely undecidable, but also that the absolute undecidability of those sentences cannot be proved by any correct means. Indeed, if $P$ is a relation and the sentence $\exists yP$ is true then there exists a number $q$ such that $P(q)$. So the question: "Does there exist a $y$ such that $P(y)$?" is decidable and the answer is YES (because $P(q) \rightarrow \exists yP(y)$). Hence if the sentence $\exists yP(y)$ is undecidable then it is false. So if one proved the undecidability of the sentence $\exists yP(y)$ then one could also prove that $\neg\exists yP(y)$. Hence one would decide the undecidable sentence $\exists yP(y)$, which is a contradiction. Consequently, the undecidability of the sentence $\exists yP(y)$ cannot be proved by any correct means.

Kalmár comes to the following conclusion: "there are pre-mathematical notions which must remain pre-mathematical ones, for they cannot permit any restriction imposed by an exact mathematical definition" (cf. Kalmar 1959, p. 79). Notions of effective computability, of solvability, of provability by any correct means can serve here as examples.

Observe that one can treat the argumentation of Kalmár given above not as an argumentation against Church's thesis but as an argumentation against the law of excluded middle (*tertium non datur*) – this law played a crucial role in Kalmár's argumentation. So did, for example, Markov.

Arguments against Church's thesis were formulated also by R. Péter (1959), J. Porte (1960) and G.L. Bowie (1973). The latter claims that the notion of an effective computability is intensional whereas the concept of recursiveness is extensional. Hence they cannot be identical and consequently Church's thesis is possibly false. Note (in favour of the intensionality of the concept of computability) that the existence of a computation of (a value of) a function depends not only on the description of the function but also on the admissible notation for inputs and outputs.

## 3. Epistemological Status of Church's Thesis

Let us look now at Church's thesis from the epistemological point of view and ask: what does this thesis really mean, is it true or false or maybe has no truth-value at all, is this problem decidable, and if yes, then where and by which means can it be decided?

Start by noting that computability is a pragmatic, pre-theoretical concept. It refers to human (possibly idealized) abilities. Hence Church's thesis is connected with philosophical questions about relations between mathematics and material or psychic reality. There are scholars who treat the notion of computability as a notion of a psychological nature. For example E. Post wrote (cf. Post 1941, pp. 408 and 419):

> [. . . ] for full generality a complete analysis would have to be made of all the possible ways in which the human mind could set up finite processes for generating sequences. [. . . ] we have to do with a certain activity of the human mind as situated in the universe. As activity, this logico-mathematical process has certain temporal properties; as situated in the universe it has certain spatial properties.

On the other hand the concept of computability has modal character whereas the notion of a recursive function (or of a function computable by Turing machine) is not a modal one. Hence Church's thesis identifies the extension of an idealized, pragmatic and modal property of functions with the extension of a formal, precisely defined *prima facie* non-modal arithmetical property of functions. Consequently Church's thesis is a proposal to exchange modality for ontology.

In a philosophical tradition one has two approaches to this problem: (1) One of them, traced to W.V.O. Quine, is skeptical of modal notions altogether and suggests that they are too vague or indeterminate for respectable scientific (or quasi-scientific) use. On such a view Church's thesis has no definite truth-value, so it is neither true nor false. (2) Other tradition, while not so skeptical of modality as such, doubts that there can be any useful reduction of a modal notion to a non-modal one.

S. Shapiro (1993) claims that both those traditions should be rejected while considering Church's thesis. In this case the problem of modality is solved by assuming that Turing machines represent in a certain sense all possible algorithms or all possible machine programs and sequences of Turing machine configurations represent possible computations. Hence Church's thesis would hold only if, for every possible algorithm there is a Turing machine that represents an algorithm that computes the same function. So we have a thesis that the possibilities of computation are reflected accurately in a certain arithmetic or set-theoretic structure. Similarly for $\lambda$-definability and for recursive functions. This leads us to the so called Church's superthesis (formulated for the first time by G. Kreisel in (1969, p. 177) in the following way:

> [...] the evidence for Church's thesis, which refers to *results*, to functions computed, actually establishes more, a kind of *superthesis*: to each [...] algorithm [...] is assigned a [...] [Turing machine] program, modulo trivial conversions, which can be seen to define the same computation process as the [algorithm].

Pragmatic modal notions do not have sharp boundaries. There are usually borderline cases. Similar situation is of course also in the case of Church's thesis – recall the idealized character of an unequivocal notion of computability. In general vague properties cannot exactly coincide with a precise one. Since computability is a vague notion and recursiveness (and other equivalent notions) is a precise one, hence Church's thesis does not literally have a determinate truth-value or else it is false. Church's thesis might then be treated as a proposal that recursiveness be substituted for computability for certain purposes and in certain contexts.

Church's thesis is suited for establishing negative results about computability. When one shows that a given function is not recursive, then one can conclude that it cannot be computed. On the other hand, if the function has been shown to be recursive, then this gives no information on the feasibility of an algorithm and does not establish that the function can be calculated in any realistic sense.

Church's thesis can also be treated simply as a definition. If we treat it as a nominal definition then its acceptance (or rejection) is a matter of taste, convenience, etc. If accepted, it is vacuously true, if rejected, there is no substantive issue. Add however that such an approach to Church's thesis cannot be found in the literature – it is a subject of philosophical and mathematical studies what proves that it is not treated as a nominal definition only.

Note that Church proposing for the first time the thesis thought of it just as a definition. In fact in the paper (1936) he wrote:

> The purpose of the present paper is to propose a definition of effective calculability [...]. (p. 346)

Recall also his words from §7 of (1936) quoted above where he said:

> *We now define* [emphasis is mine – R.M.] the notion, already discussed, of an effectively calculable function of positive integers by identifying it with the notion of a recursive function [...]. (p. 357)

But it should be stressed that Church understood here the definition as a real one (and not as a nominal definition), i.e., as an explication or rational reconstruction.

Turing in (1936-1937) – though he did not use the term 'definition', but writing that he wants to show "that all computable numbers are [Turing] 'computable' " (p. 231) – clearly regarded it just as the definition of computability.

Also by Gödel one finds words which may suggest that he treated Church's thesis as a definition. In fact in (1946) he wrote:

> [...] one has for the first time succeeded in giving an absolute definition of an interesting

> epistemological notion, i.e. one not depending on the formalism chosen. (p. 1)

And in (1951) he wrote:

> The greatest improvement was made possible through the precise definition of the concept of finite procedure. [...] This concept, [...] is equivalent to the concept of a "computable function of integers" [...] The most satisfactory way, in my opinion, is that of reducing the concept of finite procedure to that of a machine with a finite number of parts, as has been done by the British mathematician Turing. (pp. 304–305)

E. Post treated Church's thesis as "a working hypothesis" and as "a fundamental discovery in the limitations of the mathematicizing power of Homo Sapiens" (1936, p. 105). He was convinced that in the process of confirming it can receive the status of "a natural law" (cf. Post 1936, p. 105). Add that he warned against treating Church's thesis as a definition because it would dispense us from the duty and necessity of looking for confirmations of it.

The standard approach to Church's thesis hold it to be a rational reconstruction (in the sense of R. Carnap and C. Hempel). A rational reconstruction is a precise, scientific concept that is offered as an equivalent of a prescientific, intuitive, imprecise notion. It is required here that in all cases in which the intuitive notion is definitely known to apply or not to apply, the rational reconstruction should yield the same outcome. In cases where the original notion is not determinate, the reconstruction may decide arbitrarily. Confirmation of the correctness of a rational reconstruction must involve, at least in part, an empirical investigation. And, what is more important, it cannot be proved. So was for example the opinion of Kleene who wrote in (1952, pp. 317–319):

> Since our original notion of effective calculability of a function (or of effective decidability of a predicate) is a somewhat vague intuitive one, the thesis [i.e., Church's thesis – R.M.] cannot be proved.
>
> [...]
>
> While we cannot prove Church's thesis, since its role is to delimit precisely a hitherto vaguely conceived totality, we require evidence that it cannot conflict with the intuitive notion which it is supposed to complete; i.e. we require evidence that every particular function which our intuitive notion would authenticate as effectively calculable is general recursive.

Similar was the opinion of L. Kalmár who wrote in (1959):

> Church's thesis is not a mathematical theorem which can be proved or disproved in the exact mathematical sense, for it states the identity of two notions only one of which is mathematically defined while the other is used by mathematicians without exact definition. (p. 72)

J. Folina claims in (1998) that Church's thesis is true (since "there is a good deal of convincing evidence" – cf. Folina 1998, p. 321) but that it is not and cannot be mathematically proved.

Talking about "mathematical provability" one should first clearly explain what does it exactly mean. This is stressed by S. Shapiro in (1993). He says that there exists of course no ZFC proof of Church's thesis, there is also no formal proof of it in a deductive system.

To prove (in a formal way) Church's thesis one should construct a formal system in which the concept of computability would be among primitive notions and which would be based (among others) on axioms characterizing this notion. The task would be then to show that computability characterized in such a way coincides with recursiveness. But another problem would appear now: namely the problem of showing that the adopted axioms for computability do in fact reflect exactly properties of (intuitively understood) computability. Hence we would arrive at Church's thesis again, though at another level and in another context.

On the other hand it should be stressed that in mathematics one has not only formal proofs, there are also other methods of justification accepted by mathematicians. Taking this into account E. Mendelson comes in (1990) to the conclusion that it is completely unwarranted to say that "CT [i.e., Church's thesis – R.M.] is unprovable just because it states an equivalence between a vague, imprecise notion (effectively computable function) and a precise mathematical notion (partial-recursive function)" (p. 232). And adds (p. 230):

> My viewpoint can be brought out clearly by arguing that CT is another in a long list of well-accepted mathematical and logical "theses" and that CT may be just as deserving of acceptance as those theses. Of course, these theses are not ordinarily called "theses", and that is just my point.

As a justification of his claim Mendelson considers several episodes from the history of mathematics, in particular the concept of a function, of truth, of logical validity and of limits. In fact, till the 19th century a function was tied to a rule for calculating it, generally by means of a formula. In 19th and 20th centuries mathematicians started to define a function as a set of ordered pairs satisfying appropriate conditions. The identification of those notions, i.e., of an intuitive notion and the precise set-theoretical one, can be called "Peano thesis". Similarly "Tarki's thesis" is the thesis identifying the intuitive notion of truth and the precise notion of truth given by Tarski. The intuitive notion of a limit widely used in mathematical analysis in the 18th century and then in the 19th century applied by A. Cauchy to define basic notions of the calculus has been given a precise form only by K. Weierstrass in the language of $\varepsilon - \delta$. There are many other such examples: the notion of a measure as an explication of area and volume, the definition of dimension in topology, the definiction of velocity as a derivative, etc. Mendelson argues that in fact the concepts and assumptions that support the notion of recursive function are no less vague and imprecise than the notion of effective computability. They are just more familiar and are part of a respectable theory with connections to other parts of logic and mathematics (and similarly for other theses quoted above). Further the claim that a proof connecting intuitive and precise notions is impossible is false. Observe that the half of Church's thesis, i.e., the inclusion $\mathcal{R} \subseteq \mathcal{O}$ is usually treated as obvious. Arguments are here similar to others used in mathematics (and one uses here two notions only one of which is precisely given). In mathematics and logic, proof is not the only way in which a statement comes to be accepted as true. Very often – and so was the case in the quoted examples – equivalences between intuitive notions and precise ones were simply "seen" to be true without proof or based on arguments which were mixtures of intuitive perceptions and standard logical and mathematical reasonings.

Note at the end that there are also theorists who regard Church's thesis as proved. So, for example, R. Gandy says in (1988) that Turing's direct argument that every algorithm can be simulated on a Turing machine proves a theorem. He regards this analysis to be as convincing as typical mathematical work.

## 4. Conclusions

The above survey of opinions on Church's thesis shows that there is no common agreement about its epistemological status. The crucial point are always the philosophical presuppositions concerning, for example, the nature of mathematics and of a mathematical proof. The situation can be summarized as follows: there are some arguments and evidences in favour of Church's thesis (arguments against it are weaker), hence there are reasons to believe it is true. On the other hand there is no (and there cannot exist any) formal proof (on the basis of, say, Zermelo-Fraenkel set theory or any other commonly accepted basic theory) of the thesis that would convince every mathematician and close the discussions. But this happens not so rarely in mathematics (compare Mendelson's examples given above). Mathematics (and logic) is not only a formal theory but (the working mathematics) is something more. Hence one should distinguish several levels in mathematics (pre-theoretical level, level of formal reconstructions, etc.) and consequently take into account various ways of justification adopted in them.

# 9

# PHENOMENOLOGY AND PHILOSOPHY OF MATHEMATICS

## 1. Introduction

The aim of this paper is to show some connections between philosophy and the philosophy of mathematics, in particular the influence of phenomenological ideas on the philosophical conceptions concerning mathematics. We shall start by indicating the attachment of Edmund Husserl to mathematics and by presenting the main points of his phenomenological philosophy of mathematics. Next works of two philosophers who attempted to apply Husserl's ideas to the philosophy of mathematics, namely of Hermann Weyl and Oskar Becker will be briefly discussed. At last the connections between Husserl's ideas and the philosophy of mathematics of Kurt Gödel will be studied. One finds in Gödel's writings ideas similar to those of Husserl long before he started to study his works.

## 2. Husserl's Mathematical Background

The attachment of Edmund Husserl to mathematics can be seen from the very beginning of his scientific activity and career. In fact he had a mathematical background with respect to his education. As is commonly known, Husserl studied mathematics at the universities of Leipzig and Berlin under Carl Weierstrass and Leopold Kronecker (in particular he attended the full cycle of Weierstrass's lectures; Husserl's notes were among those that were used in the published edition of Weierstrass's lectures). In 1881 Husserl went to Vienna to study under the supervision of Leo Königsberger (a former student of Weierstrass) and obtained the Ph.D. in 1883 with the work *Beiträge zur Variationsrechnung* (Contributions to the Calculus of Variations).

Lectures of Brentano on psychology and philosophy attended by Husserl from 1881 at the University of Vienna impressed him so much that he decided

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to dedicate his life to philosophy. In 1886 he went to the University of Halle to obtain his *Habilitation* with Carl Stumpf, a former student of Brentano. The *Habilitationsschrift* was entitled *Über den Begriff der Zahl. Psychologische Analysen* (On the Concept of Number. Psychological Analyses) (1887). This 64 pages work was later expanded into a book (of five times the length) being one of Husserl's major works: *Philosophie der Arithmetik. Psychologische und logische Untersuchungen* (Philosophy of Arithemtic. Psychological and Logical Studies) (1891).

Working as *Privatdozent* at the University of Halle, Husserl came in contact with mathematicians: Georg Cantor, the founder of set theory (which turned to be one of the most important and fundamental theories in mathematics) and Hermann Grassmann's son, also Hermann. The former, with whom he had long philosophical conversations when they were teaching together in Halle in the 1890s, told him about Bernhard Bolzano. In fact Husserl was perhaps the first philosopher outside Bohemia to be influenced significantly by Bolzano (cf. Grattan-Guinness 2000). He discovered him first through the article by Stolz and then especially via the enthusiasm of Brentano.

Husserl's next book was two volumes *Logische Untersuchungen* (published in 1900 and 1901, resp.). Partly because of it, he was promoted in 1901 to extraordinary professor at the prestigious Göttingen university. Here one of his new colleagues was the eminent mathematician David Hilbert. At the university in Freiburg im Breisgau where he moved in 1916, Husserl met another famous mathematician, Ernst Zermelo.

Though much of Husserl's philosophy has no specific mathematical concern, mathematics had an important influence on it. In particular the origin of his philosophy can be seen in Weierstrass's program of rigorization of calculus. Also the influence of Cantor seems to be quite marked (though he was mentioned only twice in Husserl's *Habilitationscschrift*). One should mention here the choice of the number concept as the topic and the distinction of cardinal and ordinal by 'Zahl' and 'Anzahl'. We should not forget the discussions with Gottlob Frege, the founder of the logicism, one of the main trends in the modern philosophy of mathematics.

## 3. Husserl's Philosophy of Mathematics

Weierstrass' program of rigorization of calculus consisted of "le style epsilonien" (i.e., the usage of $\varepsilon-\delta$ approach in the theory of limits) and by the demand of the arithmetization of analysis. The latter should provide the reduction of all notions used in the mathematical analysis (i.e., the differential and integral calculus) to those of the arithmetic of natural numbers that seemed to be clear. Hence the concepts of (analytic) function and of a real number should be clearly defined.

And this has been done by Weierstrass, Dedekind, Cantor and others. In this situation there arose the need to characterize the very concept of a natural number. One of the approaches was that proposed by Giuseppe Peano who characterized natural numbers and their arithmetic axiomatically. Another one was the approach by Gottlob Frege who attempted to reduce this concept to pure formal logic using some ideas and methods of Cantor's set theory and defining[1] numbers as extensions of concepts (the latter were themselves taken to be the references of concept-words).

Husserl was not satisfied with those solutions. His position, especially in *Philosophie der Arithmetik* was resolutely anti-axiomatic. According to him one should not found "arithmetic on a sequence of formal definitions, out of which all the theorems of that science could be deduced purely syllogistically" – as he wrote in *Philosophie der Arithmetik* (cf. 2003, p. 311). As soon as one comes to the ultimate, elementary concepts, all defining has to come to an end and one should point to the concrete phenomena from or through which the concepts are abstracted and to show the nature of the abstraction process. According to that, one should focus on "our grasp of the concept of number" (cf. 2003, p. 311) and not on the number as such.

He wrote (cf. Husserl 2003, pp. 310–311):

> Today there is a general belief that a rigorous and thoroughgoing development of higher analysis [...] excluding all auxiliary concepts borrowed from geometry, would have to emanate from elementary arithmetic alone, in which analysis is grounded. But this elementary arithmetic has, as a matter of fact, its sole foundation in the concept of number; or, more precisely put, it has it in that never-ending series of concepts which mathematicians call 'positive whole numbers'. [...] Therefore, it is with the analysis of the concept of number that any philosophy of mathematics must begin.

In this way Husserl took on the task of continuing Weierstrass' program. In *Philosophie der Arithmetik* he wrote (cf. Husserl 2003, p. 7):

> [...] perhaps I have succeeded in preparing the way, at least on some basic points, for the true philosophy of the calculus, that desideratum of centuries.

In the *Philosophie der Arithmetik* Husserl enhanced the approach taken by Weierstrass and other mathematicians of the time in defining the natural numbers by counting with Brentano's methods of descriptive psychology.[2] Hence in the

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[1] He did it informally in *Grundlagen der Arithmetik* (1884) and then carried out the project formally in *Grundgesetze der Arithmetik* (1893 and 1903) by using his concept-script (ideography) introduced in *Begriffsschrift* (1879).

[2] Later when he was attacking the psychologistic point of view of logic and mathematics – e.g., in the first volume of *Logische Untersuchungen* – he appears to reject much of his early work, though the forms of psychologism analysed and refuted there do not apply directly to his *Philosophie der Arithmetik*. It is sometimes suggested that Husserl changed from psychologism to antipsychologism under the influence of Frege's review of his *Philosophie der Arithmetik* (cf. Frege 1894). In fact he changed his mind about psychologism as early as 1890, a year before *Philosophie* was published. He

first part of it Husserl gave a psychological analysis of our everyday concept of number. And this analysis was for him an analysis of an experience of the presentation of a number and in particular an elucidation of its origin. According to Husserl the concept of a number cannot be defined in logic. He wrote (cf. Husserl 2003, p. 136):

> [...] the difficulty lies in the phenomena, in their correct description, analysis and interpretation. It is only with reference to the phenomena that insight into the essence of the number concept is to be won.

Our intellect and time are limited and consequently we can have an intuitive understanding of only a small part of mathematics. To overcome those limitations we make use of symbols to assist our thinking. In particular we know almost all of arithmetic only indirectly through the mediation of symbols. This explains why in the second part of *Philosophie der Arithmetik* Husserl discusses the symbolic representation of numbers.

Husserl highlighted the intentional act of abstraction from maybe disparate or heterogeneous somethings to form embodiments (*Inbegriffe*). Two bases furnished the psychological foundation of the number-concept: (1) the concept of collective unification, and (2) the concept of Something.

Husserl claims that numbers are no abstracta and distinguishes, say, '5' from 'the concept 5'. According to him "the arithmetician does not operate with the number concept as such at all, but with the generally presented objects of this concept" (2003, p. 315). In his phenomenological approach to numbers the latter are seen as multiplicities (*Vielheiten*) of units.

Husserl claimed, following Brentano, that consciousness can be best characterized by being intentional in a theoretical sense of directedness towards an object. In our perception we can focus on some general features, among others on geometrical or arithmetical features, rather than focusing on the individual physical objects. Mathematical entities are typical of the kind of entity that we are dealing with when we are focusing on essence. For Husserl, mathematics is a typical example of an eidetic science, a science of eidos. In mathematics one studies certain kinds of essences: numbers in arithmetic and shapes and related features in geometry.[3] We can have insights into essences as well as physical objects. Husserl uses here the term 'intuition' (*Anschauung*) and divides it into: perception when the object is a physical object, and essential insight or *Wesensschau* when the object is an essence. For him there is no difference in principle between experiencing mathematical objects and experiencing physical objects, in fact in both cases we are structuring the world we are confronted with. Mathe-

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said that he has had doubts about psychologism from the very beginning. He attributed his change of mind to Leibniz, Bolzano, Lotze and Hume. He makes no mention of Frege as being decisive for the change.

[3] Husserl proposed an extension of geometry into a systematic examination of the kind of features one later has come to study under the heading 'topology'.

matics as an eidetic science is exploring the world of abstract entities. The latter, like numbers and other essences are transcendent (as physical objects are), i.e., they have more to them than what we grasp in any normal process of grasping, for example in perception. They have not only a richness of features that we know but they are also conceived of as having much more to them than what we know, something that we might want to find out. There arises a question: how we go about studying abstract entities? Husserl uses here the method called by him eidetic variation. It is based on an idea which Bolzano used to define analyticity and to develop his theory of probability, anticipating a main idea in Carnap's theory of probability. According to it one tries out various objects that instantiate some essences to see whether they also instantiate others.

Mathematics is a formal ontology, it studies all the possible forms of being (of objects). Formal categories in their different forms are the objects of study of mathematics – and not the sensible objects themselves. Through the faculty of categorial abstraction we are able to get rid of sensible components of judgement and focus on formal categories themselves. Thanks to eidetic intuition (or essential intuition) we are able to grasp the possibility, impossibility, necessity and contingency among concepts or among formal categories. Categorial intuition together with categorial abstraction and eidetic intuition are the basis for mathematical knowledge.

Comparing Husserl and Frege one sees that for the former direct experience, i.e., perception, is the ultimate basis for the meaningful analysis of numbers (and other mathematical notions), whereas the latter relies on the certainty given by logic. Husserl wants only to describe our experiences. Frege's logical analysis consists in constructing a notion of number in the ideography. For Husserl such an approach is artificial or, as he says, "chimerical".[4] He claims that one should analyse concepts as they are given to us.

## 4. Weyl's and Becker's Phenomenological Philosophy of Mathematics

Ideas of Husserl found response in papers of the famous German mathematician Hermann Weyl (1885–1955). His interests in philosophy go back to his graduate student days between 1904 and 1908 and his allegiance to it lasted till the early twenties.[5] The influence of Husserl's ideas can be seen in the care with which Weyl treated issues like the relationship between intuition and formalization (cf. his *Das Kontinuum*, 1918), the connection between his construction postulates

[4] Cf. Husserl 1970, pp. 119–120; 2003, p. 125.

[5] Few years after the publication of *Das Kontinuum* (1918) Weyl joined the intuitionistic camp of L.W.J. Brouwer and developed his own approach to the intuitionism claiming that philosophy and intuitionism are strictly connected. Later he attempted to do justice to Hilbert's program of formalism and doing this he changed his philosophical position substantially.

and the idea of a pure syntax of relations, the appeal to a *Wesensschau*, etc. In the preface to the work *Das Kontinuum*, Weyl explicitly declares that he agrees with the conceptions that underlie the *Logische Untersuchungen* with respect to epistemological side of logic. Answering to Husserl's gift of the second edition of the *Logische Untersuchungen* to him and his wife he wrote in a letter to Husserl's (cf. Husserl 1994, p. 290):

> You have made me and my wife very happy with the last volume of the *Logical Investigations*; and we thank you with admiration for this present. [...] Despite all the faults you attribute to the *Logical Investigations* from your present standpoint, I find the conclusive results of this work – which has rendered such an enormous service to the spirit of pure objectivity in epistemology – the decisive insights on evidence and truth, and the recognition that 'intuition' [*Anschauung*] extends beyond sensual intuition, established with great clarity and concisiveness.

On the other hand Husserl read Weyl's *Das Kontinuum* as well as published in the same year *Raum, Zeit, Materie* (cf. 1922) and found them close to his views. He stressed and praised Weyl's attempts to develop a philosophy of mathematics on the base of logico-mathematical intuition. Husserl was pleased to have Weyl – who was a prominent mathematician – on his side. In a private correspondence he wrote to Weyl that his works were being read very carefully in Freiburg and had had an important impact on new phenomenological investigations, in particular those of his assistent-to be Oskar Becker.

Oskar Becker (1889–1964) studied mathematics at Leipzig and wrote his doctoral dissertation in mathematics under Otto Hölder and Karl Rohn in 1914. He then devoted himself to philosophy and wrote his *Habilitationsschrift* on the phenomenological foundations of geometry and relativity under Husserl's direction in 1923. He admitted that it was Weyl's work that made a phenomenological foundation of geometry possible. Becker became Husserl's assistant in the same year. In 1927 he published his major work *Mathematische Existenz*. The book was strongly influenced by Heidegger's investigations, in particular by his investigations on the facticity of *Dasein*. This led Becker to pose the problem of mathematical existence within the confines of human existence. He wrote (cf. 1927, p. 636):

> The factual life of mankind [...] is the ontical foundation also for the mathematical.

This stand point in the philosophy of mathematics led Becker to find the origin of mathematical abstractions in concrete aspects of human life. In this way he became critical of the Husserl's style of phenomenological analysis. This anthropological current played an important rôle in Becker's analysis of the transfinite. Hence Becker utilized not only Husserl's philosophy but also Heideggerian hermeneutics, in particular discussing arithmetical counting as "being towards death". At the end of his life Becker re-emphasized the distinction between intuition of the formal and Platonic realm as opposed to the concrete existential realm and developed his own approach to the philosophy called by him 'mantic'. With this word he referred to the fact that there is a divinatory aspect related to any attempt to understand 'Natur'. In the light of this mathematics appears as a divinatory science which by means of symbols allows us to go beyond what is accessible. Mantic philosophy will have to replace the older 'eidetic' philosophy.

Becker's works have not had great influence on later debates in the foundations of mathematics, despite many interesting analyzes included in them, in particular of the existence of mathematical objects.

Talking about Weyl and Becker one should mention also Felix Kaufmann (1895–1949), an Austrian-American philosopher of law. He studied jurisprudence and philosophy in Vienna and from 1922 till 1938 (when he left for the USA) he was a *Privatdozent* there. He was associated with the Vienna Circle. He wrote on the foundations of mathematics attempting, along with Weyl and Becker, to apply the phenomenology of Husserl to constructive mathematics. His main work is here the book *Das Unendliche in der Mathematik und seine Ausschaltung* (1930).

## 5. Gödel's Philosophy of Mathematics and Phenomenology

One of the most eminent logicians and philosophers of mathematics by whom we find Husserl's phenomenological ideas is Kurt Gödel (1906–1978). Let us start by noting that Husserl never referred to Gödel. In fact he was more than 70 when Gödel obtained his great results on the incompleteness and consistency, and he died a few years later, in 1938, without seeming to have taken notice of Gödel's work. Also Gödel never referred to Husserl in his published works. However his *Nachlass* shows that he knew Husserl's work quite well and appreciated it highly.

Gödel started to study Husserl's works in 1959 and became soon absorbed by them finding the author quite congenial. He owned all Husserl's main works.[6] The underlinings and comments (mostly in Gabelsberger shorthand) in the margin indicate that he studied them carefully. Most of his comments are positive and expand upon Husserl's points but sometimes he is critical. One should note that Gödel expressed philosophical views on mathematics similar to those of Husserl long before he started to study them. Views found in Husserl's writings were not radically different from his own. It seems that what impressed him was Husserl's general philosophy which would provide a systematic framework for a number of his own earlier ideas on the foundations of mathematics.

[6] He owned among others the *Logische Untersuchungen* (in the edition from 1968), *Ideen*, *Cartesianische Meditationen und Pariser Vorträge*, *Die Krisis der europäischen Wissenschaften und die transzendentale Phänemenologie*.

Gödel considered both central questions in the philosophy of mathematics: (1) what is the ontological status of mathematical entities, and (2) how do we find out anything about them?

Considering the first problem one should say that Gödel had held realist views on mathematical entities since his student days (cf. Wang 1974, pp. 8–11) – more exactly since 1921–1922. In "Russell's Mathematical Logic" (1944) he wrote about classes and concepts:

> It seems to me that the assumption of such objects is quite as legitimate as the assumption of physical bodies and there is quite as much reason to believe in their existence. They are in the same sense necessary to obtain a satisfactory systems of mathematics as physical bodies are necessary for a satisfactory theory of our sense perceptions and in both cases it is impossible to interpret the propositions one wants to assert about these entities as propositions about the "data", i.e., in the latter case the actually occurring sense perceptions. (p. 141)

Similar views were expressed by him in his Gibbs lecture (1951) and in the unfinished contribution to *The Philosophy of Rudolf Carnap* (1953) "Is Mathematics Syntax of Language?" (cf. Gödel 1953). In (1953, p. 9) he writes about concepts and their properties:

> Mathematical propositions, it is true, do not express physical properties of the structures concerned [in physics], but rather properties of the *concepts* in which we describe those structures. But this only shows that the properties of those concepts are something quite as objective and independent of our choice as physical properties of matter. This is not surprising, since concepts are composed of primitive ones, which, as well as their properties, we can create as little as the primitive constituents of matter and their properties.

It should be stressed that Gödel does not claim here the objective existence of properties but says only that they are as objective as the physical properties of matter. Compare this with Husserl's claim that abstract objects of mathematics have – like other essences – the same ontological status as physical objects, that they are objective but not in the straightforward realist sense.

The comparison of the status of mathematical objects and physical objects one finds also in Supplement to the second edition of Gödel's paper "What is Cantor's Continuum Problem?" where he says (cf. Gödel 1947, p. 272) that the question of the objective existence of the objects of mathematical intuition is an exact replica of the question of the objective existence of objects of the outer world. Føllesdal (1995a, p. 440) notes that "Gödel's use of the phrase 'exact replica' brings to mind the analogy Husserl saw between our intuition of essences in *Wesensschau* and of physical objects in perception".

Let us turn now to the second problem, i.e., to the epistemology of mathematics. As indicated above in "Russell's Mathematical Logic" (1944) Gödel talked about elementary mathematical evidence or mathematical "data" and compared it to sense perception. The notion of mathematical intuition has been discussed by him also in the papers (1951) and (1953) quoted above. In (1951, p. 320) he wrote:

> What is wrong, however, is that the *meaning* of the terms (that is, concepts they denote) is asserted to be something manmade and consisting merely in semantical conventions. The truth, I believe, is that these concepts form an objective reality of their own, which we cannot create or change, but only *perceive* and describe.

In (1953, p. 359) he writes:

> The similarity between mathematical *intuition* and *physical sense* is very striking. It is arbitrary to consider "This is red" an immediate datum, but not so to consider the proposition expressing modus ponens or complete induction (or perhaps some simpler propositions from which the latter follows). For the difference, as far as it is relevant here, consists solely in the fact that in the first case a relationship between a concept and a particular object is perceived, while in the second case it is a relationship between concepts.

In the Supplement to the second edition of "What is Cantor's Continuum Problem?" (Gödel 1947) he writes:

> But despite their remoteness from sense experience, we do have something like a *perception* of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical *intuition*, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them [. . . ]. (p. 271)

Gödel did not explain what is the object of mathematical intuition. There are the following possibilities: propositions (cf. 1953), concepts (cf. 1951), sets and concepts (1947), or all three. Recall that Husserl distinguished two kinds of intuition: perception (where physical objects are intuited) and eidetic intuition (where the object is an abstract entity) and claimed that the former is more basic. It is not clear if Gödel shared his views in this respect.

It is worth quoting still one passage from the second edition of (Gödel 1947) where he wrote (pp. 271–271):

> That something besides the sensations actually is immediately given follows (independently of mathematics) from the fact that even our ideas referring to physical objects contain constituents qualitatively different from sensations or mere combinations of sensations, e.g., the idea of object itself. [. . . ] Evidently the "given" underlying mathematics is closely related to the abstract elements contained in our empirical ideas. It by no means follows, however, that the data of this second kind, because they cannot be associated with actions of certain things upon our sense organs, are something purely subjective, as Kant asserted. Rather they, too, may represent an aspect of objective reality, but, as opposed to the sensations, their presence in us may be due to another kind of relationship between ourselves and reality.

Føllesdal (1995a, p. 442) suggests that Gödel's point in this passage is that "what is given in our experience is not just physical objects, but also various abstract features that are instantiated by these objects".

Mathematical intuition cannot guarantee us certainty of our knowledge. In fact neither perception nor categorical intuition are infallible sources of evidence. Gödel writes about four different methods one can use to get insight into the mathematical reality:

- elementary consequences,
- success, i.e., fruitfulness in consequences,
- clarification,
- systematicity.

With the first one we have to do in the situation when recondite axioms have elementary consequences, e.g., axioms concerning great transfinite numbers can have consequences in the arithmetic of natural numbers. Clarification refers to situations when a discussed hypothesis cannot be solved generally but it is solvable with the help of some new axioms (compare the problem of the continuum hypothesis and the axiom of constructibility). The last systematicity refers to the method of arranging the axioms in a systematic manner what enables us to discover new ones.

The last method (that recalls Husserl's "reflective equilibrium" approach to justification) was mentioned by Gödel in the manuscript "The Modern Development of the Foundations of Mathematics in Light of Philosophy" (1961). Gödel described there in philosophical terms the development of the study of the foundations of mathematics in the 20th century and fitted it into a general scheme of possible philosophical *Weltanschauungen*. Among others he discussed also Husserl's philosophy finding in it the method for the clarification of meaning of mathematical concepts.[7] He wrote there (cf. 1961, p. 385):

> [...] it turns out that in the systematic establishment of the axioms of mathematics, new axioms, which do not follow by formal logic from those previously established, again and again become evident. It is not at all excluded by the negative results mentioned earlier that nevertheless every clearly posed mathematical yes-or-no question is solvable in this way. For it is just this becoming evident of more and more new axioms on the basis of the meaning of the primitive notions that a machine cannot imitate.

Gödel refers here to his famous incompleteness results from (1931a). They state that (1) every consistent theory containing the arithmetic of natural numbers contains undecidable propositions and that (2) no such theory can prove its own consistency. Those results showed that neither Hilbert's program of justification of the classical mathematics by means of finitary methods nor Carnap's syntactical program reducing mathematics to its syntax can be realized. Hence the rôle of mathematical intuition which can help us to find out deeper meaning and properties of mathematical concepts that are not included in definitions given by axioms. Gödel says in (1961) that there "exists today the beginning of a science

[7] This is the only place in which Gödel mentions Husserl and his philosophy explicitly.

which claims to possess a systematic method for such clarification of meaning, and that is the philosophy founded by Husserl" (p. 382). And he continues (1961, p. 383):

> Here clarification of meaning consists in concentrating more intensely on the concepts in questions by directing our attention in a certain way, namely, onto our own acts in the use of those concepts, onto our own powers in carrying out those acts, etc. In so doing, one must keep clearly in mind that this philosophy is not a science in the same sense as the other sciences. Rather it is [or in any case should be] a procedure or technique that should produce in us a new state of consciousness in which we describe in detail the basic concepts we use in our thought, or grasp other, hitherto unknown, basic concepts.

This path of Gödel from the incompleteness results to philosophy is not surprising. In a sense the incompleteness theorems support and are supported by phenomenological views. They support philosophy because they suggest that an intuition of mathematical essences or a grasp of abstract concepts that cannot be understood on the basis of axioms alone is required in order to solve certain problems and to obtain consistency proofs for formal theories. On the other hand they are supported by philosophy because the latter gives mathematical essences their due. Gödel claimed that it is necessary to ascend to stronger, more abstract principles and axioms to be able to solve problems from the lower levels (for example to set theoretic principles to solve number-theoretic problems). This idea was strongly supported by the results of Paris, Harrington and Kirby which provided examples of genuine mathematical statements that refer only to natural numbers, that are undecidable in number theory but that can be solved by using infinite sets of natural numbers.

In the paper "The Modern Development of the Foundations of Mathematics in Light of Philosophy" (1961) Gödel says also that it is not excluded that every clearly formulated mathematical yes-or-no question can be solved through cultivating our knowledge of abstract concepts, through developing our intuition of essences. In fact in this way more and more new axioms become evident on the basis of the meaning of the primitive concepts that a machine, i.e., a formal procedure, cannot emulate.

It seems that Gödel have settled on Husserl's philosophy because according to it we are directed toward and have access to essences in our experience – and this is a support for platonism which was Gödel's favourite conception in the philosophy of mathematics.

## 6. Conclusions

Husserl's postpsychologistic, transcendental view of mathematics is still a live option in the philosophy of mathematics. As Tieszen writes, it is "compatible with the post-Fregean, post-Hilbertian and post-Gödelian situation in the foundations of mathematics” (cf. Tieszen 1994, p. 335). Phenomenological approach to the philosophy of mathematics is still being developed by various authors. The starting point for their considerations are, however, not directly Husserl’s works but rather Gödel’s considerations. Let us mention here, for example, P. Benacerraf, Ch. Chihara, P. Maddy, M. Steiner, Ch. Parsons and R. Tieszen. They are not only commenting Gödel’s works but are developing their own phenomenological interpretations of mathematics concentrating first of all on the problem of mathematical intuition – cf., for example, Maddy (1980), Parsons (1980) or Tieszen (1988). One should also mention here G.-C. Rota (cf. Atten 2007).

# PART II

# HISTORY

# OF LOGIC AND MATHEMATICS

# 10

# HOENE-WROŃSKI – GENIUS OR MADMAN?[1]

The aim of the paper is to present main facts from the life of Józef Maria Hoene-Wroński as well as his most important achievements in philosophy and mathematics.

## 1. The Life of Hoene-Wroński

Józef Maria Hoene-Wroński was born on 23rd August 1776 in Wolsztyn near Poznań.[2] His father Antoni Hoene (a Czech who came to Wolsztyn from Saksonia) was an architect. In 1777 the family moved to Poznań where between 1786 and 1790 Józef attended the school (Szkoła Wydziałowa). After finishing the school he decided to enter the army but his father was against it. So Józef left the family house and having changed the name to Wroński became the cadet of the artillery. He moved up very quickly: in July 1794 he was Second Lieutenant and in August Lieutenant and the commander of a battery. In the battle by Maciejowice he was taken prisoner. He entered then the Russian army and was promoted to the rank of major; he was even in the headquarters of A.W. Suvorov.

After the death of his father in 1796 and after he has come into an inheritance (about 17,035 Polish zloties) Wroński decided to resign from the army career and to devote himself to science. In 1797 he resigned (he was granted the right to carry the officer's uniform till the end of his life). In 1798 Wroński left for Königsberg where he wanted to study philosophy. When it turned out that

[1] Originally published under the title "Genius or Madman? On Life and Work of J.M. Hoene-Wroński" in: W. Więsław (ed.), *European Mathematics in the Last Centuries*, pp. 77–86 (2005), Wrocław: Stefan Banach International Mathematical Center/Institute of Mathematics, Wrocław University. © Copyright by Institute of Mathematics, Wrocław University. Reprinted with kind permission of the Institute of Mathematics of Wrocław University.

[2] This is the most probable date. Wroński himself in a biographical note for the police from the year 1801 wrote that he was born on 20th August 1776 and on his gravestone the date 24th August 1776 is written.

In: Roman Murawski, *Essays in the Philosophy and History of Logic and Mathematics* (*Poznań Studies in the Philosophy of the Sciences and the Humanities*, vol. xx), pp. 149–160. Amsterdam/New York, NY: Rodopi, 2009.

Kant is already pensioned he changed his plans and started his studies at universities in Halle and Göttingen. In 1800 he left to England and next to France with the aim to enter Polish legions. General Dąbrowski accepted his request and sent him to Marseille where at that time both legions of Polish Corp were quartered. In Marseille Wroński came into several scientific contacts, became the member of Marseille Academy of Sciences and of Marseille Medical Society. Being engaged in science, he soon left legions.

For the further scientific fate of Wroński very important was the day 15th August 1803, hence the day on which in the Roman catholic church the Feast of the Assumption (of Mary) is celebrated. On that day, during a ball he received illumination. To remember this day he took the new forename, namely Maria.

He devoted himself at that time to two problems:

(1) to develop a new philosophy, the "achromatic" philosophy[3] as he used to call it, which should overcome the limits of the world of things and reach the Absolute and the principles of the creation in order to deduce from them a logically consistent theory of the whole universe,
(2) to realize the fundamental reform of the system of knowledge which should begin with the reform of mathematics (the latter should consist ultimately of the deduction of all parts of mathematics from a unique general law, namely from the "absolute law of algorithm").

The costs of living and of the edition of his works (as well as of rewriting his manuscripts) were soon so high that all his funds have been exhausted. The unique source of income were the lessons in mathematics he was giving. In 1810 he married one of his pupils – Wiktoria Sarrazin de Montferrier.

In September this year he decided to move to Paris and to present there the results of his investigations. Unfortunately he found there neither recognition nor support. So he ran in financial troubles. In this situation he turned to the Russian embassy with the request for a support. He was granted a support and could publish *Introduction á la philosophie des mathématiques et technie d'algorithmie* which was dedicated to the emperor of Russia Alexander I. This work has not found recognition and the author was forced to look for further patrons. He wrote even to Napoleon but received no answer.

Fortunately he found a new pupil, namely Peter Joseph Arson who was a rich businessman and who wanted to study. He became Wroński's student and patron and helped Wroński also by scientific and editorial as well as daily duties. His generosity was enormous (his financial contributions is estimated at 108,516 francs). The reason for that was his hope that Wroński will let him to the mystery and will reveal him the essence of the Absolute. The latter promised to do so but finally was not true to his word. So Arson left Wroński.

[3]The name comes from the Greek *chrema* – thing.

Using the money Hoene-Wroński was given by Arson, he started to publish a journal *Le Sphinx*. This journal together with the other journal *Ultra* which has also been founded by Wroński should help to popularize the ideas and principles of his social doctrine. According to it the world consists of two blocks: the party of "independent", i.e., the party of "human rights" and the party of "royalists", i.e., the party of "God's rights". Both are right but only partially. The outcome from that antinomic situation saw Wroński in the knowledge and in the practical realization of the absolute truth. Both journals were published a short time.

To get money for living Wroński decided to be occupied by inventions. After the attempts to get a loan (to cover the costs of the journey to St. Petersburg or at least to Warsaw) from the Russian ambassador in Paris failed he decided in 1820 to travel to England. (To be able to do that he was forced to pledge all his movables.) In London he presented to Thomas Young – the member of the London Royal Society for Supporting the Natural Sciences – a new theory of the moon. This theory was intended to solve definitively the problem of measuring the longitude. He also gave him his manuscript with the new theory of solving the equations. Both works were rejected by Young because he saw in them first of all "metaphysical abstractions". This decision was of course not accepted by Wroński and in this way the quarrel with Young began. Since Wroński saw no chances for a success he decided to come back to France. Even the news that all his mathematical manuscripts which he had pledged by booksellers have been sold by the weight of the paper did not keep him back.

After coming back to France in 1823 he was occupied by constructing mathematical instruments and by improving steam machines. He was also active in writing. In particular he wrote then the *Absolute Encyclopedia* (which till today exists only as a manuscript) and prepared a memorial to the Pope Leo XII. The latter consisted of the explanation of the principles of the absolute philosophy with applications to theology. Wroński proclaimed there also a new principle of the religion which should make possible (with the help of the reason) the solving of all problems of faith. He worked also on *Messianisme. Union finale de la philosophie et de la religion, constituant la philosophie absolue*, volume I: *Prodrome du messianisme. Révélaton des destinées de l'humanité* (1831). He began then to call his doctrine and his role in the history by the name Messianism.

Using the support of (among others) Emil Thayer, Wroński was able to publish his next works, in particular the second volume of *Métapolitique messianique* (1823) and *Le destin de la France, de l'Allemagne et de la Russie, comme Prolégomènes du messianisme* (1842–1843). The latter won for him two followers from the circle of Polish emigration, namely Antoni Bukaty and Leonard Niedźwiecki (Niedźwiecki has later published at his own expense some of Wroński's works). He attacked Adam Mickiewicz and Andrzej Towiański and reproached them for having stolen the name Messianism and having used it for their “mad theosophical phantasies”.[4]

The cooperation with Thayer went on to the year 1847 and the breach was turbulent – similarly as it was in the case of Arson. After that the financial situation of Wroński was catastrophic. He tried to find a solution by selling his works by the weight of the paper. The rescue came from the side of the Kamil Count Durutte – he invited Wroński to Metz and covered there his costs of living as well as the costs of publishing his works.

After the coup d’état of Ludwig Napolean Wroński came back to Paris. Here he found a new student and supporter – namely Ludwik Crozals who wanted to be introduced into the world of Messianic truths. Wroński rejected his request (what did not abstain him from using his financial support). He justified his refusal by saying that “his doctrine brings new ideas to the world and as such has so far no fixed price; its price can be either null or infinite”.

At the end of 1853 Wroński fell ill. He died on 10th August and was buried on the cemetery in Neuilly. On the gravestone (the costs of it were covered by Count Durutte) the words “L’acte de chercher la Verité accuse le pouvoir de latrouver” as well as formulas of three mathematical laws (treated by Wroński as symbols of the Messianism) and the letters A.M.D.G. (Ad Maiorem Dei Gloriam) were engraved.

## 2. Works and Manuscripts of Wroński

The best bibliography of Hoene-Wroński was given by S. Dickstein in *Katalog dzieł i rękopisów Hoene-Wroński* [The Catalogue of Works and Manuscripts of Hoene-Wroński] (1896a) and by B.J. Gawecki in *Wroński i o Wrońskim* [Wroński and About Wroński] (1958).

Manuscripts of Hoene-Wroński belong now to the Biblioteka Kórnicka [Kórnik Library]. A part of them (mostly manuscripts with mathematico-physical contents) was bought in 1875 by the count Jan Działyński from Hoene-Wroński’s stepdaughter Batylda Conseillant, the rest (mostly manuscripts of philosophical contents) was the property of Leonard Niedźwiecki (we mentioned him earlier) and after his death became the property of Biblioteka Kórnicka.

[4]The poet Adam Mickiewicz and Andrzej Towiański were representatives of one of the trends in Polish Messianism – both, similarly as Wroński, were active in France. The Messianism was an ideology proclaiming that the Polish nation plays the role of the saviour of the whole humanity and of the spiritual leader, hence of the Messiah. It started after the overthrow of the November uprising (1831) as an attempt to explain the metaphysical sense of the overthrow and to describe its role in the history. As a historiographical conception it was heterogeneous. It referred to the Bible as well as to ideas of French, German and other thinkers (L.C. Saint-Martin, J. Boehme, E. Swedenborg).

The list of published works by Hoene-Wroński comprises 107 items, among them Dickstein mentions: 28 mathematico-physical papers, 35 philosophical and political works, 8 papers devoted to problems of transport, 11 polemic papers as well as 23 manifestos, programs and brochures. Add that on title pages of most of Hoene-Wroński's works there is a sign of Sphinx on which the "the law of highest algorithm", i.e., the law:

$$Fx = A_0 \cdot \Omega_0 + A_1 \cdot \Omega_1 + A_2 \cdot \Omega_2 + A_3 \cdot \Omega_3 + \dots$$

is written.

The list of Hoene-Wroński's manuscripts was divided by Dickstein into some sections, namely: pure and applied mathematics (222 items), transport (37), statistics and economy (19), philosophy and pedagogy (23), politics and jurisprudence (20), varia (16). Most of the manuscripts contain a date. At the end of every work one finds sign A.M.D.G. or/and (especially in earlier works) at the beginning the symbol O (what meant: in nomine coeli) or letters W.I.B. (an abbreviation of Polish: W imię Boże, what means: In God's name). Manuscripts were written exclusively in French (with one exception which was written in English). Many of them are copies prepared by the secretary or by Hoene-Wroński's wife. Mathematical formulas were mostly written by Hoene-Wroński himself.

Hoene-Wroński wrote and published almost exclusively in French. His works have been translated into Italian, Spanish, Portuguese, German, Russian, English and Polish. His selected philosophical works have been published in French and Italian, namely: *Oeuvre philosophique de Hoëne-Wroński* (2 volumes, 1933 und 1936) and *Collezione italiana degli scritti filosofici* (3 parts, 1870–1872 and 1878).

## 3. Philosophy of Hoene-Wroński

The philosophical system of Hoene-Wroński is included into the Messianic philosophy. This philosophy was born under the influence of German thinkers like Kant, Schelling and Hegel. Hoene-Wroński belonged to the earlier generation of Polish Messianists and was in a sense a forerunner. Other Messianists were active between 1830 and 1863 (one should mention here Bronisław Trentowski, Józef Gołuchowski, August Count Cieszkowski, Karol Libelt and Józef Kremer).

The characteristic feature of those philosophers was their interest in the metaphysics. The metaphysics founded by them was rather spiritual than idealistic (like the German one). Typical for it was the theistic conviction of the existence of God as a person, of the immortality of soul and of the full and unbounded superiority of spiritual powers over the bodily ones. The aim of this philosophy was not only the knowledge of the truth but first of all the reform of the life and the redeption of the mankind. Characteristic was the belief in the metaphysical role of the nation. It was taught that a man can realize his fate only in the framework of a national spirit and that the nations decide about the development of the humanity. The special task was given to the Polish nation – namely the task of being the Messiah. This explains also the name "Messianism" for the trend.

The starting point of the philosophical system of Hoene-Wroński was the philosophy of Immanuel Kant. Wroński claimed that Kant made an error by treating the knowledge (reason) and the objective reality as being heterogeneous instead of treating them as homogeneous. This led to the duality of knowledge and being, of mental and phenomenal worlds. To overcome those limits one should derive both of them from a single higher principle: from the Absolute (one can see here the influence of Hegel; Hegel reduced the empirical reality to the thinking, to the absolute world-ghost whereas Hoene-Wroński wanted to derive both from one main principle).

But what is the Absolute? Hoene-Wroński gave no answer to that question. He understood the Absolute in an abstract way and claimed that one can give various features of the Absolute but they will be only descriptions of it relatively to other beings (objects) and not the description of the essence of it. To get to know the Absolute one should "overcome the temporal conditions of a rational being" and to raise to the absolute reason. This can be done in a "pure inspection", hence by intuition which is a cognitive activity as well as an existential one.

The aim of Hoene-Wroński was not only to get to know and to describe the Absolute but to derive the whole reality from the Absolute using one general principle (also here the influence of Hegel can be seen). He called it "the law of the creation" (*loi de la création*). He spoke very enigmatically about this law and claimed that the being creates itself: it comes into existence by a dichotomy (it plays here a similar role as that played by the trichotomy in Hegel's dialectics) and by the disintegration of any unity into thesis and antithesis (antitheses) .

Philosophy leads to the knowledge about the Absolute and therefore one can identify it with the religion. It transforms the revealed religion into the rational one. As a consequence the discoverer of the absolute philosophy becomes in a certain sense a Messiah who proclaims the unity of the knowledge and religion. For that reason Hoene-Wroński distinguished two main parts of the philosophy: the one which gets to know the reality (he called it theory) and the one that directs it to the fulfilment of its aims (called technie by him). Just the second component (unifying the philosophy and the practice) makes the system of Hoene-Wroński richer than the system of Hegel and the whole German idealism.

Being led by the law of creation the mankind can fulfill its destiny and to pass from the present political system which is full of antagonisms and inconsistencies to a fully rational system. In connection with this Hoene-Wroński distinguished four stages in the hitherto history. Each of them had different aims, in particular materialistic aims (Orient), moral ones (Greece, Rome), religious aims (the Middle Ages) and intellectual ones (modern times till the 18th century). The 19th century was a transitory period: period of the struggle between the conservative trend aiming at the good and the liberal one whose aim is the truth. The future of the mankind is in the unification of the good and the truth, of the religion and the knowledge, in a creation of the epoch of "absolute good and truth". And just here a special role should be played by Slavonic nations (Hoene-Wroński gave no justification of this thesis).

One can see that the philosophical system of Hoene-Wroński was in fact idealistic. The mystical elements were braided in it with extremely rationalistic ones. Hence his system belongs to the rationalistic trend as well as to the romantic one. It was a peculiar combination of metaphysics, historiography, religion, ethics and politics. This explains the interests of Hoene-Wroński in the philosophy but also his engagement in the political activity.

## 4. Mathematical Works of Hoene-Wroński

Hoene-Wroński was interested in many branches of mathematics. He worked in algebra, differential and integral calculus, function theory, differential equations, arithmetic and number theory, combinatorics and probability, geometry and trigonometry as well as general mechanics, mechanics of liquids, thermodynamics, mathematical physics, astronomy and philosophy and history of mathematics. Most of his works have not been published and exist in manuscripts.

We cannot go into technical details of Hoene-Wroński's mathematical works – we shall discuss only main ideas contained in them.

Mathematics of Hoene-Wroński grew out of his philosophical ideas presented above, even more – it was subordinated to hem. The main goal of Wroński was the reform of all the existing mathematics and the eduction of it from certain fundamental principles and laws.

At the beginning Hoene-Wroński worked in the theory of equations, in particular he was interested in the problem of solvability of algebraic equations. It is worth noticing that he had enormous patience in calculations. Most of the results of his work in this direction have not been published.

The first "public" appearance of Hoene-Wroński was his dissertation *Priemier principe des méthodes analytiques* (not published) presented to the French Academy in 1810. One finds there an idea which will later appear in almost all his mathematical works, namely the idea that every knowledge, hence in particular mathematics, consists of the theory and the technie. The aim of the theory is to investigate the essence of mathematical quantity and the aim of the technie is the investigation of its values. Hence a theory is a system of theorems and laws whereas technie consists of methods. Hoene-Wroński divides mathematics into arithmetic and geometry and obtains in this way arithmetical theory and technie as well as geometrical theory and technie. The latter is the then new branch of mathematics – descriptive geometry. According to Hoene-Wroński the arithmetical (or analytical) technie did not exist so far. He claimed that there existed only one unique principle which enabled us to unify all known methods in a systematic way. Just this principle is the subject of the considered dissertation. This principle or the absolute law (*loi absolute*) of technical creation, later called "the highest law" (*loi suprême*) consists of the representing of a function of a variable $x$ by a sum of some infinite sequence. Making some additional assumptions Hoene-Wroński deduces from it (as particular cases) various known formulas and laws as well as indicates how it can be applied to solving equations. The decision of the commission which should evaluate the dissertation was very discreet though rather friendly for the author.

Soon afterwards Hoene-Wroński published a fundamental work *Introduction à la philosophie des mathématiques et technie d'algorithmie*. He distinguishes here two parts of mathematics: algorithmie and geometry. Algorithmie is divided into the knowledge about laws of numbers (algebra) and the knowledge about facts concerning numbers (arithmetic). The laws of extent are the subject of the general geometry whereas the facts concerning the extent – the subject of the special geometry. The task of the theory of algorithmie is to describe the nature of all "elementary algorithms" and their mutual influence and interconnections, i.e., of the "systematic algorithms". This subject has been then developed in the considered work. At the end of the book one finds "an architectonic table of mathematic" indicating the classification of branches of mathematics according to the principles presented in the work.

The work has not found many readers and those who read it were of critical opinion. In particular the mathematician Joseph Diaz Gergonne (1771-1859), editor of the journal *Annales de Mathématiques* wrote there that the dissertation of Hoene-Wroński is characterized by an obscure and hardly understandable style. Its main weakness is the subordination of mathematical matters to principles of the imagined philosophical system. He admitted that Hoene-Wroński has received some results in mathematics but it is inappropriate to connect them with a metaphysical system. Laplace called Wroński "revealer of imaginary illusions [phantasms] included into the philosophy of mathematics".

Next mathematical work by Hoene-Wroński was the dissertation *Résolution générale des équations de tous les dégrés* (1812). It contains a general method of solving algebraic equations of arbitrary degree as well as formulas for the solutions. They were given without proofs. The work has been sharply criticized. Wroński did not acknowledge the censure (most probably he did not know about the results of Ruffini from 1799).[5] A great debate followed which sometimes over-went into passionate personal polemics. Gergonne demanded from Hoene-Wroński proofs and examples of the application of his methods. Wroński did not react to the reviews and was further convinced of the perfection of his method. He did not take note of the critique of his theories and works indicating simultaneously the incorrectnesses (it is not clear whether he knew about other works in this field and whether he read them). Still in 1818 Hoene-Wroński justified his decision not to publish the proof of his methods saying that his solution is so great and valuable that it can be paid by nobody.

In 1812 Hoene-Wroński published another work, namely *Réfutation de la théorie des fonctions analytiques de Lagrange*. He criticized there the work *Théorie des fonctions analytiques* by Lagrange and in particular the idea of developing the differential calculus on the basis of the theory of sums of sequences. Hoene-Wroński's work was criticized by Academie Française first of all for the lack of clarity, for presenting formulas in the form of puzzles and for the lack of proofs. But it should be admitted that Wroński's work contained some important statements and remarks which have not been seen or have been simply ignored (one of the reasons could be certainly the unclear language used by Hoene-Wroński as well as his mannerism of presenting his results in a way which had nothing in common with the commonly accepted standards in the mathematical community).

In years 1814–1819, thanks to the financial support of Arson, Hoene-Wroński was able to publish his most extensive mathematical work: *Philosophie de l'infini* (1814), *Philosophie de la Technie algorithmique* (volume I: 1815, volume II: 1816–1817) and *Critique de la théorie des fonctions génératrices de Laplace* (1819). Those works were planned as the realization of his project to reform mathematical knowledge and simultaneously as the warrant of the reform of the whole of human knowledge.

In the first work Hoene-Wroński considered the concept of the infinity in mathematics. It is also in a certain sense a reaction to the work by L.N.M. Carnot *Réflexions sur la métaphysique du calcul infinitésimal* (first edition: 1783, second edition: 1813) though the name Carnot is not mentioned there. One finds there critique of Carnot's conceptions and further the philosophy of the differential calculus and finally some remarks on Lagrange's *Théorie des fonctions analytiques*.

The aim of the work *Philosophie de la Technie algorithmique* was to justify and to indicate applications of "the highest law" which was considered by Hoene-Wroński as his most important mathematical discovery. One finds there

[5] Paolo Ruffini (1765–1822) published in 1799 a thesis that there do not exist general formulas for solutions of algebraic equations of degree higher than 4. His proof contained gaps. The first correct proof of this heorem was given by Niels Henrik Abel (1802–1829) in 1826 (he did not know the works of Ruffini).

a "philosophical" deduction of this law, i.e., Hoene-Wroński justifies the possibility of presenting any function of the variable $x$ in the form

$$F(x) = A_0 \cdot \Omega_0 + A_1 \cdot \Omega_1 + A_2 \cdot \Omega_2 + A_3 \cdot \Omega_3 + \dots,$$

where $\Omega_i$ are functions of $x$. Wroński presented also a "mathematical" proof of the law. Unfortunately it was given in a way which was typical for him, i.e., without any details and having not precisely described the conditions and assumptions.

In *Critique de la théorie des fonctions génératrices de Laplace* Hoene-Wroński attempts to show that the generatrixes of Laplace do not provide – as Laplace pretended – a general tool and being applied to differential equations they provide in fact only artificial methods.

In 1812 Hoene-Wroński's work *Introduction to a Course of Mathematics* has been published. It was thought as a popular presentation to the general public of his plans to reform mathematics. The author begins by stating that any positive knowledge is based on mathematics or at least uses mathematics. He distinguishes four periods in the development of mathematics. The first one is the period when mathematics was carried on *in concreto*, i.e., there was no abstraction from the material reality, mathematics had a practical character (so was the case of mathematics in ancient Egypt and Babilon). The second period is the period of Greek mathematics. It can be characterized by the fact that abstraction was used but – according to Hoene-Wroński – mathematical truths were "only particular facts [cases] and have still not reached the general truths". The third period is the time from Cardano and Fermat till Kepler and Wallis. Some general truths did appear in mathematics, but they were isolated, they were "individual mathematical products". In particular the formulas for solutions of equations of degree 3 and 4 have been found but there was no idea about the general setting of the problem. The last fourth period has begun with Newton and Leibniz. Then methods have been developed which can be applied to "all the appearances of the nature". This period is characterized by the usage of sums of sequences, the only common tool so far.

The characteristic feature of all periods in the hitherto development of mathematics was – according to Hoene-Wroński – that they were based on some relative principles. This means that there were no absolute principles and – on the other hand – science should be based just on such ideas. Hence the prediction of the new higher stage in the development of mathematics. Its basis should be the reform proposed by Wroński. It consists of the division of mathematics into theory and technie. All mathematical truths should be deduced from the unique highest law and in this way they should receive the absolute certainty.

After having returned from England to Paris (1823) Hoene-Wroński devoted himself to the construction of calculating machines, in particular of the "arithmetical ring" (it was thought as an instrument for multiplying and dividing numbers), “arithmoscope” (an instrument on which various arithmetical operations could be performed) and at last “universal calculator”. With this trend is also connected his work *Canons de logarithmes* (1827). He presented there the idea of simplification of tables of the logarithmic function by the decomposition of every number and every logarithm into three components. He has taken into account the fact that various approximations are needed and therefore prepared six canons (tables) of logarithms. The first one contained logarithms with four digits, the next three – logarithms with five digits, the fifth – with six digits and the last one – logarithms with seven digits. Unfortunately it is rather difficult to use the tables.

To finish the overview of Hoene-Wroński’s mathematical works let us mention still one of them, namely the short note “Loi téléologique du hasard” (1828) which shows that he was interested also in the probability theory. He said in the paper about “the teleological law of chance”. It has been already discovered but the exact explication of its mathematical character requires still further studies in which algorithms of higher order will be necessary. This law should enable us “to describe all cases of chance and to become lords of the games; games will be deprived of the fatality”. He added also that he has already successfully used this law in a lottery in France.

## 5. Conclusions

Hoene-Wroński belongs to scientists who are rather forgotten today. His ideas have found neither supporters nor successors. It should make one wonder because he has been an extraordinary scientist and one of the most original thinker. He knew many (a dozen or so) languages, had good knowledge in various domains, he was a real scholar and erudite. On the other hand he was rather a difficult person in contact and this had certainly influenced the fate of his ideas.

He belonged to the best European metaphysicians of the beginning of the 19th century. Unfortunately the ways in which he presented his ideas and results, the unclear language he used and his self-confidence (which sometimes was close to arrogance) isolated him from his contemporaries. His philosophical ideas did not find any echo. He had no big circle of supporters or students. He was alone.

The fate of his mathematical ideas was similar. His ideas were rejected or ignored and not positively evaluated. The reason was the subordination of his mathematics to the philosophical ideas, his unclear and imprecise language in which he formulated his results and which was far from the standards accepted at that time.[6] Additionally he has consciously hidden his proofs and was con-

[6]One should take into account the fact that Hoene-Wroński lived at the time when many funda-

vinced that they are of special value and meaning. Hence he has ignored the results of other mathematicians. Even more: he read and knew almost no contemporary mathematical literature. Only some of his achievements and of his ideas survived. In fact the unique trace of his ideas which is still recognized is the concept of Wroński's determinant, i.e., the Wronskian[7] which was introduced in 1812 in the work *Réfutation de la théorie des fonctions analytiques de Lagrange*. It is worth noting that his ideas connected with the problem of solving of algebraical equations found recently appreciation. It has turned out that Hoene-Wroński was looking not for exact formulas giving the solutions but was interested in formulas and methods which enable the approximation of solutions. Nowadays studies based on Hoene-Wroński's ideas and leading to the numerical approximations of solutions are developed.

Similarly his idea of the development of a function into the sum of a sequence – though not precise enough – contained something interesting. It has been developed among others by Stefan Banach (cf. Banach 1939).

Remarkable are also Hoene-Wroński's attempts to unify and systematize the whole of mathematics. He has not succeeded in accomplishing it. His idea found also no support and understanding by his contemporaries. The reason was the fact that the whole project has been based on rather obscure philosophical ideas. Quite different was the situation later when mathematicians like Cauchy, Bolzano, Weierstrass, Dedekind as well as Frege, Russell and Whitehead reduced mathematics (in particular analysis) to the arithmetic of natural numbers and at the end to logic (and set theory). What distinguishes both those projects is the precision, clarity and exactness of the latter mathematicians and the imprecision and unclarity of Hoene-Wroński. Maybe it was too early at Hoene-Wroński's days and the idea was not yet ripe. The needed concepts had not been developed yet.

Summing up one can say that at least some of Hoene-Wroński's ideas were really deep but one cannot understand literally what he has said – one should disclose the seeds of the truth. Mathematical activity of Hoene-Wroński was something on the boarder between the greatness and madness.

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mental mathematical concepts have not been precisely defined yet.

[7] The name was introduced in 1882 by the English mathematician Thomas Muir.

# 11

# GRASSMANN'S CONTRIBUTION TO MATHEMATICS

Hermann Günter Grassmann (1809–1877) was no professional mathematician and had no formal university training in this discipline. After finishing gymnasium he decided to study theology. So he went (with his elder brother) in 1927 to Berlin to study at the university. There he took courses in theology, classical languages, philosophy and literature. But he does not appear to have taken any courses in mathematics or physics. Nevertheless he was interested in mathematics what became evident when he returned in 1830 to Stettin after completing the studies. Perhaps the influence of his father Justus Günther Grassmann (1779–1852), a teacher of mathematics and physics at a gymnasium in Stettin, was important here. Hermann decided to become a school teacher as well but he was also determined to undertake mathematical research on his own.

It is worth noting here – in connection with Grassmann's studies in Berlin – that he attended the lectures and sermons of the neo-Kantian Friedrich Schleiermacher (1768–1834) and was influenced philosophically by his *Dialektik* (1839). This influence can be seen in his later works. In particular he drew upon pairs of opposites, for example: pure mathematics (or mathematics of forms) and its applications, discrete and continuous, space and time, analysis and synthesis – all this was covered by the principle known in German philosophy as "Polarität".

Grassmann was active as a teacher at various schools in Stettin (with the exception of the school year 1834/1835 when he taught at the Gewerbeschule in Berlin). At the beginning he taught mathematics, physics, German, Latin and religious studies only at the lower level (this explains the wide range of topics). The examinations passed in 1839 (in theology) and in 1840 allowed him to teach mathematics, physics, chemistry and mineralogy at all secondary school levels.

In 1847 Grassmann attempted to get a university position. In fact he asked the Prussian Ministry of Education to be considered for such a position. The ministry asked Ernst Kummer (1810–1893) for the opinion. Unfortunately the opinion was not positive and this ended any chance for Grassmann. Kummer wrote in his report that Grassmann's works contain "considerably good mate-

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rial expressed in a deficient form" and that his results in mathematics "will be ignored by mathematicians as they were so far".

Grassmann was a dedicated teacher. He wrote also several textbooks in mathematics and languages. This activity took a lot of time and he was unable to devote much time to researches in mathematics. Nevertheless he did not resign from them. Despite the fact that his scientific works did not find an appropriate acceptance and appreciation by the contemporaries, even more, were largely ignored, he continued his studies moving temporarily from mathematics to linguistics or physics. His works devoted to Sanskrit – contrary to his achievements in mathematics – found recognition among specialist still during his life: in 1876 he was elected to the American Oriental Society and the University in Tübingen honored him with the honorary doctorate (*doctor honoris causa*). His translation of *Rig Veda* and a huge commentary on it is – according to *Encyclopedia Britannica* – still used today.

* * *

For the examination taken by Grassmann in 1840 (mentioned above) he should submit an essay. His essay *Theorie der Ebbe und Flut* was devoted to the theory of the tides – cf. (1840). As a basic theory he took the theory from Laplace's *Méchanique célest*e and Lagrange's *Méchanique analytique* but exposed them making use of the vector methods which he had been developing since 1832 (as he wrote later in the preface to his main work, namely *Die Ausdehnungslehre*, 1844). This enabled him an original and simplified approach. This essay introduced for the first time an analysis based on vectors including vector addition and subtraction, vector differentiation and vector function theory. It was the first known appearance of what is now called linear algebra and of the notion of a vector space. He developed those methods in later works, in particular in *Die lineale Ausdehnungslehre* (1844) and *Die Ausdehnungslehre. Vollständig und in strenger Form begründet* (1862) seeing that his theory can be widely applied. The examiners accepted the essay but they failed to see the importance of innovations introduced by Grassmann.

The book *Die lineale Ausdehnungslehre, ein Zweig der Mathematik* published in 1844 is his most important work. In this original work Grassmann proposed a new foundation for all of mathematics. The book began with quite general definitions of a philosophical nature. Further he developed the idea of an algebra in which the symbols representing geometric entities such as points, lines and planes, are manipulated using certain rules. In fact he studied algebras whose elements are not specified, hence are abstract quantities. He considered systems of elements on which he defined a formal operation of addition, scalar multiplication and multiplication. Starting with undefined elements which he called 'simple quantities' he generalized more complex quantities using specified rules.

He represented subspaces of a space by coordinates leading to point mapping of an algebraic manifold now called the Grassmannian.[1] Fearnley-Sanders writes in (1979, p. 811):

> Beginning with a collection of "units" $e_1, e_2, e_3, \ldots$ he effectively defines the free linear space which they generate; that is to say, he considers formal linear combinations $a_1 e_1 + a_2 e_2 + a_3 e_3 + \ldots$ where the $a_j$ are real numbers, defines addition and multiplication by real numbers [in what is now the usual way] and formally proves the linear space properties for these operations.

Further Grassmann developed the theory of linear independence (similar to the presentation found in the modern texts in algebra!), defined the notions of subspace, independence, span, dimension, join and meet of subspaces, projections of elements onto subspaces. He showed that any finite set has an independent subset with the same span and that any independent set extends to a basis. He obtained the formula for change of coordinates under change of basis, defined elementary transformations of basis and showed that every change of basis (equivalently, in modern terms, every invertible linear transformation) is a product of elementaries.

*Ausdehnungslehre* contains laws of vector spaces but, since Grassmann also has a multiplication defined, his structures satisfy the properties of what are called today algebras. Grassmann defined there the exterior product called also combinatorial product (*äußeres Produkt* or *kombinatorisches Produkt*) – this is the key operation of an algebra called now Grassmann algebra or exterior algebra.[2] In 1878 William Kingdom Clifford (1845–1879) joined this exterior algebra to William Rowan Hamilton's (1805–1865) quaternions.[3]

Among various important consequences of Grassmann's approach in *Ausdehnungslehre* was the fact that once geometry has been put into the algebraic form then the apparent restrictions of 3-dimensional space vanish – in fact the number of possible dimensions is unbounded. Grassmann wrote in (1844):

> If two different rules of change are applied, then the collection of elements produced [...] forms a system of the second step. [...] If still a third independent rule is added, then a system of the third step is attained, and so forth. Space theory may serve here as an example. [...] The plane is the system of the second step. [...] If one adds a third independent direction, then the whole infinite space (system of the third step) is produced. [...] One

[1]A Grassmannian is a space which parameterizes all linear subspaces of a vector space $V$ of a given dimension.

[2]The Grassmann algebra or the exterior algebra of a given vector space $V$ over a field $K$ is the unital associative algebra $\Lambda(V)$ generated by the exterior product, where the exterior product (or wedge product) of vectors is an algebraic construction generalizing certain features of the cross product to higher dimensions. Grassmann algebras are widely used in contemporary geometry, especially differential geometry and algebraic geometry through the algebra of differential forms, as well as in multilinear algebra and related fields.

[3]Clifford algebras are used today in the theory of quadratic forms and in relativistic quantum mechanics.

> cannot here go further than up to three independent directions (rules of change), while in the pure theory of extension their quantity can increase up to infinity.

The reception of the *Ausdehnungslehre* was bad – the book was largely ignored. Why? It was simply too revolutionary, far too much ahead of its time to be appreciated. As Petsche writes in (2006, p. 42):

> This book blew up the contemporary pictures of how geometry should be treated. Extensive philosophical introductory considerations, presentation of an abstract theory of connections that should provide a foundation for the whole of mathematics, spare usage of formulas, rejection of the geometry as a mathematical discipline and the development of a theory of mathematical varieties that is $n$-dimensional and free of any metric and many others were "proposed" to the reader.[4]

Such ideas will appear later in Riemann's (1826–1866) theory of $n$-dimensional varieties and in the Hamilton's concept of a quaternion.

On the other hand Grassmann – as autodidact – presented his ideas using new notions and concepts invented by himself and having no tradition. His presentation was too abstract and not always well-phrased.[5] One should remember that in Grassmann's days the general notion of an abstract algebra had yet to be defined and the only axiomatic theory was in fact Euclidean geometry. Möbius (1790–1868) who was the only person in Germany being close to the mathematics developed by Grassmann did not understand his approach and declined to write a review. Gauss (1777–1855) answered the request of Grassmann saying that he is too busy to learn the terminology of the *Ausdehnungslehre*. Notice also that the slow reception of Grassmann's works can have its source also in the fact that they were published "in the mathematical steppes of Pomerania" as Grattan-Guinness writes in (2000, p. 159).

On the other hand Augustin Louis Cauchy (1789–1857) and Adhémar Jean de Saint-Venant (1797–1886) claimed that they have invented systems similar to that of Grassmann. The claim of the latter is a fair one since he published in 1845 a work in which he multiplied line segments in an analogous way to Grassmann. Saint-Venant claimed that he had developed his ideas already in 1832. Cauchy

[4] "Dieses Buch sprengte die zeitgenössischen Vorstellungen von der Behandlung der Geometrie. Umfangreiche philosophische Vorbetrachtungen, Darlegung einer abstrakten, als Grundlage der gesamten Mathematik konzipierten Theorie der Verknüpfungen, spärlicher Formelgebrauch, Ablehnung der Geometrie als mathematische Disziplin und Entwicklung einer $n$-dimensionalen, metrikfreien Theorie der mathematischen Mannigfaltigkeit u.a.m. wurden dem Leser 'zugemutet'."

[5] A. Clebsch wrote in (1871, p. 8): "Unfortunately the beautiful works of that most important geometer are still not widely known; the reason of this can be seen in the fact that in Grassmann's presentations the geometrical results are shown as consequences of much more general and very abstract investigations which in their unusual form cause substantial problems for the reader." ("Leider sind die schönen Arbeiten dieses höchst bedeutsamen Geometers noch immer wenig gekannt; was wohl hauptsächlich dem Umstande zuzuschreiben ist, dass in der Darstellung Graßmanns diese geometrischen Resultate als Corollare viel allgemeinerer und sehr abstrakter Untersuchungen auftreten, die in ihrer ungöhnlichen Form dem Leser nicht unerhebliche Schwierigkeiten bereiten.")

published in 1853 in *Comptes Rendus* a paper "Sur clefs algébrique" in which he described a formal symbolic method which coincides with that of Grassmann's method – but he made no reference to Grassmann. Grassmann complained to the Académie des Sciences that his work had priority. He wrote[6]:

> I recalled at a glance that the principles which are there established and the results which are proved were exactly the same as those which I published in 1844, and of which I gave at the same time numerous applications to algebraic analysis, geometry, mechanics and other branches of physics.

Unfortunately he received no answer.

Not discouraged by those circumstances Grassmann went on to apply his new concepts and methods with the hope that once people will saw that the theory can be successfully applied they would take it seriously. He wrote "Neue Theorie der Elektrodynamik" (1845) and various papers with applications to algebraic curves and surfaces. One of those papers found a recognition – though not full.

In 1846 Möbius invited Grassmann to enter a competition to solve a problem posed by Leibniz: to devise a geometric calculus devoid of coordinates and metric properties (Leibniz used here the term *analysis situs*). Grassmann's work *Geometrische Analyse geknüpft an die von Leibniz erfundene geometrische Charakteristik* (1847) has won (it was the only entry to the competition). On the other hand Möbius, one of the judges, criticized the way in which Grassmann introduced abstract ideas without giving no intuitions which would help the reader to understand them.

Convinced of the value of his theory Grassmann planned in the fifties to write a second volume of the *Ausdehnungslehre* but he abandoned those plans and decided to rewrite completely the work from 1844 in an attempt to have its significance recognized. In this way the book *Die Ausdehnungslehre. Vollständig und in strenger Form begründet* published in 1862 appeared. In the preface he wrote[7]:

> I remain completely confident that the labour I have expanded on the science presented here and which has demanded a significant part of my life as well as the most strenuous application of my powers, will not be lost. It is true that I am aware that the form which I have given the science is imperfect and must be imperfect. But I know and feel obliged to state (though I run the risk of seeming arrogant) that even if this work should again remain unused for another seventeen years or even longer, without entering into the actual development of science, still that time will come when it will be brought forth from the dust of oblivion and when ideas now dormant will bring forth fruit. I know that if I also fail to gather around me (as I have until now desired in vain) a circle of scholars, whom I could fructify with these ideas, and whom I could stimulate to develop and enrich them further, yet there will come a time when these ideas, perhaps in a new form, will arise anew and will

[6] Quotation after Fearnley-Sander (1979, p. 811).
[7] Translation after Fearnley-Sander (1979, p. 817).

> enter into a living communication with contemporary developments. For truth is eternal and divine.

In the introduction Grassmann defended his formal methods – he says that he is setting up an axiomatic theory (this shows that he was ahead of his time). The book was written in much better style than the *Ausdehnungslehre* from 1844 – in fact the style is similar to the style of modern textbooks. Unfortunately the book found no better reception than the first version. Grassmann failed to convince his contemporaries of the significance and meaning of his theory.

At the beginning of the sixties Grassmann published one more work that should be mentioned here. We mean his *Lehrbuch der Mathematik für höhere Lehranstalten. Erster Theil: Arithmetik* (1861). It is important because the first axiomatic presentation of arithmetic was set out in it. It is worth noticing that in Euclid's *Elements* only geometry was presented in an axiomatic way and no axioms for the arithmetic were given. Wang says in (1957) that "the application of the axiomatic method in the development of numbers is not natural" (p. 146). This can explain why the first attempts to axiomatize the arithmetic appeared so late. On the other hand it is clear that if one wants to have "a systematic understanding of the domain of number theory, the axiomatic method suggests itself" (Wang 1957, p. 146).

The introducing of the axiomatic for the arithmetic of natural numbers is usually associated with the name of Giuseppe Peano. But in fact the priority should be granted to Grassmann. Peano made extensive use of Grassmann's work in his development of the arithmetic in his *Arithmetices principia nova methodo exposita* from 1889 (cf. Peano 1889a) what he acknowledges writing explicitly in the introduction[8]:

> For proofs in arithmetic, I used *Grassmann 1861*.

Grassmann's *Lehrbuch der Arithmetik* was probably the first serious attempt to provide a more or less axiomatic basis for the arithmetic. He dealt not with natural numbers but with the totality of integers. However his approach can be reformulated to characterize just natural numbers. Grassmann was probably the first to introduce recursive definitions for addition and multiplication. He proved on the basis of those definitions ordinary laws of arithmetic using mathematical induction. Unfortunately there are some defects in his presentation. There is, for example, no mention of the fact that different numbers have different successors and that 1 is not the successor of any number. There are also no explicitly given rules of inference. Nevertheless *Lehrbuch der Arithmetik* shows that – as Hermann Hankel (1839–1873) said – Grassmann can be seen as the first mathematician who tried to strictly observe the construction of a theory of quantity

[8]He referred also to the Dedekind's work (1888) writing that it "was also most useful".

from a pure theory of forms (*Formenlehre*). The basis of the method used in this work can be seen in his *Ausdehnungslehre* from 1844.

* * *

As mentioned above, Grassmann's works and ideas did not find an appropriate appreciation by the contemporaries and were in fact largely ignored. But his ideas and methods were so deep that they inspired several mathematicians and influenced further developments in mathematics.

Hamilton has mentioned Grassmann and his *Ausdehnungslehre* in *Lectures on Quaternions* from 1853 but this had no further consequences. The only mathematician who appreciated Grassmann's ideas during his lifetime was H. Hankel. His work *Theorie der complexen Zahlensysteme* from 1867 helped to make Grassmann's ideas better known – he credited Grassmann's *Ausdehnungslehre* as giving the foundation for his work. Henkel's *Theorie* called in 1869 Felix Klein's (1849–1925) attention to the work of Grassmann. Klein told about Grassmann his colleague Alfred Clebsch (1833–1872). Thanks to efforts of Clebsch was Grassmann in 1871 elected to Göttinger Gesellschaft der Wissenschaften.

The ideas of Grassmann have been used and further developed by various mathematicians, they were inspiration for many investigations. Let us indicate some of them.

Grassmann's work *Lehrbuch der Mathematik für höhere Lehranstalten. Erster Theil: Arithmetik* (1861) was the main source for Ernst Schröder's (1841–1902) *Lehrbuch der Arithmetik und Algebra* published in 1873 in which he considered seven algebraical operations (adding, multiplying, raising to a power, subtracting, dividing, extracting the roots and forming the logarithms) and defined (pure) mathematics as the "doctrine of numbers"[9] (cf. 1873, p. 2). Its influence on Peano and his axiomatic development of arithmetic is evident – as indicated above, Peano acknowledged this influence in (1889a). It is worth noting here that Peano – what was new – increased in his work the role of logic in mathematics – more exactly, wished to show how logic could help mathematics.

The influence of Grassmann's *Ausdehnungslehre* was really big. It inspired Felix Klein and his Erlanger Program. In the latter one finds repeated references to Grassmann. It influenced also Peano's development of the theory of linear spaces in his work *Calcolo geometrico secondo l'Ausdehnungslehre di H. Graßmann preceduto dalle operazioni della logica deduttiva* (1888). Peano credited there Leibniz', Möbius' and Grassmann's work as well as Hamilton's work on quaternions as providing ideas which led him to his formal calculus. Peano gave in this book the basic calculus of set operation and introduced the modern notation. He also used and indeed popularized some notations of Grassmann. Like

[9]Notice that for Grassmann mathematics was the "science of the connection of quantities" (cf. 1861, p. 1). Note also that Schröder defined the notion of number without determining what kind of objects numbers are.

Grassmann he stressed commutativity, distributivity and associativity where applicable. In Chapter IX Peano gave axioms for a linear space.

Grassmann's work is indicated as one of the main sources of inspiration by Walther von Dyck (1856–1934) in his abstract conception of groups. It inspired also Alfred North Whitehead's (1861–1947) *A Treatise on Universal Algebra* (1898). This book included the first systematic exposition in English of the theory of extension and the exterior algebra. Whitehead acknowledged the influence of Grassmann writing in (1898, p. 32):

> The discussions of this chapter are largely based on the 'Uebersicht der allgemeinen Formenlehre' which forms the introductory chapter to Graßmann's Ausdehnungslehre from 1844.

The theory of extension led to the development of differential forms and to the application of such forms to analysis and geometry. Differential geometry makes use of the exterior algebra developed by Grassmann. He contributed also to the development of the vector and tensor calculus. His works play also a role in contemporary mathematical physics (cf. Penrose 2004, Chapters 11 and 12).

As we see, though not appreciated during his life, Hermann Günther Grassmann played a very important role – by his original ideas – in the development of mathematics. As Fearnley-Sanders writes (cf. 1979, p. 816):

> All mathematicians stand, as Newton said he did, on the shoulders of giants, but few have come closer than Hermann Grassmann to creating, single-handedly, a new subject.

Grassmann's name appeared not only in technical works but has the firm place in textbooks in the history of mathematics. It remains also in some technical terms named after him, in particular: Grassmann number, Grassmann algebra, Grassmannian, Grassmann's laws in phonology and optics.

# 12

# GIUSEPPE PEANO AND SYMBOLIC LOGIC[1]

If we asked an average mathematician with what he associates the name 'Peano', he would probably answer: "With two things: axioms for natural numbers and with the curve filling a square". It sounds rather unsatisfactory when we compare it with the full attainments and merits of Peano. It shows that his rôle in the development of mathematics and logic is not well known. This situation is perhaps a consequence of the fact that many of Peano's achievements and ideas have been acquired by mathematicians and are now commonly used. In such situations one forgets the creator and thinks that all these things existed from the very beginning and there was no need to discover them. On the other hand the knowledge about those of Peano's achievements which are a bit more known is rather superficial and inadequate.

In this paper I would like to discuss briefly Peano's scientific achievements paying special attention to the rôle he has played in the development of mathematical logic and the foundations of mathematics.

Giuseppe Peano was active and worked in three fields: analysis, logic and, in the last period of his life, comparative linguistics and international language (he has published 216 papers and 15 books, 193 of them were devoted to mathematics and logic, 38 to linguistics). Though he is known first of all as a pioneer of symbolic logic and as a creator and propagator of the axiomatic method, he himself considered as most important his papers devoted to analysis. When he published in 1915 a list of his publications he supplied it with the following remark: "My papers are devoted mostly to analysis and they proved to be not quite useless since, in the opinion of some competent persons, they have contributed to the development of this part of mathematics" (cf. Kennedy 1973a, p. 15). Talking about "the opinion of some competent persons" G. Peano thought here

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about *Enzyklopedie der mathematischen Wissenschaften* where A. Pringsheim quoted two books of Peano among 19 most important papers in analysis since Euler and Cauchy.

The first Peano's paper in the field of analysis was one devoted to the integrability of functions published in 1883. He has introduced there original concepts of an integral and of an area. In (1885–1886) he has proved the theorem on the existence of a solution of the first-order differential equation $y' = f(x, y)$ on the condition that $f$ is continuous. This result was generalized by him in (1890a) to the case of a system of differential equations (he has used this time another method). It is worth mentioning that just in this paper we find the first explicit, if negative, allusion to the axiom of choice[2] (of course Cantor and others were using this axiom before Peano but they were not conscious of the fact that they are using the principle which is not quite obvious and needs explanation).

Peano's contributions to the analysis are not only his original results but also the fact that he has explained and made precise many concepts which were used imprecisely – he has done it mainly by providing various counterexamples. Many such counterexamples can be found already in his book written on the basis of A. Genocchi's lectures (Peano was as assistant to and later substitute for A. Genocchi at the University of Turin) and published under the title *Calcolo differenziale e principii di calcolo integrale, publicato con aggiunte del Dr. Giuseppe Peano* (1884).

His best known counterexample is the example of a curve filling a square given in the paper (1890b). The curve is given by continuous parametric functions $x = x(t)$ and $y = y(t)$ and is such that as $t$ varies throughout the unit interval $I$, the graph of the curve includes every point of the unit square $I \times I$. This was the counterexample to the commonly accepted notion that an arc of a curve given by continuous parametric functions could be enclosed in an arbitrary small region.[3] Peano has published his result without any diagram (to avoid suspicions that his proof is not complete and contains gaps). His proof was purely analytic. The first graphic representation of Peano's curve was given by D. Hilbert. Nevertheless we may suppose that Peano discovered the curve filling the unit square considering various diagrams. He was so proud of his discovery that, as H.C. Kennedy writes in (1973a), "he has one of the curves in the sequence put on the terrace of his house, in black tiles on white" (p. 13).

Let us turn now to the logical papers of Giuseppe Peano and consider his contributions to mathematical logic and the foundations of mathematics. As his

[2]Peano wrote: "However, since one cannot apply infinitely many times an arbitrary law which one assigns (*on fait correspondre*) to a class an individual of that class, we have formed here a definite law by which, under suitable assumptions, one assigns to every class of a certain system an individual of that class" (see p. 210).

[3]S. Mardešić showed in (1960, p. 88, Theorem 4) that the only non-degenerate ordered continuum $C$ which admits a mapping $f : C \longrightarrow C \times C$ being one-one and onto is the real line segment $I$.

first work in this domain one can treat his 20 page introductory chapter to the book (1888). This chapter is quite independent and has no connections to the following ones. Peano's considerations are based here on some works of G. Boole, E. Schröder, C.S. Peirce and others. He notices, and he will repeat it later in many places, the equivalence of the algebra of sets and of the propositional calculus. Many symbols introduced in this paper will not be used later – they will be replaced by better and more adequate ones.

Already in this paper we can see that G. Peano set great store by mathematical symbolism. He considers this problem in many of his papers. The propagation of a symbolic language in mathematics and the invention of a good symbolism is just one of his greatest merits. The fact that we can read Peano's papers so easily today proves among others that many symbols he introduced and used have been adopted directly or in a slightly modified form in the language of modern mathematics and logic.

It is worth mentioning the connections of this activity of Peano with the program of Leibniz. This program was well known to Peano. He often mentioned Leibniz's ideas. He also encouraged one of his students and latter an assistant G. Vacca to study carefully the manuscripts of Leibniz in Hannover. This was a fruitful project.

Leibniz, "from his childhood acquainted with the scholastic logic, was enchanted by the idea of a method which would reduce all notions used by human beings to some primitive notions constituting in such a way 'an alphabet of human thought' and form combinations of them in a mechanical way to obtain all true sentences" (cf. Bourbaki 1960). These dreams and plans were presented by him in an attempt to design a universal symbolic language called *characteristica universalis*. It was supposed to be a system of signs fulfilling the following conditions (cf. Scholz 1959):

1. There is a one-one correspondence between the signs of the system (provided they are not signs of empty places) and thoughts (in the most broad sense).
2. The signs must be chosen in such a way that if an idea (thought) can be composed into components then the sign for this idea will have a parallel decomposition.
3. One must devise a system of rules to operate on the signs such that if an idea $M_1$ is a logical consequence of an idea $M_2$ then the "picture" of $M_2$ can be interpreted as a consequence of the "picture" of $M_1$.

Leibniz did not succeed in realizing this program. As a partial realization of it one can consider the notion of a formalized language coming from the twentieth century (it is only a partial realization because it concerns mainly the language of mathematics and Leibniz dreamed about a universal language which would enable us to express in a symbolic way any thought and to analyze any reasoning).[4] The history of mathematics and logic, in particular their development in the 19th and 20th centuries, show how difficult it was to realize this program. One must mention here works of G. Boole, E. Schröder, G. Frege and of course G. Peano.

Peano considered the realization of Leibniz's ideas as a main task of the mathematical sciences. He has expressed this opinion in many of his papers. Almost the whole of his activity in the field of logic and foundations of mathematics went in this direction.

Leibniz's *characteristica universalis* is, as we have shown above, first of all a symbolic language in which one may formulate statements. And, as Peano claims so often:

> symbols represent not words but ideas. Therefore one must use the same symbol whenever the same idea appears, even if in the colloqual language they were not represented by the same expression. In this way we establish a one-one correspondence between ideas and symbols – such a correspondence cannot be found in the colloquial speech. This ideography is based on theorems of logic discovered since the time of Leibniz. The shape of symbols can be changed, i.e., one can change some signs representing the basic ideas, but there cannot exist two essentially different ideographies. (1896–1897, p. 573)

The symbolic language enables us not only to be more precise and clear but it helps also to understand better some things, it "enables the pupils of a secondary school to solve easily problems which in the past could be solved only by great minds as Euclid or Diophantus" – as Peano wrote in (1915, p. 168).[5] Just in this paper Peano gives a historical review of introducing symbols to mathematics and shows precisely the advantages which it brings: so first of all the shortness of expressions needed to state theorems and proofs, the small number of basic symbols with the help of which one builds further more complicated expressions ("in the colloquial language one needs about thousand words to express the logical relations, in the symbolic language of mathematical logic ten symbols are enough. [. . . ] professor Padoa has reduced this number of necessary symbols to three" – 1915, p. 169). Further, a symbolism facilitates reasoning, helps even to obtain new results. Moreover, in some cases the usage of a symbolic language is simply necessary because we have no suitable expressions in the colloquial language to state some ideas or constructions (with such a necessity were confronted for example, as Peano writes in (1915), A.N. Whitehead and B. Russell when they were writing *Principia Mathematica* – "the greatest work written completely with the help of symbols" – p. 167).

[4]It is worth mentioning that Leibniz hoped that *characteristica universalis* would help in particular to decide any philosophical problems. He wrote: "And when this will come [i.e., when the idea of universal language will be realized – R.M.] then two philosophers wanting to decide something will proceed as two calculators do. It will be enough for them to take pencils, to go to their tablets and say: 'Let us calculate!'." (1875–1890, vol. VII, p. 200)

[5]Cf. also (1910) where he claims that this method ought to be more widely used in schools, for it facilitates "the analysis of some difficult mathematical problems" (p. 36).

As we have shown above, the idea of Leibniz's *characteristica universalis* consists not only of a suitable symbolism but also of a method which would enable us to investigate the relation of logical consequence, to prove theorems about various mathematical objects on the basis of some axioms fixed at the beginning – hence an axiomatic-deductive method. G. Peano has done very much also in this domain, particularly in axiomatizing arithmetic and geometry.

We must mention here first of all his famous work *Arithmetices principia nova methodo exposita* (1889a). This small booklet with a Latin title (to be precise, almost Latin – the word 'arithmetices' is only a transliteration of an appropriate Greek word, in Latin it ought to be 'arithmeticae'), written also in Latin (more exactly: in *latino sine flexione* – see below) contains the first formulation of Peano's postulates for natural numbers.

The work consists of a preface and a proper part. In the preface needed notions are introduced and a symbolism is fixed. G. Peano writes at the beginning:

> I have represented by signs all ideas which appear in the foundations of arithmetic. With the help of this representation all sentences are expressed by signs. Those signs belong either to logic or to arithmetic. [...] By this notation every sentence become a shape as precise as an equation in algebra. Having sentences written in such a way we can deduce from them other sentences – it is done by a process similar to a looking for a solution of an algebraic equation. This was exactly the aim and the reason for writing this paper. (1889a, p. 3)

It is worth mentioning that it is just in *Arithmetices principia* that the symbol $\varepsilon$ was used for the first time to denote the membership relation as well as the symbol $\supset$ to denote inclusion[6] ('$a \supset b$' was read by Peano as: $a$ is included in $b$). There is also a remark that those two relations must be strictly distinguished.[7] In the preface we find also various theorems of the propositional calculus, of the algebra of classes and of the predicate calculus. The symbolism introduced here is much better than that which was used by G. Boole and E. Schröder. G. Peano distinguishes also between the propositional calculus and the algebra of classes treating them as two different calculi and not as two interpretations of one calculus. A new and useful notation for the universal quantifier is introduced:[8] namely if $a$ and $b$ are two formulas containing free variables $x, y, \ldots$ then Peano writes $a \supset_{x,y,\ldots} b$ to denote what we would write today as $\forall x \forall y \ldots (a \longrightarrow b)$.

After having introduced the symbolism and after some preliminary remarks G. Peano begins the proper part of the paper, i.e., the development of arithmetic. This part is written fully in the symbolic language! He starts from his famous ax-

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[6]The same symbol was used by Peano to denote also a relation between sentences; namely if $a$ and $b$ are sentences then "$a \supset b$" means "$b$ follows from $a$". It seems that he did not distinguish yet implication and logical consequence.

[7]Except 1-element classes. The distinction between $x$ and $\{x\}$ appears only in a paper written one year later, namely in (1890a), p. 192.

[8]Let us notice that Peano was the first who distinguished between free and bounded variables – he called them, resp., real and apparent variables (cf. Bocheński 1962, p. 409).

ioms for natural numbers. The arithmetical primitive notions are the following: number ($N$), one (1), successor ($x+1$) and equality (=). He formulates four identity axioms (today we treat them as logical axioms) and five proper arithmetical ones. In his original notation they look like this:

$$1 \,\varepsilon\, N,$$

$$a \,\varepsilon\, N \,.\supset.\, a+1 \,\varepsilon\, N,$$

$$a, b \,\varepsilon\, N \,.\; \supset:\; a = b \,.\; = \;.\, a+1 = b+1,$$

$$a \,\varepsilon\, N. \supset .\, a + 1 - = 1,$$

$$k \,\varepsilon\, K \therefore 1 \,\varepsilon\, k \therefore x \,\varepsilon\, N \,.\, x \,\varepsilon\, k \;:\supset_x\; .\, x+1 \,\varepsilon\, k \;::\supset\; .\, N \supset k$$

where $K$ denotes the family of all classes. In the notation used today they would be written as follows:

$$1 \in N,$$

$$a \in N \longrightarrow a+1 \in N,$$

$$a, b \in N \longrightarrow (a = b \longleftrightarrow a+1 = b+1),$$

$$a \in N \longrightarrow (a+1 \neq 1),$$

$$[k \in K \,\&\, 1 \in k \,\&\, \forall x(x \in N \,\&\, x \in k \longrightarrow x+1 \in k)] \longrightarrow N \subset k.$$

He deduces from these axioms various arithmetical theorems introducing on the way some new notions.[9] He considers not only natural numbers and their arithmetic but also fractions, reals and even a notion of a limit and some definitions from set theory.[10]

Observe that Peano's system is not exactly the system which we call today Peano arithmetic. For the latter is a first order system based on the first order predicate calculus and the original system of Peano was not of the first order. It was formulated in a language with sets, classes and the membership relation. Hence it was a system based on set theory.

It would be good to pay here more attention to the way of deducing theorems from the axioms used by Peano. It is not a deduction in a today's sense of the word. For Peano had in his system no explicitly formulated rules of inference. Hence there are no formalized proofs. We have simply sequences of formulas – sequences which we meet in usual, "normal" proofs used in mathematical practice. This is perhaps a consequence of the fact that Peano was interested in logic only as in a tool which helps us to order the mathematics and to make it

[9]It is worth mentioning that Peano uses definitions by recursion though they do not satisfy the conditions he put on definitions – he claims namely that a definition must be of the form $x = a$ or $\alpha \supset x = a$ where $\alpha$ is a certain condition, $x$ is a notion being defined and "$a$ is a set of signs with a known meaning" (1889a, p. 5). There is also no theorem similar to Theorem 126 from the paper of R. Dedekind (1888) which would justify definitions by recursion.

[10]Peano proved that his axioms for natural numbers are independent in his later paper (1891a).

more clear. He was not interested in logic itself. Hence he was satisfied, as all "normal" mathematicians are, to rely on an intuitive notion of deduction from the axioms. He did not reduce everything to subtle and precise considerations of the formal logic where each step of the proof must be based on some rule of inference fixed at the beginning. Hence the works of Peano were not so valuable for the logic itself as, for example, subtle and penetrating works of G. Frege, the German logician living at the same time. But they were very important from another point of view – namely they showed how to express, how to formulate mathematical theories in a symbolic language. Peano believed also that this will help to answer the question: "how to recognize a valid mathematical proof?".[11]

The considerations of Peano in *Arithmetices principia* were based, of course, on works of his predecessors. He used for example partially the logical symbolism of G. Boole and E. Schröder. In writing down proofs of various theorems of arithmetic he used some parts of H. Grassmann's *Lehrbuch der Arithmetik* (1861). But the most interesting things are the connections of Peano's *Arithmetices principia* with the work of R. Dedekind *Was sind und was sollen die Zahlen?* (1888).

It is not clear whether Peano's discovery of the axioms for natural numbers was independent of the work of Dedekind. Peano wrote in *Arithmetices principia* that the Dedekind's work was "quite useful" for him. This sentence and another one, namely: "The preceding primitive propositions are due to Dedekind" (1891a, p. 99) serve as a basis for an argumentation that Peano took his axioms from the work of Dedekind. In this way argue for example J. van Heijenoort (who writes in (1967, p. 83): "Peano acknowledges that his axioms come from Dedekind") and Hao Wang ("It is rather well known through Peano's own acknowledgement [...] that Peano borrowed his axioms from Dedekind [...]", Wang 1957, p. 145). On the other hand H.C. Kennedy claims that Peano has acknowledged only in the above sentences Dedekind's priority of publication (cf. Kennedy 1972). He cites the words of Peano who wrote in (1896–1899):

> The composition of my work of 1889 was still independent of the publication of Dedekind just mentioned; before it was printed I had moral proof of the independence of the primitive propositions from which I started, in their substantial coincidence with the definitions of Dedekind. Later I succeeded in proving their independence. (p. 76)

In this way H.C. Kennedy (1972, p. 135) comes to the conclusion that:

> the reference to Dedekind's work was added to the preface of *Arithmetices principia* just

[11] He wrote: "[...] this question can be given an entirely satisfactory solution. In fact, reducing the propositions [...] to formulas analogous to algebraic equations and then examining the usual proofs, we discover that these consists in transformations of propositions and groups of propositions, having a high degree of analogy with the transformation of simultaneous equations. These transformations, or logical identities, of which we make constant use in our argument, can be stated and studied." (1889b, p. 53).

> before the pamphlet went to press, and we have an explanation of how Dedekind's work was "useful".

Hence the problem of Peano's or Dedekind's priority seems to be rather undecidable. All we can do in this situation is to compare their sets of axioms for natural numbers. Axioms of Peano were given above. Dedekind formulated his axioms in a different language. A set $K$ (Dedekind says: a system $K$) is called by him a chain (*eine Kette*) if and only if there exists a mapping $\varphi : K \to K$ such that $\varphi(K) \subset K$, where $\varphi(K)$ is the image of $K$ with respect to $\varphi$. A chain of an element $a$ is the intersection of all chains containing $a$. We denote this chain by $a_0$. A set $S$ is said to be infinite (*unendlich*) if and only if there exists a one-one mapping $\varphi : S \to S$ such that $\varphi(S)$ is a proper subset of $S$. A set $N$ is said to be simply infinite (*einfach unendlich*) if and only if there is a one-one mapping $\varphi : N \to N$ such that there exists an element $a \in N - \varphi(N)$ with the property that $N$ is the chain of the element $a$. In this case the element $a$ will be denoted by 1 and will be called the basic element (*Grundelement*) of $N$. Observe that $\varphi$ generates an ordering of $N$. It can be proved that any infinite set $S$ contains (as a subset) a simply infinite set $N$. Dedekind writes now (1888, p. 17):

> If one studies a simply infinite set $N$ ordered by a mapping $\varphi$ abstracting from its particular features of its elements and takes into account only the fact that its elements are distinguishable and that they are ordered by $\varphi$, then one calls them natural numbers or ordinal numbers or simply numbers and the basic element 1 is called the basic number of the sequence of numbers $N$ (*die Grundzahl der Zahlenreihe N*). Therefore (the process of abstraction!) numbers can be called free creation of the human spirit (*freie Schöpfung des menschlichen Geistes*).

Regardless of the solution of the problem of Peano's or Dedekind's priority it remains Peano's merit that he has given the axioms for natural numbers in a nice, precise and clear form and – this is the most important thing – has shown how to derive from them theorems of arithmetic.[12]

At the end of the introduction to *Arithmetices principia* Peano wrote:

> My book ought to be considered as a sample of this new method. Using the notation which we have introduced one can formulate and prove infinitely many other theorems, in particular theorems about rational and irrational numbers.

He was convinced that with the help of the introduced symbols, perhaps supplemented by some new symbols denoting new primitive notions, one can express in a clear and precise way theorems of every science.

In his succeeding papers G. Peano successively showed how to apply this method to mathematics.[13] He wrote various papers reducing to a minimum the

[12] It is interesting how Dedekind obtained his characterization of natural numbers. Some information is contained in the very interesting letter of Dedekind to H. Keferstein of 27th February 1890 – cf. Wang (1957) and Heijenoort (1967).

[13] Peano attempted also to treat logic according to the symbolic and axiomatic method. Cf. his

usage of colloquial language. One should mention here the monograph *I principii di geometria logicamente esposti* (1889b) – hence published in the same year as *Arithmetices principia*. He used here logical and arithmetical symbols and some additional special geometrical symbols. His system of geometry is based on two primitive notions, namely on the notions of point and of interval. It is similar to the approach of Pasch. Geometry is just the second domain of mathematics where Peano successfully applied his symbolic-axiomatic method. Investigating the foundations of geometry G. Peano popularized the vector method of H. Grassmann and brought it to the perfection. In this way he gave also a certain impulse to the Italian school of vector analysis.

The idea of presenting mathematics in the framework of an artificial symbolic language and of deriving then all theorems from some fundamental axioms was also the basic idea of Peano's famous project "Formulario". It was proclaimed in 1892 in the journal *Rivista di Matematica*. This journal was founded by Peano in 1892 and here his main logical papers and papers of his students were published. The aim of this project was to publish all known mathematical theorems. This plan was to be carried out using, of course, the symbolic language introduced by Peano. A special journal *Formulario Mathematico* was founded to publish the results of this project.[14] It was edited by Peano and his collaborators: Vailati, Castellano, Burali-Forti, Giudice, Vivanti, Betazzi, Fano and others. Five volumes appeared: Introduction 1894, vol. I – 1895, vol. II – 1897–1899,[15] vol. III – 1901, vol. IV – 1903, vol. V – 1908. The last volume contained about 4200 theorems! Peano even bought a small printing office (in 1898 from Faá di Bruno for 407 lira) and studied the art of printing. One of the results of the project was a further simplification of the mathematical symbolism. Peano treated "Formulario" as a fulfilment of Leibniz's ideas.[16] He hoped that it will be widely used by professors and students. He himself immediately began to use it in his classes. But the response was very weak. One reason was the fact that almost everything was written there in symbols, the other was the usage of *Latino sine flexione* for explanations and commentaries (instead of, for example, French).

The most striking and remarkable feature of Peano's symbolism is exactly its simplicity. Just for this reason it has been accepted by almost all mathematicians and became a common property. It was a result of Peano's genius. One can see it easily by comparing his symbolism with the symbolism of G. Frege. Works of

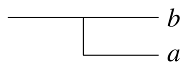
paper (1891b) which contains an analysis of the propositional calculus based on 4 primitive notions and 12 axioms.

[14]More exactly the first three volumes were published under the title *Formulaire de Mathématiques*, the forth one was titled *Formulaire Mathématique* (they were written in French) and the fifth volume (written in *Latino sine flexione* – see below) – *Formulario Mathematico*.

[15]Vol. II was published in three parts, resp., in 1897, 1898 and 1899.

[16]He wrote: "After two centuries, this 'dream' of the inventor of the infinitesimal calculus has become a reality [...]. We now have the solution to the problem proposed by Leibniz." (1915, p. 168)

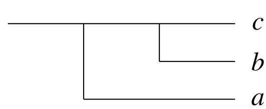
Frege, though deeper, more systematic and precise were unknown during a long time just because they were written in a very complicated and "hard" symbolism.[17] The superiority of Peano's symbolism follows, among other things, from the fact that it is linear as are the common writing and colloquial speech. According to Frege flat (two-dimensional) pictures had to be drawn. Fore example the implication "if *a* then *b*" was written by Peano as: $a \supset b$, while Frege wrote:




In the case of a double implication "if *a* then if *b* then *c*" the notation by Peano and Frege was, resp., the following:

$$a \,.\!\supset b \supset c,$$

and




(A point denotes here a parenthesis. Peano used a whole system of points as parentheses – there is some analogy with the notation proposed by Leibniz.)

To be frank we must say that the idea of presenting the whole of mathematics in an artificial symbolic language was not easily accepted by all mathematicians, logicians and philosophers. There was certain resistance. There is known, for example, the disdain of the great French mathematician Henri Poincaré. He wrote:

> The essential part of this language consists of some algebraic symbols denoting connectives: if, and, or. Maybe they are useful, but if they will help to renew the whole philosophy is another question. It is hard to suppose that the word "if" as written in the form $\supset$ gains some new power. This invention of Peano was called in former times a pasigraphy, i.e., the art of writing a mathematical treatise using no word of the colloquial language. This name indicates very well the applicability of this art. Later on it became more dignified by being called logistic. This word is used in Military Schools to denote the art of guiding and placing apart the army in the camp (in French: *maréchal des logis*); it is clear that the new logistic had nothing to do with that, that the new name claims to do a "revolution in logic". (1908, pp. 166–167)

[17] It is worth noting here that Peano was one of the few before 1900 who gave serious recognition to Frege's work. They corresponded, sent each other copies of their papers, exchanged comments and remarks on them. Their aims in creating an ideography were distinct. Frege was more interested in investigations into the foundations of mathematics whereas Peano wished to create a symbolism which will help to express mathematical theories. Frege has admitted that Peano's symbolism was easier for the typesetter and that it often took less room but simultaneously has written that "the convenience of the typesetter is not the highest good" (cf. 1976, p. 246).

We have stated that Peano was interested in logic only as a tool to investigate mathematics and to order it and make it more clear according to the axiomatic-deductive method. As a consequence almost all his logical papers contain many examples of applications of logical methods in various parts of mathematics. On the other hand this restriction has caused that his results in logic are not so deep as, for example, the results of Frege whom we have mentioned already. Nevertheless his influence on the development of mathematical logic was great. In the last decade of the 19th century Peano and his school played first fiddle. Later the center was moved to England where Bertrand Russell was active – but Peano played also here an important rôle. Russell met Peano at the International Philosophical Congress in Paris in August 1900 and this meeting was described then in his *Autobiography* as "a turning point in my intellectual life" (1967, p. 178).[18]

Working on the project "Formulario" Peano paid great attention to definitions of new notions in terms of primitive notions. This was the reason of his great interest in the problem of definitions. Between 1911 and 1915 Peano wrote six papers having the word "definition" in the title. The paper (1921) contains the basic rules and principles of defining new concepts in mathematics which were accepted by Peano. He states here first of all that every definition should be an equation or an equivalence (and that this rule can be found already by Aristotle), all definitions in mathematics ought to be nominal; Aristotle's principle *"per genus proximum et differentiam specificam"* is not applicable to them. He writes, referring to Aristotle again, that the object which is being defined has not necessarily to exist. The condition of homogeneity of definitions is also formulated – it says that *definiendum* and *definiens* must contain the same free variables. Definitions are useful though not necessary because *definiendum* can be always replaced by *definiens*. Moreover, this elimination procedure gives a criterion, coming already from Aristotle, of the usefulness of a definition: "if after the elimination of definiendum the expression is not longer and more compli-

[18]Russell (1967) wrote: "The Congress was a turning point in my intellectual life, because I met there Peano. I already knew him by name and had seen some of his work, but had not taken the trouble to master his notation. In discussions at the Congress I observed that he was always more precise than anyone else, and that he invariably got the better of any argument upon which he embarked. As the days went by, I decided that this must be owing to his mathematical logic. I therefore got him to give me all his works, and as soon as the Congress was over I retired to Fernhurst to study quietly every word written by him and his disciples. It became clear to me that his notation afforded an instrument of logical analysis such as I had been asking for years, and that by studying him I was acquiring a new and powerful techniques for the work I had long wanted to do." (p. 178–179)

In 1912 B. Russell wrote in a letter to Jourdin (cf. Kennedy 1973b, p. 369): "Until I got hold of Peano, it had never struck me that Symbolic Logic would be any use for the Principles of mathematics, because I knew the Boolean stuff and found it useless. It was Peano's, together with the discovery that relations could be fitted into his system, that led me to adopt symbolic logic."

It was also through Peano that Russell learned of Frege and his works – cf. Kennedy (1973b, p. 370).

cated as it was before then there was in fact no need to introduce this definition" (1921, 184).

Considering G. Peano's logical works one cannot forget his interest in the linguistics and in a project of an international language. His first papers devoted to this subject appeared in 1903. After 1908, when he was elected the president of the Academy Volapük, called later Academia pro Interlingua, he devoted himself completely to it.

Peano's linguistic investigations grew out of his work in logic, especially from his interests in mathematical and logical symbolism. They are also connected with some ideas of Leibniz – in particular with *characteristica universalis*. Peano has called the language which he created *latino sine flexione* (Latin without inflection). Its most striking feature is just the reduction of grammatical rules to a minimum. It could be briefly characterized as follows:

1. Every word which is common to European languages is accepted (this postulate seems to be a bit imprecise and in fact it was understood in various ways even by Peano – once (in 1915) he said that the whole vocabulary of the new language must come from Latin, later (in 1927) he wrote that the vocabularies of English, French, Spanish, Italian, Portuguese, German and Russian must be accepted).
2. Every international word coming from Latin is given in a form of its Latin root.
3. Plural is built by adding the ending -s.
4. Essentially only roots are used, there are almost no endings (except the ending -s in plural); cases are built with the help of prepositions: de, ad, pro, ab, in, etc.; there are no genders, no articles, there is no compliance in number (singular and plural), case, person or gender; there is no conjugation – tenses and moods are described with the help of words: si, que, ut, jam, etc.

In 1915 G. Peano published *Vocabulario commune ad latino-italiano-français-english-deutsch* which was an important and significant step in creating the new language.

G. Peano did not claim that this language will replace national ones. That was not the purpose. His aim was to build a language which could help in communicating in the international area. G. Peano himself wrote many mathematical and lingustic papers in this language. As a sample of it let us quote a fragment of Peano's commentary concerning the foundations of arithmetic (cf. 1901, p. 27 – we quote after Kneale 1962, pp. 473–474):

> Quaestio si nos pote defini $N_0$ significa si nos pote scribe aequalitate de forma
>
> $$N_0 = \text{expressione composito per signos noto, } \cup, \cap, -, \ldots, \iota,$$
>
> quod non es facile. Ergo nos sume tres idea $N_0$, $o$, $+$ ut idea primitivo per que nos defini omni

symbolo de Arithmetica. Nos determinata valore de symbolo non definito per systema de proposito primitivo sequente.

The attempts to realize the idea of an interlingua failed. The lack of a precise criterion which part of Latin and of other languages has to be adopted and which part has to be rejected caused the existence of many "dialects" of it. They were very different and it was difficult to unify them. This was the reason that the language was rejected even by its enthusiastic adherents. In the late forties A. Gode, supported by the International Auxiliary Language Association, changed a bit the idea of an interlingua and made it a bit more alive. He promoted it as a language which is very useful in communication especially among scientists. But the results of this activity are rather mediocre.

* * *

Coming to conclusions, we see that Giuseppe Peano has played a great rôle in the development of mathematical logic and the foundations of mathematics – greater than one could judge taking into account how much attention is paid to him in books devoted to the history of logic (cf. Bocheński 1962 and Kneale 1962). The main idea which one can spot in his papers and which has directed his whole activity was the demand for clarity and precision. Realizing this idea he proposed a good and useful symbolism to be used in mathematics. How good it was is proved by the fact that it has been adopted almost without changes by modern mathematics. Peano has shown by many examples how to express mathematics in the framework of this symbolic language. To clarify and to order mathematical knowledge he promoted the axiomatic-deductive method (using his symbolic language) and showed by many examples how to apply it in particular parts of mathematics (arithmetic, geometry). Logic was for him a tool which helps to clarify and to make more precise mathematical reasonings. The usage of a symbolism ought to help us to see better the logical connections and interrelations between particular sentences in arguments and to analyze the relation of logical consequence. It is worth noting that Peano made no attempts to found mathematics on any discipline. As a consequence he has denied the validity of Russell's logicism, i.e., the reduction of mathematics to logic. He was interested only in reducing a given mathematical theory to a minimal number of axioms expressed in a language with the minimal number of primitive terms from which other theorems can be derived and other notions of the theory can be defined. Peano was also not a formalist in the sense of Hilbert. He believed that mathematics can be and ought to be based on experience.[19]

[19]Peano wrote in the paper (1906): "But a proof that a system of postulates for arithmetic, or for geometry, does not involve a contradiction, is not, according to me, necessary. For we do not create postulates at will, but we assume as postulates the simplest propositions that are written, explicitly or implicitly, in every text of arithmetic or geometry. Our analysis of the principles of these sciences is a

Taking into account the further development of mathematical logic and the foundations of mathematics we can surely call Giuseppe Peano the pioneer and promoter of symbolic logic.

reduction of the ordinary affirmations to a minimum number, that which is necessary and sufficient. The system of postulates for arithmetic and geometry is satisfied by the ideas of number and point that every writer of arithmetic and geometry has. We think number, therefore number exists.

"A proof of consistency of a system of postulates can be useful, if the postulates are hypothetical and do not correspond to real facts" (p. 364).

In 1923 Peano wrote (cf. Kennedy 1973a, p. 17): "Mathematics has a place between logic and experimental sciences."

# 13

# E.L. POST AND THE DEVELOPMENT OF LOGIC[1]

In the paper the contribution of Emil L. Post (1897–1954) to mathematical logic and recursion theory will be considered. In particular we shall study: (1) the significance of the results of his doctoral dissertation for the development of the metamathematical studies of propositional calculus, (2) the significance of his studies on canonical systems for the theory of formal languages as well as for the foundations of computation theory, (3) his anticipation of Gödel's and Church's results on incompleteness and undecidability, (4) his results on the undecidabiity of various algebraic formal systems and finally (5) his contribution to establishing and to the development of recursion theory as an independent domain of the foundations of mathematics. At the end some remarks on the philosophical and methodological background of his results will be made.

## 1. Post's Doctoral Dissertation

Most of scientific papers by Emil Leon Post were devoted to mathematical logic and to the foundations of mathematics.[2]

His first paper in logic was the doctoral dissertation from 1920 published in 1921 in *American Journal of Mathematics* under the title "Introduction to a General Theory of Elementary Propositions" (cf. Post 1921). It was written under the influence of A.N. Whitehead's and B. Russell's *Principia Mathematica* (cf. 1910–1913) on the one hand (Post participated in a seminar led by Keyser at Columbia University devoted to *Principia*) and *A Survey of Symbolic Logic*

[1] Originally published under the title "E.L. Post and the Development of Mathematical Logic and Recursion Theory", *Studies in Logic, Grammar and Rhetoric* 2 (15) (1998), 17–30. Reprinted with kind permission of Wydawnictwo Uniwersytetu w Białymstoku (Publishing House of the University in Białystok).

[2] Post published 14 papers and 19 abstracts (one of the papers was published after his death): 4 papers and 8 abstracts were devoted to algebra and analysis and 10 papers and 11 abstracts to mathematical logic and foundations of mathematics.

In: Roman Murawski, *Essays in the Philosophy and History of Logic and Mathematics* (*Poznań Studies in the Philosophy of the Sciences and the Humanities*, vol. 98), pp. 183–193. Amsterdam/New York, NY: Rodopi, 2010.

by C.I. Lewis (1918). Post's doctoral dissertation contained the first metamathematical results concerning a system of logic. In particular Post isolated the part of *Principia* called today the propositional calculus, introduced the truth table method and showed that the system of axioms for the propositional calculus given by Whitehead and Russell is complete, consistent and decidable (in fact Post spoke about the finiteness problem instead of decidability). He also proved that this system is complete in the sense called today after him, i.e., that if one adds to this system an unprovable formula as a new axiom then the extended system will be inconsistent. It is worth mentioning here that the completeness, the consistency and the independence of the axioms of the propositional calculus of *Prinicipia* was proved by Paul Bernays in his *Habilitationsschrift* in 1918 but this result has not been published until 1926 (cf. Bernays 1926) and Post did not know it.

In Post's dissertation one finds also an idea of many-valued logics obtained by generalizing the 2-valued truth table to $m$-valued truth tables ($m \geqslant 2$). Post proposed also a general method of studying systems of logic (treated as systems for inferences) by finitary symbol manipulations (later he called such systems canonical systems of the type $A$) – hence he considered formal logical systems as combinatorial systems.

In his doctoral dissertation Post mentioned also his studies of systems of 2-valued truth functions closed under superposition. In particular he showed that every truth function is definable in terms of negation and disjunction.

Those latter considerations were developed in the monograph *The Two-Valued Iterative Systems of Mathematical Logic* from 1941 (cf. Post 1941). One finds there among others the result called today Post's Functional Completeness Theorem. It gives a sufficient and necessary condition for a set of 2-valued truth functions to be complete, i.e., to have he property that any 2-valued truth function is definable in terms of it. Post distinguished five properties of truth functions. His theorem says that a given set $X$ of truth functions is complete if and only if for every of those five properties there exists in $X$ a truth function which does not have it. Did Post prove this theorem? In Post (1941) one finds no proof satisfying the standards accepted today. The reason for that was Post's baroque notation (it was in fact an unprecise adaptation of the imprecise notation of Jevons from his *Pure Logic*, cf. Jevons 1864), other reason was the fact that Post seemed to be simultaneously pursuing several different topics. The proof of Post's theorem satisfying today's standards can be found in the paper by Pelletier and Martin (1990).

## 2. Canonical Systems

During a year stay at Princeton University 1920–21 (he was awarded the postdoctoral Procter Fellowship) Post studied mainly canonical systems. Those systems founded the theory of formal languages. Post's results in this field were the anticipation of famous results by Gödel and Church on the incompleteness and undecidability of first-order logic.

The investigations mentioned above were connected with the following ideas: the whole *Principia Mathematica* can be considered as a canonical system of type $A$, i.e., as a system of signs in an alphabet which can be manipulated according to a given set of rules. Post wanted to solve the decision problem (in his terminology: the finiteness problem) for canonical systems of type $A$. In his way he wanted to find a method which would decide for any given formula of the system of *Principia* whether it is formally provable in it or not. Since *Principia* are a formalization of the whole of mathematics, this method would give a decision procedure for the whole of mathematics.

Besides canonical systems of type $A$ Post introduced systems of type $B$ and $C$. Just the latter are known today as Post's production systems. Let us define them using the contemporary terminology and notation.

Let $\Sigma$ be a finite alphabet. Its elements are called terminals. One uses also non-terminal symbols $P_{\cdot}$. A canonical production has the form:

$$\begin{array}{cccccccc}
g_{11} & P_{i'_1} & g_{12} & P_{i'_2} & \ldots & g_{1m_1} & P_{i'_{m_1}} & g_{1(m_1+1)} \\
g_{21} & P_{i''_1} & g_{22} & P_{i''_2} & & \ldots g_{2m_2} & P_{i''_{m_2}} & g_{2(m_2+1)} \\
\ldots & \ldots & \ldots & \ldots & \ldots & \ldots & \ldots & \ldots \\
g_{k1} & P_{i^{(k)}_1} & g_{k2} & P_{i^{(k)}_2} & \ldots & g_{km_k} & P_{i^{(k)}_{m_k}} & g_{k(m_k+1)} \\
 & & & & \Downarrow & & & \\
g_1 & P_{i_1} & g_2 & P_{i_2} & \ldots & g_m & P_{i_m} & g_{m+1}
\end{array}$$

where $g_k$ are strings on the alphabet $\Sigma$, $P_k$ are variable strings (non-terminals) and each of the $P$'s in the line following the $\Downarrow$ also occurs as one of the $P$'s above $\Downarrow$. A system in canonical form of type $C$ consists of a finite set of strings (Post called them initial assertions) together with a finite set of canonical productions. It generates of course a subset of $\Sigma^*$ (= the set of all finite strings over $\Sigma$) which can be obtained from the initial assertions by the iterations of canonical productions. Today it is known that such sets are exactly recursively enumerable languages.

Post proved the equivalence of canonical systems of type $A$, $B$ and $C$. He also showed that this part of *Principia* which corresponds to the first-order predicate calculus can be formalized as a canonical system of type $B$ and consequently also of type $C$. Moreover the set of all provable formulas of the system of *Principia* can be regarded as a set of strings of symbols which can be generated by certain canonical system of type $C$.

Post proved also the important Normal Form Theorem for canonical systems of type $C$. Let us say that a canonical system of type $C$ is normal if and only if it has exactly one initial assertion and each of its productions has the form

$$gP \Longrightarrow P\overline{g}.$$

A set of strings $U \subseteq \Sigma^*$ is said to be normal if and only if there exists a normal system on an alphabet $\Delta$ containing $\Sigma$ and generating a set $\mathcal{N}$ such that $U = \mathcal{N} \cap \Sigma^*$. Post's Normal Form Theorem says now that if $U \subseteq \Sigma^*$ is a set of strings generated by some canonical system of type $C$ then $U$ is normal – a proof can be found in (1943) and (1965).

One of the consequences of Normal Form Theorem is that a decision procedure for canonical systems of type $C$ would induce a decision procedure for the whole system of *Principia*.

Post started his researches in this direction by considering normal systems of a special form, so-called tag systems. Tag systems are normal systems in which all $g$'s in production rules are of the same length (but not necessarily the $\overline{g}$'s) and $\overline{g}$'s depend only on the first symbol of the appropriate string $g$. In particular Post started by studying the following simple system:

$$agP \Longrightarrow Paa,$$
$$bgP \Longrightarrow Pbbab,$$

where $g \in \{a, b\}^*$ and $|g| = 2$. This case turned out to be difficult (the question whether such systems are decidable is open till today).

Post supposed that tag systems are recursively undecidable. This was proved by M. Minsky in (1961).

Note that the procedure of generating expressions by a canonical system of type $C$ is similar to the process of generating computable functions from certain given initial functions by iterations of certain given operations on functions.[3] Hence those systems can be regarded as a formalism making precise the intuitive notion of effective computability. Consequently one can formulate a thesis analogous to Church's thesis and called Post's thesis which says that any finitely given language is generated by rules of some canonical normal system.

An application of Cantor's diagonal method led Post to the conclusion that the decision problem for normal systems has a negative solution. In 1921 Post sketched a formal proof of this. He wrote in (1965, pp. 421–422):

> We [...] conclude that the finiteness problem for the class of all normal systems is unsolvable, that is, there is no finite method which would uniformly enable us to tell of an arbitrary normal system and arbitrary sequence on the letters thereof whether that sequence is or is not generated by the operations of the system from the primitive sequence of the system.

Those considerations led him also to the conclusion about the incompleteness, i.e., to the conclusion that (cf. Post 1943):

> *not only was every (finitary) symbolic logic incomplete relative to a certain fixed class of propositions [...] but that every such logic was extendable relative to that class of propositions. (p. 215)*

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[3]It was shown later that the definition of effective computability by Post's canonical systems is equivalent to the definitions in the language of $\lambda$-definability, of recursive functions or by Turing machines.

In (1943) he also wrote:

> *No normal-deductive-system is equivalent to the complete logical system (if such there be); better, given any normal-deductive-system there exists another which second proves more theorems (to put it roughly) than the first. (p. 200)*

And he added

> *A complete symbolic logic is impossible. (p. 215)*

Those results anticipated results by Gödel and Church on the incompleteness and undecidability of systems of first-order logic (cf. Gödel 1931a; Church 1936a and 1936b). Post knew of course that his results are, as he wrote, "fragmentary". He never published them (though in 1941, hence already after the publication of Gödel's and Church's results) he tried to publish results of his investigations from 1920–1921).

What was his reaction to the appearance of the Gödel's paper (1931a)? On the one hand he was disappointed that not his name will be connected with results he anticipated and on the other he admired the genius and contribution of Gödel. In a postcard to Gödel sent on 19th October 1938 Post wrote (cf. Davis 1994, p. xvii):

> I am afraid that I took advantage of you on this, I hope but our first meeting. But for fifteen years I had carried around the thought of astounding the mathematical world with my unorthodox ideas, and meeting the man chiefly responsible for the vanishing of that dream rather carried me away.
>
> Since you seemed interested in my way of arriving at these new developments perhaps Church can show you a long letter I wrote to him about them. As for any claims I might make perhaps the best I can say is that I would have *proved* Gödel's Theorem in 1921 – had I been Gödel.

And in the letter to Gödel from 30th October 1938 he wrote (cf. Davis 1994, p. xvii):

> [. . . ] after all it is not ideas but the execution of ideas that constitute a mark of greatness.

Considering decidability problem one should mention Post's contribution to making precise the notion of effective computability. Post was of the opinion that Herbrand-Gödel's and Church-Kleene's definitions were both lacking in that neither constituted a "fundamental" analysis of the notion of algorithmic process. In (1936) he proposed a definition based on the operations of marking an empty box and erasing the mark in a marked box. Note the similarity with the definition given at the same time by A. Turing (1936–1937). The difference between both approaches consists is that Turing formulated his definition in terms of an idealized computer while Post in terms of a program (a list of instructions written in a given language).

At the beginning of the forties Post wrote a paper in which he tried to describe his studies on the incompleteness and undecidability from 1920–1921 anticipating Gödel's and Church's results. It is the paper "Absolutely Unsolvable Problems and Relatively Undecidable Propositions – Account of an Anticipation". It was submitted in 1941 to *American Journal of Mathematics*. In a letter to H. Weyl accompanying the manuscript Post explained why he did not publish his results twenty years earlier and wants to do it now, i.e., after the publications by Gödel and Church. Among reasons he mentions problems he had with publishing his earlier papers (in particular (1921 and (1941)) which did not find a recognition and appreciation by mathematicians as well as the problems with the health which delayed the preparation of full detailed proofs. Though the editors appreciated the significance of Post's investigations and results, the paper has been rejected. Communicating this decision H. Weyl wrote in a letter to Post from 2nd March 1942 (cf. Davis 1994, p. xix):

> [...] I have little doubt that twenty years ago your work, partly because of its then revolutionary character, did not find its due recognition. However, we cannot turn the clock back; in the meantime Gödel, Church and others have done what they have done, and the American Journal is no place for historical accounts; [...]. (Personally, you may be comforted by the certainty that most of the leading logicians, at least in this country, know in a general way of your anticipation.)

Only a small part of Post's paper has been published, i.e., the part containing his Normal Form Theorem (cf. Post 1943). The full version of the paper "Absolutely Unsolvable Problems and Relatively Undecidable Propositions – Account of an Anticipation" was published posthumously in Davis' book *The Undecidable* (1965).

## 3. Recursion Theory

Main Post's results which found the recognition and appreciation belong to the recursion theory. They were of course connected with his investigations described above.

Considering Post's results in the recursion theory one must tell first of all about his paper "Recursively Enumerable Sets of Positive Integers and Their Decision Problems" (1944). It turned out to be the most influential of his publications and fundamental for the whole recursion theory. Recursion theory was for the first time presented in it as an autonomous branch of mathematics.

One finds there:

- a theorem called today Post's Theorem and stating that a set $X$ is recursive if and only if the set $X$ and its complement $-X$ are recursively enumerable,[4]

- a theorem stating that (a) any infinite recursively enumerable set has an infinite recursive subset and (b) there exists a recursively enumerable set which is not recursive.

Main subject of the considered paper is the mutual reducibility of recursively enumerable sets. Recall that a set $X$ is said to be many-one reducible to a set $Y$ if and only if there exists a recursive function $f$ such that

$$x \in X \equiv f(x) \in Y.$$

If the function $f$ is one-one then we say about one-one reducibility.

Post proved the existence of a recursively enumerable set $K$ which is complete with respect to many-one (one-one) reducibility, i.e., such that any recursively enumerable set $X$ is many-one (one-one) reducible to $K$. Hence $K$ has a maximal degree of unsolvability with respect to many-one (one-one) reducibility. Post constructed also a recursively enumerable set which is simple, i.e., has the property that there exists no infinite recursively enumerable subset of its complement. Such a set cannot be of a maximal degree with respect to one-one reducibility. This led Post to the formulation of the following problem called today Post's problem: does there exist a recursively enumerable but not recursive set of a degree lower than the degree of the complete set $K$ with respect to a given type of reducibility?

Post did not succeed in solving this problem. Though he proved in (1948) the existence of sets of a degree lower than the degree of the set $K$ but those sets were not recursively enumerable.

Post's problem was solved independently by A.A. Muchnik (1956) and R. Friedberg (1957) by a new method introduced by them – and called the priority method. This method proved later to be really fruitful – many results in the recursion theory, in particular in the theory of degrees, have been obtained by it. They showed that there exist two recursively enumerable sets $A$ and $B$ such that $A$ is not recursive in $B$ and $B$ is not recursive in $A$.

As a consequence one obtains that degrees of unsolvability (even the degrees of recursively enumerable sets) are not linearly ordered and that there are recursively enumerable degrees other than the degree of recursive sets and the degree of the complete set (sets $A$ and $B$ constructed by Friedberg and Muchnik are examples of such sets).

Discussing here Post's paper "Recursively Enumerable Sets of Positive Integers and Their Decision Problems" one should mention the way in which Post presented his results in it. This way became a norm and a standard in the recursion theory. It consisted in giving rather informal proofs with a description of

[4] Recall that a set $X$ is recursively enumerable if and only if there exists a recursive function $f$ such that $X$ is the image of $f$ if and only if there exists a recursive relation $R$ such that $x \in X \equiv \exists y R(x, y)$.

intuitions. Post saw the need of providing formal proofs but on the other hand he wrote:

> [...] the real mathematics involved must lie in the informal development. For in every instance the informal "proof" was first obtained; and once gotten, transforming it into the formal proof turned out to be a routine chore. (1944, p. 284)

The considered paper had important long-term effects. It was the beginning of extensive studies of recursively enumerable sets, in particular of various types of reducibility. Here one can also see the source of such important notion as polynomial time reducibility or of studies connected with NP–completeness.

Post continued his studies of degrees of unsolvability in the paper "Degrees of Recursive Unsolvability (Preliminary Report)" (1948) where he generalized the notion of a degree to the case of any sets (not necessarily recursively enumerable) and proved the existence of a pair of incomparable degrees (both were lower than the degree of the complete recursively enumerable set $K$). He announced also a theorem (called today Post's theorem) stating that a set $X$ is recursive in a set $A \in \Sigma^0_n(\Pi^0_n)$ if and only if $X$ is a $\Delta^0_{n+1}$ set.

One should also mention here the joint paper by Post with S.C. Kleene "The upper semi-lattice of degrees of recursive unsolvability" (cf. Kleene-Post 1954). One finds there among others a theorem stating that the ordered set of degrees of unsolvability contains a densely ordered subset.

## 4. Undecidability of Algebraic Combinatorial Systems

Discussing Post's contribution to the recursion theory one should say about his results on undecidability of algebraic combinatorial systems. They provided "mathematical" examples of undecidable problems.

Let us start with the correspondence problem. It can be formulated as follows. A correspondence system is a finite set of ordered pairs $(g_1, h_1)$, $(g_2, h_2)$, $\ldots$, $(g_n, h_n)$ such that $g_i, h_i \in \Sigma^*$, $\Sigma$ a given finite alphabet. Such a system is said to be solvable if there exists a sequence $i_1, i_2, \ldots, i_k$ such that $1 \leqslant i_1, i_2, \ldots, i_k \leqslant n$ and

$$g_{i_1} g_{i_2} \ldots g_{i_k} = h_{i_1} h_{i_2} \ldots h_{i_k}.$$

The correspondence problem is to provide an algorithm for determining of a given correspondence system whether it is solvable or not. Post proved in the paper "A Variant of a Recursively Unsolvable Problem" (1946) that such an algorithm does not exist, hence the correspondence problem is not recursively solvable. This result plays an important role in the theory of formal languages.

Church suggested to Post to study also the decidability problem for Thue's systems known also as the word problem for monoids or semi-groups. It was formulated by the Norwegian mathematician Axel Thue in 1914 and can be formulated as follows: Let $\Sigma$ be a finite alphabet. Define an equivalence relation $\approx$ in $\Sigma^*$ by giving a finite set of pairs of words for which this equivalence holds, i.e., by putting

$$u_1 \approx v_1, u_2 \approx v_2, \ldots, u_n \approx v_n,$$

and closing this under the substitution of $v_i$ for $u_i$ ($i = 1, \ldots, n$). The problem consists now in providing an algorithm for determining of an arbitrary pair $(u, v) \in \Sigma^* \times \Sigma^*$ of strings whether or not $u \approx v$. In the paper "Recursive Unsolvability of a Problem of Thue" (1947) Post gave an example of a set of initial pairs defining an equivalence relation $\approx$ for which the word problem is unsolvable. In the proof Post used Turing machines (in fact he showed that the theory of Turing machines can be interpreted in terms of the word problem in such a way that an algorithm for the latter could be transformed into an algorithm for a problem concerning Turing machines known to be unsolvable). Post also gave a very careful technical critique of Turing's paper (1936–1937).

It is worth adding that the recursive unsolvability of the word problem was established independently by A.A. Markov in (1947) who based his proof on Post's normal systems.

## 5. Post's Philosophical and Methodological Ideas

Discussing the works and results of Post in the field of mathematical logic and recursion theory one should consider their philosophical and methodological background.

Like Gödel Post emphasized the significance of the absoluteness and the fundamental character of the notion of recursive solvability. He attempted also to explain the notion of provability – more exactly, he wanted to find a precise notion which would explain the intuitive notion of provability in arithmetic in such a way as the notion of recursiveness explains the notion of effective computability and solvability. He hoped that this will enable us to find absolutely undecidable arithmetical propositions. Later (before the death) he added to this also the problem of providing an absolute explication and explanation of the general mathematical notion of definability (he was convinced that this should be done even before giving the absolute explication of the notion of provability).

Post emphasized (cf. 1965, p. 64):

> I study Mathematics as a product of the human mind and not as absolute.

He was convinced that mathematical thinking is in fact creative. He wrote in (1965, p. 4):

> [...] mathematical thinking is, and must be, essentially creative.

He thought also that the human capacity to know cannot be closed and reduced to a formal system. And added (cf. 1965, p. 4):

> [...] creativeness of human mathematics has a counterpart inescapable limitations thereof – witness the absolutely unsolvable (combinatory) problems.

On the other hand he was convinced that the results on the undecidability and incompleteness indicate that human capacity to know with respect to mathematics are in fact bounded in spite of the creativeness of the mathematical thinking. He wrote in (1965, p. 56):

> The unsolvability of the finiteness problem for all normal systems, and the essential incompleteness of all symbolic logics, are evidences of limitations in man's mathematical powers, creative though these be.

Post claimed that there exist absolutely undecidable (i.e., unsolvable by no methods and means) propositions and that there is no complete system of logic.[5] A consequence of this was in his opinion the fact that (1965, p. 55):

> logic must not only in some parts of its description (as in the operations), but in its very operation be informal. Better still, we may write
>
> *The Logical Process is Essentially Creative.*

Consequently the human mind can never be replaced by a machine. He wrote in (1965, p. 55):

> We see that a *machine* would never give a complete logic; for once the machine is made *we* could prove a theorem it does not prove.

## 6. The Significance of Post's Results for the Development of Mathematical Logic and Foundations of Mathematics

The above considerations lead us to the conclusion that Post's works and results contributed very much to the development of mathematical logic and the foundations of mathematics. His works (together with works of J. Łukasiewicz) initiated the investigations on many-valued logics and on the Post algebras connected with them. His studies of the propositional calculus (the results of which were included in his doctoral dissertation) were the first metamathematical studies of a system of logic. Most significant were probably his works and results in the recursion theory. They contributed very much to establishing this field as an autonomous branch of the foundations of mathematics. They began intensive studies on degrees of unsolvability, in particular of recursively enumerable degrees, investigations on the (un)decidability of various systems, in particular combinatorial systems in algebra and on the various types of recursive reducibil-

[5] In (1965, p. 54, original emphasis) he wrote: "*A complete symbolic logic is impossible*".

ity. They influenced also the researches in the computer science (though Post showed no interest in computers). They were also very important for the theory of formal languages.

Post's investigations and results were in a sense ahead of his time, were precursory (compare his anticipation of Gödel's and Church's results described above). This had of course negative consequences as the fear of being not understood properly and the delay of publication of the results. Problems with health, in particular the illness under which he suffered almost the whole life, also hindered him from publishing the results at proper time. Many of Post's results were left in an incomplete form. He tried the whole time to improve his results and to find the most general form which also caused some delay. Nevertheless his contribution to the mathematical logic and to the foundations of mathematics was really significant.

# 14

# JOHN VON NEUMANN AND HILBERT'S SCHOOL[1]

The aim of the paper is to describe main achievements of John von Neumann in the foundations of mathematics and to indicate his connections with Hilbert's School. In particular we shall discuss von Neumann's contributions to the axiomatic set theory, his proof of the consistency of a fragment of the arithmetic of natural numbers and his discovery (independent of Gödel) of the second incompleteness theorem.

## 1. Introduction

Contacts of John (then still Janos, later Johann) von Neumann with David Hilbert and his school began in the twenties of the 20th century. Being formally a student of the University of Budapest (in fact he appeared there only to pass exams) he was spending his time in Germany and in Switzerland studying there physics and chemistry as well as visiting Hilbert in Göttingen (to discuss with him mathematics). After graduating in chemistry in ETH in Zurich (1925) and receiving the doctorate in Budapest (1926) (his doctoral dissertation was devoted to the axiomatization of set theory – cf. below), he became *Privatdozent* at the University in Berlin (1927–1929), and next in Hamburg (1929–1930). In 1930 he left Germany and went to the USA.[2]

[1] Originally published under the title "John von Neumann and Hilbert's School of Foundations of Mathematics", *Studies in Logic, Grammar and Rhetoric* 7 (20), 2004, 37–55. Reprinted with kind permission of Wydawnictwo Uniwersytetu w Białymstoku (Publishing House of the University in Białystok).

[2] We are not describing further the life of von Neumann and stop at about 1930 because his disappointment with the investigations in the foundations of mathematics led to the fact that after 1930 he lost interest in the foundational problems and turned his attention to other parts of mathematics, in particular to its applications (see Section 4). Note only that in 1930–1931 von Neumann was a visiting lecturer at Princeton University in New Jersey, later a professor there. Since 1933 he was a professor at the Institute for Advanced Study in Princeton. He died in 1957 at the age of 54.

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Talking about Hilbert's School we mean the group of mathematicians around Hilbert working in the foundations of mathematics and in the metamathematics (proof theory) in the frameworks of Hilbert's program of justification of the classical mathematics (by finitistic methods).[3]

Main works of von Neumann from the period from 1922 (the data of his first publication) till 1931 concern mainly metamathematics as well as the quantum mechanics and the theory of operators (also Hilbert worked at that time in just those domains). In this paper we shall be interested in the former, i.e., works devoted to and connected with Hilbert's metamathematical program.

Recall (to make clearer further considerations) that one of the steps in the realization of Hilbert's program was the formalization (and in particular the axiomatization) of the classical mathematics (this was necessary for further investigations of mathematical theories by finitistic methods of the proof theory).

The main achievements of von Neumann connected with the ideology of Hilbert's School are the following:

- the axiomatization of set theory and (connected with that) elegant theory of the ordinal and cardinal numbers as well as the first strict formulation of principles of definitions by the transfinite induction,
- the proof (by finitistic methods) of the consistency of a fragment of the arithmetic of natural numbers,
- the discovery and the proof of the second incompleteness theorem (this was done independently of Gödel).

The rest of the paper will be devoted just to those items.

## 2. Foundations of Set Theory

Contribution of von Neumann devoted to the set theory consisted not only of having proposed a new elegant axiomatic system (extending the system of Zermelo-Fraekel ZFC) but also of having proposed several innovations enriching the system ZFC, in particular the definition of ordinal and cardinal numbers and the theory of definitions by transfinite induction.

The definition of ordinals and cardinals was given by von Neumann in the paper "Zur Einführung der transfiniten Zahlen" (1923) – it was his second publication. He has given there a definition of an ordinal number which could "give unequivocal and concrete form to Cantor's notion of ordinal number" (1923, p. 199) in the context of axiomatized set theories (cf. von Neumann 1923). Von Neumann's ordinal numbers are – using the terminology of G. Cantor – repre-

[3]There is a rich literature on Hilbert's program – see, e.g., Murawski (1999a) and the literature indicated there.

sentatives of order types of well ordered sets. In (1923) von Neumann wrote:

> What we really wish to do is to take as the basis of our considerations the proposition: 'Every ordinal is the type of the set of all ordinals that precede it.' But, in order to avoid the vague notion 'type', we express it in the form: 'Every ordinal is the set of the ordinals that precede it.' This is not a proposition proved about ordinals; rather, it would be a definition of them if transfinite induction had already been established.[4]

In this way one obtains the sequence $\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}, \{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}$, etc. – i.e., von Neumann's ordinal numbers.

Those sets – as representatives – are in fact very useful, especially in the axiomatic set theory because they can be easily defined in terms of the relation $\in$ only and they are well order by the relation $\in$. They enable also an elegant definition of cardinal numbers. In the paper (1928) one finds the following definition: a well ordered set $M$ is said to be an ordinal (number) if and only if for all $x \in M$, $x$ is equal to the initial segment of $M$ determined by $x$ itself (as von Neumann wrote: $x = A(x; M)$). Elements of ordinal numbers are also ordinal numbers. An ordinal number is said to be a cardinal number if and only if it is not equipollent to any of its own elements.

In the paper (1923) von Neumann presupposed the notions of a well ordered set and of the similarity and then proved that for any well ordered set there exists a unique ordinal number corresponding to it. All that was done in a naïve set theory but a remark was added that it can be done also in an axiomatic set theory. And in fact von Neumann did it in papers (1928a) and (1928b). To be able to do this in a formal way one needs the Axiom of Replacement (in the paper (1923) von Neumann called it Fraenkel's axiom). Since that time von Neumann was an staunch advocate of this axiom.

The problem of definitions by transfinite induction was considered by von Neumann in the paper "Über die Definition durch transfinite Induction, und verwandte Fragen der allgemeinen Mengenlehre" (1928). He showed there that one can always use definitions by induction on ordinal numbers and that such definitions are unequivocal. He proved that for any given condition $\varphi(x, y)$ there exists a unique function $f$ whose domain consists of ordinals such that for any ordinal $\alpha$ one has $f(\alpha) = \varphi(F(f, \alpha), \alpha)$ where $F(f, \alpha)$ is a graph of the function $f$ for arguments being elements of $\alpha$.

Why is the discussed paper so important? For many years, in fact since the axiomatization of set theory by Zermelo, there were no formal counterparts of ordinal and cardinal numbers and this was the reason of avoiding them in the axiomatic set theory. It became a custom to look for ways of avoiding trans-

[4] "Wir wollen eigentlich den Satz: 'Jede Ordnungszahl ist der Typus der Menge aller ihr vorangehenden Ordnungszahlen' zur Grundlage unserer Überlegungen machen. Damit aber der vage Begriff 'Typus' vermieden werde, in dieser Form: 'Jede Ordnungszahl ist die Menge der ihr vorangehenden Ordnungszahlen.' Dies ist kein bewiesener Satz über Ordnungszahlen, es wäre vielmehr, wenn die tranfinite Induktion schon begründet wäre, eine Definition derselben."

finite numbers and transfinite induction in mathematical reasonings (cf., e.g., Kuratowski 1921 and 1922). Von Neumann's paper introduced a new paradigm which works till today. The leading idea of the paper was the will to give set theory as wide field as possible.

The most important and known contribution of John von Neumann is undoubtedly a new approach and new axiomatization of set theory. The main ideas connected with that appeared by von Neumann already in 1923 (he was then 20 (sic!)). He described them in a letter to Ernst Zermelo from August 1923.[5] He wrote there that the impulse to his ideas came from a work by Zermelo "Untersuchungen über die Grundlagen der Mengenlehre. I" (1908) and added that in some points he went away from Zermelo's ideas, in particular

- the notion of 'definite property' had been avoided – instead the "acceptable schemas" for the construction of functions and sets had been presented,
- the axiom of replacement had been assumed – it was necessary for the theory of ordinal numbers (later von Neumann emphasized, like Fraenkel and Skolem, that it is needed in order to establish the whole series of cardinalities – cf. von Neumann 1928b),
- sets that are "too big" (for example the set of all sets) had been admitted but they were taken to be inadmissible as elements of sets (that sufficed to avoid the paradoxes).

About 1922–1923 while preparing a paper in which those ideas should be developed he contacted Abraham Fraenkel. The latter recalled this (already after the death of von Neumann) in a letter to Stanisław Ulam in such a way (see letter from Fraenkel to Ulam in Ulam 1958, p. 32):

> Around 1922–23, being then professor at Marburg University, I received from Professor Erhard Schmidt, Berlin (on behalf of the *Redaktion* of the *Mathematische Zeitschrift*) a long manuscript of an author unknown to me, Johann von Neumann, with the title "Die Axiomatisierung der Mengenlehre", this being his eventual doctor[al] dissertation which appeared in the *Zeitschrift* only in 1928 (vol. 27). I was asked to express my views since it seemed incomprehensible. I don't maintain that I understood everything, but enough to see that this was an outstanding work and to recognize *ex ungue leonem*. While answering in this sense, I invited the young scholar to visit me (in Marburg) and discussed things with him, strongly advising him to prepare the ground for the understanding of so technical an essay by a more informal essay which should stress the new access to the problem and its fundament consequences. He wrote such an essay under the title "Eine Axiomatisierung der Mengenlehre" and I published it in 1925 in the *Journal für Mathematik* (vol. 154) of which I was then Associate Editor.

Before continuing the story let us explain that *ex ungue leonem* – spotting a lion from the claw – is an expression used by Daniel Bernoulli while talking

[5] This letter was partly reproduced in Meschkowski (1967, pp. 289–291).

about Newton two and a half centuries ago. Bernoulli was namely sent a mathematical paper without a name of the author but he immediately recognized that it has been written just by Newton.

Von Neuman wrote at the beginning of the paper "Eine Axiomatisierung der Mengenlehre" (1925):

> The aim of the present work is to give a logically unobjectionable axiomatic treatment of set theory. I would like to say something first about difficulties which make such an axiomatization of set theory desirable.[6]

He stressed explicitly three points mentioned in the letter to Zermelo.

The characteristic feature of the system of set theory proposed by von Neumann is the distinction between classes, "domains" (*Bereiche*) and sets (*Mengen*). Classes are introduced by the Principle of Comprehension – von Neumann seems to have regarded this principle as the quintessence of what he called "naïve set theory" (cf. von Neumann 1923; 1928a; 1929). His appproach to set theory was strongly based on the idea of limitation of size according to which: a class is a set if and only if it is not "too big". The latter notion was described by the following axiom:

(∗) *A class is "too big"* (in the terminology of Gödel (1940) – *is a proper class*) *if and only if it is equivalent to the class of all things.*

Hence a class of the cardinality smaller than the cardinality of the class of all sets is a set. Von Neumann states further that the above principle implies both the Axiom of Separation and the Axiom of Replacement. It implies also the well ordering theorem (he indicated it already in the letter to Zermelo). Indeed, according to the reasoning used in the Burali-Forti paradox, the class *On* of all ordinal numbers is not a set, hence by the above principle it is equipollent with the class $V$ of all sets. In this way one obtains a strengthened version of the well ordering theorem, namely:

> *The class V of all sets can be well ordered.*

In the paper "Die Axiomatisierung der Mengenlehre" (1928a) (this was in fact a "mathematical" version of the system of set theory announced in the paper (1925)) von Neumann observed that the principle gives also a global choice function $F$ such that for any nonempty set $A$ it holds: $F(A) \in A$.

The Axiom of Choice, being a consequence of (∗), enabled von Neumann to introduce ordinal and cardinal numbers without the necessity of introducing any

[6] "Das Ziel der vorliegenden Arbeit ist, eine logisch einwandfreie axiomatische Darstellung der Mengenlehre zu geben. Ich möchte dabei einleitend einiges über die Schwierigkeiten sagen, die einen derartigen Aufbau der Mengenlehre erwünscht gemacht haben." (p. 34)

new primitive notions. It was in fact a realization of the idea he wrote about in 1923. He used here the Fraenkel's Axiom of Replacement.

Observe that the distinction between classes and sets appeared already by Georg Cantor – he wanted to eliminate in this way the paradox of the set of all sets. Cantor used to call classes "absolutely infinite multiplicities". But he gave no precise criterion of distinguishing classes and sets – it was given only by von Neumann. The latter has also shown that Cantor was mistaken when he claimed that the absolutely infinite multiplicities (e.g., the multiplicity of all ordinal numbers) cannot be treated as consistent objects.

In the original formulation of set theory by von Neumann there are no notions of a set and a class. Instead one has there the primitive notion of a function (and of the relation $\in$). Von Neumann claimed that it is in fact only a technical matter – indeed, the notions of a set and of a function are mutually definable, i.e., a set can be treated as a function with values 0 and 1 (the characteristic function of the set) and, vice versa, a function can be defined as a set of ordered pairs.

Add that in the von Neumann's system of set theory there are no urelements – there are only pure sets and classes. On the other hand among axioms there is the Axiom of Foundation introduced by Dimitri Mirimanoff in (1917).[7] This axiom ensures that there are no infinite decreasing $\in$-sequences, i.e., such sequences that $\ldots \in x_n \in \ldots \in x_1 \in x_0$ and that there are neither sets $x$ such that $x \in x$ nor sets $x$ and $y$ such that $x \in y$ and $y \in x$. This axiom implies that the system of set theory containing it becomes similar to the theory of types: one can say that the system ZF with the Axiom of Foundation can be treated as an extension of the (cumulative) theory of types to the transfinite types described in a simpler language than it was the case by Russell.

It should be noticed that von Neumann was one of the first who investigated metatheoretical properties of the axiomatic set theory. In particular he studied his own system from the point of view of the categoricity (1925) and of the relative consistency (1929). Probably he was also the first author who called attention to the Skolem paradox. According to von Neumann this paradox stamps axiomatic set theory "with the mark of unreality" and gives reasons to "entertain reservations" about it (cf. 1925, p. 53).

Von Neumann wrote about the proof of the relative consistency of his system of set theory in the paper "Über eine Widerspruchsfreiheitsfrage der axiomatischen Mengenlehre" (1929). He saw main difficulties in the axiom $(*)$. Therefore he considered two axiomatic systems: system S which was his original system (hence with the axiom $(*)$) and the system $S^*$ which was von Neumann's system but with the Axiom of Replacement and the Axiom of Choice instead of the axiom $(*)$. In the paper (1929) he proved that:

[7] In fact it was for the first time discussed by Mirimanoff and Skolem it was just von Neumann who as the first formulated it explicitly.

1. $S^*$ will remain consistent if one adds the Axiom of Foundation and does not admit urlements,
2. $S^*$ is a subsystem of such a system.

Hence von Neumenn proved the relative consistency of the Axiom of Foundation with respect to the system $S^*$. It was in fact the first significant metatheoretical result on set theory.

It is worth saying that in (1929) von Neumann developed the cumulative hierarchy in technical details. Using the Axiom of Foundation and the ordinal numbers he showed that the universe of sets can be divided into "levels" indexed by ordinal numbers. He introduced the notion of a rank of a set: a rank of a set $x$ is the smallest ordinal number $\alpha$ such that the set $x$ appears at the level $\alpha$. This hierarchy is cumulative, i.e., lower levels are included in higher ones. The hierarchy can be precisely defined as follows:

$$
\begin{aligned}
V_0 &= \emptyset, \\
V_{\alpha+1} &= V_\alpha \cup \mathcal{P}(V_\alpha), \\
V_\lambda &= \bigcup_{\alpha<\lambda} V_\alpha \quad \text{for } \lambda \in \lim, \\
V &= \bigcup_{\alpha\in \mathrm{On}} V_\alpha, \\
\mathrm{rank}(x) &= \mu\alpha(x \in V_\alpha).
\end{aligned}
$$

It is worth adding here that von Neumann treated the Axiom of Foundation rather as a tool in the metatheoretical investigations of set theory.

We are talking the whole time about axioms of set theory but no axioms have been given so far. It is time to do it!

Let us start by stating that the main idea underlying von Neumann's system of set theory has been accepted with enthusiasm – in fact it provided a remedium to too drastic restrictions put on objects of set theory by the system ZF of Zermelo-Fraenkel (one was convinced that such strong restrictions are not needed in order to eliminate paradoxes; on the other hand the restrictions put by ZF made the development of mathematics within ZF very difficult and unnatural). Nevertheless the system of von Neumann was not very popular among specialists – the reason was the fact that it was rather counter-intuitive and was based on a rather difficult notions (recall that the primitive notion of a function instead of the notion of a set was used there). Hence the need of reformulating the original system. That has been done by Paul Bernays: in (1937) he announced the foundations, and in a series of papers published in the period 1937–1958 (cf. 1937, 1941a, 1942, 1958) he gave an extensive axiomatic system of set theory which realized the ideas of von Neumann and simultaneously he succeeded to formulate his system in a language close to the language of the system ZF.

In (1937, p. 65) he wrote:

> The purpose of modifying the von Neumann system is to remain nearer to the structure of the original Zermelo system and to utilize at the same time some of the set-theoretic concepts of the Schröder logic and of *Principia Mathematica* which have become familiar to logicians. As will be seen, a considerable simplification results from this arrangement.

The universe of set theory consists by Bernays of two parts:

- sets denoted by $x, y, z, \ldots$,
- classes denoted by $A, B, C, \ldots$

Hence it is not an elementary system! There are two primitive notions: $\in$ (= to be an element of (to belong to) a set) and $\eta$ (= to be an element (to belong to) a class). Hence one has two types of atomic formulas: $x \in y$ and $x\eta A$. There are also two groups of axioms: axioms about sets (they are analogous to axioms of Zermelo) and axioms characterizing classes. The very important feature of Bernays' axioms is the fact that there are only finitely many axioms and there are no axiom schemes.

In a work devoted to the consistency of the Axiom of Choice and of the Generalized Continuum Hypothesis K. Gödel gave an axiomatic system of set theory which is in fact a modification of Bernays' system. Its main advantage is that it is an elementary system (i.e., it contains only one type of variables).[8]

Let us describe now in details the system NBG of Gödel. It is based on the idea that the variables vary over classes. Among classes we distinguish those classes that are elements of other classes. They are called sets and their totality is denoted by $V$. The remaining classes are called proper classes.

Define the class $V$ as follows

$$x \in V \longleftrightarrow (\exists y)(x \in y).$$

Hence $x$ is a set if and only if there exists a class $y$ such that $x \in y$. Define also a notion of a function in the following way:

$$\mathrm{Func}(r) \longleftrightarrow \forall x \forall y \forall z[(x, y) \in r \wedge (x, z) \in r \longrightarrow y = z].$$

The system NBG is based on the following nonlogical axioms:

- (Extensionality)
$$\forall x\ \forall y\ [\forall z(z \in x \longleftrightarrow z \in y) \longrightarrow x = y],$$
- (Axiom of Classes)
$$\exists x\ \forall y\ [\exists z(y \in z) \longrightarrow y \in x],$$
- (Axiom of the Empty Set)
$$\exists x\ [\forall y\ (y \notin x) \wedge \exists z\ (x \in z)],$$

[8]It is worth noting here that the idea of using in the system of von Neumann – Bernays only one type of variables and one membership relation is due to Alfred Tarski – cf. Mostowski (1939, p. 208; 1949, p. 144).

- (Pairing Axiom)
$$\forall x \in V\ \forall y \in V\ \exists z \in V\ [\forall u(u \in z \longleftrightarrow u = x \vee u = y)],$$
- (Axiom Scheme of Class Existence) if $\Phi$ is a formula with free variables $v_1, \ldots, v_n$, then the following formula
$$\forall v_1, \ldots, v_n \in V\ \ \exists z\ \ \forall x\ [x \in z \longleftrightarrow$$
$$\longleftrightarrow (x \in V \wedge \Phi^{(V)}(x, v_1, v_2, \ldots, v_n))]$$
is an axiom (note that one cannot quantifier in $\Phi$ over class variables!); $\Phi^{(V)}$ denotes the relativization of $\Phi$ to the class $V$,
- (Axiom of Union)
$$\forall x \in V\ \exists y \in V\ \forall u\ [u \in y \longleftrightarrow \exists v(u \in v \wedge v \in x)],$$
- (Power Set Axiom)
$$\forall x \in V\ \exists y \in V\ \forall u\ (u \in y \longleftrightarrow u \subseteq x),$$
- (Infinity Axiom)
$$\exists x \in V\ [\emptyset \in x \wedge \forall u \in x\ \forall v \in x\ (u \cup \{v\} \in x)],$$
- (Axiom of Replacement)
$$\forall x \in\ V\ \ \forall r\ [Func(r) \longrightarrow$$
$$\longrightarrow \exists y \in V\ \forall u\ (u \in y \longleftrightarrow \exists v \in x\ ((v, u) \in r)],$$
- (Axiom of Foundation)
$$\forall x[x \neq \emptyset \longrightarrow \exists y \in x\ (x \cap y = \emptyset)].$$

We one adds to this system the following Axiom of Global Choice (in a strong version):
$$\exists x[Func(x) \wedge \forall y \in V[y \neq \emptyset \longrightarrow \exists z(z \in y \wedge (y, z) \in x)]]$$
then one obtains the system denoted as NBGC.

It has turned out that the axiom scheme of class existence can be replaced by the following (finitely many!) axioms:
$$\exists a\ \forall x, y \in V[(x, y) \in a \longleftrightarrow x \in y]$$
(it says that $a$ jest a graph of the membership relation $\in$ for sets),
$$\forall a\ \forall b\ \exists c\ \forall x[x \in c \longleftrightarrow (x \in a \wedge x \in b)]$$
(it defines the intersection of classes),
$$\forall a\ \exists b\ \forall x \in V\ [x \in b \longleftrightarrow x \notin a]$$
(it defines the complement of a class),
$$\forall a\ \exists b\ \forall x \in V\ [x \in b \longleftrightarrow \exists y \in V((x, y) \in a)]$$
(it defines the left domain of a relation),
$$\forall a\ \exists b\ \forall x, y \in V\ [(x, y) \in b \longleftrightarrow x \in a),$$
$$\forall a\ \exists b\ \forall x, y, z \in V\ [(x, y, z) \in b \longleftrightarrow (y, z, x) \in a],$$

$$\forall a\, \exists b\, \forall x, y, z \in V\, [(x, y, z) \in b \longleftrightarrow (x, z, y) \in a].$$

The systems NBG and NBGC have very nice metamathematical properties, in particular:

- NBG (NBGC) is finitely axiomatizable (observe that the Zermelo-Fraenkel system ZF is not finitely axiomatizable!),
- NBG is a conservative extension of ZF with respect to formulas saying about sets, i.e., for any formula $\varphi$ of the language of set theory:
  $$\mathrm{ZF} \vdash \varphi \qquad \text{if and only if} \qquad \mathrm{NBG} \vdash \varphi^{(V)}$$
  where $\varphi^{(V)}$ denotes the relativization of $\varphi$ to the class $V$ of all sets (and similarly for NBGC and ZFC where the latter symbol denotes the theory ZF plus the Axiom of Choice AC),
- NBG is consistent if and only if ZF is consistent (and similarly for NBGC and ZFC).

## 3. Consistency Proof for Arithmetic

One of the main aims of Hilbert's program was the consistency proof (by save finitary methods) for the whole classical mathematics. Students of Hilbert took this task and soon first partial results appeared. The first work in this direction was the paper by Wilhelm Ackermann (1924–1925) where he gave a finitistic proof of the consistency of arithmetic of natural numbers without the axiom (scheme) of induction.[9]

Next attempt to solve the problem of the consistency was the paper "Zur Hilbertschen Beweistheorie" (1927) by von Neumann. He used another formalism than that in Ackermann (1924–1925) and, similarly as Ackermann, proved in fact the consistency of a fragment of arithmetic of natural numbers obtained by putting some restrictions on the induction. We cannot consider here the (complicated) technical details of von Neumann's proof. It is worth mentioning that in the introductory section of von Neumann's (1927) a nice and precise formulation of aims and methods of Hilbert's proof theory was given. It indicated how was at that time the state of affairs and how Hilbert's program was understood. Therefore we shall quote the appropriate passages.

Von Neumann writes that the essential tasks of proof theory are (cf. von Neumann 1927, pp. 256–257):

> I. First of all one wants to give a proof of the consistency of the classical mathematics. Under 'classical mathematics' one means the mathematics in the sense in which

[9] In fact it was a much weaker system than the usual system of arithmetic but the paper provided the first attempt to solve the problem of consistency. Later in the paper (1940) Ackermann proved the consistency of the full arithmetic of natural numbers by using methods from his paper (1924) and the transfinite induction.

it was understood before the begin of the criticism of set theory. All set-theoretic methods essentially belong to it but not the proper abstract set theory. [...]

II. To this end the whole language and proving machinery of the classical mathematics should be formalized in an absolutely strong way. The formalism cannot be too narrow.

III. Then one must prove the consistency of this system, i.e., one should show that certain formulas of the formalism just described can never be "proved".

IV. One should always strongly distinguish here between various types of "proving": between formal ("mathematical") proving in a given formal system and contents ("metamathematical") proving [of statements] about the system. Whereas the former one is an arbitrarily defined logical game (which should to a large extent be analogues to the classical mathematics), the latter is a chain of directly evident contents insights. Hence this "contents proving" must proceed according to the intuitionistic logic of Brouwer and Weyl. Proof theory should so to speak construct classical mathematics on the intuitionistic base and in this way lead the strict intuitionism ad absurdum.[10]

Note that von Neumann identifies here finitistic methods with intuitionistic ones. This was then current among members of the Hilbert's school. The distinction between those two notions was to be made explicit a few years later – cf. Hilbert and Bernays (1934, pp. 34 and 43) and Bernays (1934; 1935; 1941b), see also Murawski (2001).

As an interesting detail let us add that on the paper (1927) by von Neumann reacted critically Stanisław Leśniewski publishing the paper "Grundzüge eines neuen Systems der Grundlagen der Mathematik" (1929) in which he critically analyzed various attempts to formalize logic and mathematics. Leśniewski among others expresses there his doubts concerning the meaning and significance of von Neumann's proof of the consistency of (a fragment of) arithmetic and constructs – to maintain his thesis – a "counterexample", namely he de-

[10] "I. In erster Linie wird der Nachweis der Widerspruchsfreiheit der klassischen Mathematik angestrebt. Unter 'klassischer Mathematik' wird dabei die Mathematik in demjenigen Sinne verstanden, wie sie bis zum Auftreten der Kritiker der Mengenlehre anerkannt war. Alle mengentheoretischen Methoden gehören im wesentlichen zu ihr, nicht aber die eigentliche abstrakte Mengenlehre. [...]

"II. Zu diesem Zwecke muß der ganze Aussagen- und Beweisapparat der klassischen Mathematik absolut streng formalisiert werden. Der Formalismus darf keinesfalls zu eng sein.

"III. Sodann muß die Widerspruchsfreiheit dieses Systems nachgewiesen werden, d.h. es muß gezeigt werden, daß gewisse Aussagen 'Formeln' innerhalb des beschriebenen Formalismus niemals "bewiesen" werden können.

"IV. Hierbei muß stets scharf zwischen verschiedenen Arten des 'Beweisens' unterschieden werden: Dem formalistischen ('mathematischen') Beweisen innerhalb des formalen Systems, und dem inhaltlichen („metamathematischen") Beweisen über das System. Während das erstere ein willkürlich definiertes logisches Spiel ist (das freilich mit der klassischen Mathematik weitgehend analog sein muß), ist das letztere eine Verkettung unmittelbar evidenter inhaltlicher Einsichten. Dieses 'inhaltliche Beweisen' muß also ganz im Sinne der Brouwer-Weylschen intuitionistischen Logik verlaufen: Die Beweistheorie soll sozusagen auf intuitionistischer Basis die klassische Mathematik aufbauen und den strikten Intuitionismus so ad absurdum führen."

duces (on the basis of von Neumann's system) two formulas $a$ and $\neg a$, hence an inconsistency.

Von Neumann answered to Leśniewski's objections in the paper "Bemerkungen zu den Ausführungen von Herrn St. Leśniewski über meine Arbeit 'Zur Hilbertschen Beweistheorie' " (1931a). Analyzing the objections of Leśniewski he came to the conclusion that there is in fact a misunderstanding resulting from various ways in which they both understand principles of formalization. He used also the occasion to fulfil the gap in his paper (1927).

Add also that looking for a proof of the consistency of the classical mathematics and being (still) convinced of the possibility of finding such a proof (in particular a proof of the consistency of the theory of real numbers) von Neumann doubted whether there are any chances to find such a proof for the set theory – cf. his paper (1929).

## 4. Von Neumann and Gödel's Second Incompleteness Theorem

How much von Neumann was engaged in the realization of Hilbert's program and how high was his position in this group can be judged from the fact that just he has been invited by the organizers of the Second Conference on the Epistemology of Exact Sciences (organized by Die Gesellschaft für Empirische Philosophie)[11] held in Königsberg, 5–7th September 1930, to give a lecture presenting formalism – one of the three main trends in the contemporary philosophy of mathematics and the foundations of mathematics founded by Hilbert. The other two main trends: logicism and intuitionism were presented by Rudolf Carnap and Arend Heyting, resp.

In his lecture "Die formalistische Grundlegung der Mathematik" (cf. 1931b) von Neumann recalled basic presuppositions of Hilbert's program and claimed that thanks to the works of Russell and his school a significant part of the tasks put by Hilbert has already been realized. In fact the unique task that should be fulfilled now is "to find a finitistically combinatorial proof of the consistency of the classical mathematics" (1931b, p. 237). And he added that this task turned out to be difficult. On the other hand, partial results obtained so far by W. Ackermann, H. Weyl and himself make possible to cherish hopes that it can be realized. He finished his lecture by saying: "Whether this can be done for a more difficult and more important system of [the whole] classical mathematics will show the future" (1931b, p. 239).

On the last day of the conference, i.e., on 7th September 1930, a young Austrian mathematician Kurt Gödel announced his recent (not yet published)

[11] This conference was organized together with the 91st Convention of the Society of German Scientists and Physicians (Gesellschaft deutscher Naturforscher und Ärzte) and the 6th Conference of German Mathematicians and Physicists (Deutsche Physiker- und Mathematikertagung).

results on the incompleteness of the system of arithmetic of natural numbers and richer systems.

It seems that the only participant of the conference in Königsberg who immediately grasped the meaning of Gödel's theorem and understood it was von Neumann. After Gödel's talk he had a long discussion with him and asked him about details of the proof. Soon after coming back from the conference to Berlin he wrote a letter to Gödel (on 20th November 1930) in which he announced that he had received a remarkable corollary from Gödel's First Theorem, namely a theorem on the unprovability of the consistency of arithmetic in arithmetic itself. In the meantime Gödel developed his Second Incompleteness Theorem and included it in his paper "Über formal unentscheidbare Sätze der *Principia Mathematica* und verwandter Systeme. I" (cf. Gödel, 1931a). In this situation von Neumann decided to leave the priority of the discovery to Gödel.

## 5. Concluding Remarks

Gödel's incompleteness results had great influence on von Neumann's views towards the perspectives of investigations on the foundations of mathematics. He claimed that "Gödel's result has shown the unrealizability of Hilbert's program" and that "there is no more reason to reject intuitionism" (cf. his letter to Carnap of 6th June 1931 – see Mancosu 1999, p. 39 and p. 41, resp.). He added in this letter:

> Therefore I consider the state of the foundational discussion in Königsberg to be outdated, for Gödel's fundamental discoveries have brought the question to a completely different level. (I know that Gödel is much more careful in the evaluation of his results, but in my opinion on this point he does not see the connections correctly). (Mancosu 1999, p. 42)

Incompleteness results of Gödel changed the opinions cherished by von Neumann and convinced him that the program of Hilbert cannot be realized. In the paper "The Mathematician"(1947) he wrote:

> My personal opinion, which is shared by many others, is, that Gödel has shown that Hilbert's program is essentially hopeless. (p. 189)

Another reason for the disappointment of von Neumann's with the investigations in the foundations of mathematics could be the fact that he became aware of the lack of categoricity of set theory, i.e., that there exist various non-isomorphic models of set theory. The latter fact implies that it is impossible to describe the world of mathematics in a unique way. In fact there is no absolute description, all descriptions are relative.

Not only von Neumman was aware of this feature of set theory. Also Fraenkel and Thoralf Skolem realized this. And they have proposed various measures. In particular Fraenkel in his very first article "Über die Zermelosche Begründung der Mengenlehre” (1921) sought to render set theory categorical by introducing his Axiom of Restriction, inverse to the completeness axiom that Hilbert had proposed for geometry in 1899. Whereas Hilbert had postulated the existence of a maximal model satisfying his other axioms, Fraenkel’s Axiom of Restriction asserted that the only sets to exist were those whose existence was implied by Zermelo’s axioms and by the Axiom of Replacement. In particular, there were no urelements. One should add that Fraenkel did not distinguish properly a language and a metalanguage and confused them.

The approach of Skolem was different – but we will not go into technical details here (see, for example, Moore 1982, Section 4.9).

Von Neumann also examined the possible categoricity of set theory. In order to render it as likely as possible that his own system was categorical, he went beyond Mirimanoff and augmented it by the axiom stating that there are no infinite descending $\in$-sequences. He recognized that his system would surely lack categoricity unless he excluded weakly inaccessible cardinals (i.e., regular cardinals with an index being a limit ordinal). Von Neumann rejected also the Fraenkel’s Axiom of Restriction as untenable because it relied on the concept of subdomain and hence on inconsistent “naïve” set theory. He was also aware of the difficulties implied by Löwenheim-Skolem theorem.

Von Neumann treated the lack of categoricity of set theory, certain relativism of it as an argument in favor of intuitionism (cf. his 1925). He stressed also the distance between the naïve and the formalized set theory and called attention to the arbitrariness of restrictions introduced in axiomatic set theory (cf. 1925, 1928a, 1929). He saw also no rescue and no hope in Hilbert’s program and his proof theory – in fact the latter was concerned with consistency and not with categoricity.

One should notice here that von Neumann’s analyses lacked a clear understanding of the difference and divergence between first-order and second-order logic and their effects on categoricity. Today it is known, e.g., that Hilbert’s axioms for Euclidian geometry and for the real numbers as well as Dedekind-Peano axioms for the arithmetic of natural numbers are categorical in second-order logic and non-categoric in the first-order logic. Only Zermelo (perhaps under the influence of Hilbert[12]) claimed that the first-order logic is insufficient for mathematics, and in particular for set theory. It became the dominant element in Zermelo’s publications from the period 1929–1935. It is worth noting here that he spoke about this for the first time in his lectures held in Warsaw in May and June 1929.

[12]Hilbert and Ackermann wrote in (1928, p. 100): “As soon as the object of investigation becomes the foundation of [...] mathematical theories, as soon as we went to determine in what relation the theory stands to logic and to what extent it can be obtained from purely logical operations and concepts, then second-order logic is essential.” In particular they defined the set-theoretic concept of well-ordering by means of second-order, rather than first-order, logic.

After 1931 von Neumann ceased publishing on the mathematical logic and the foundations of mathematics – he came to the conclusion that a mathematician should devote his attention to problems connected with the applications. In (1947) he wrote:

> As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from "reality", it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely *l'art pour l'art*. [...] In other words, at a great distance from its empirical source, or after much "abstract" inbreeding, a mathematical subject is in danger of degeneration. (p. 196)

# 15

# CONTRIBUTION OF POLISH LOGICIANS TO DECIDABILITY THEORY[1]

## 1. Introduction

Researches on decidability problems have arisen from Hilbert's program.[2] To save the integrity of the classical mathematics and to overcome difficulties disclosed by antinomies and paradoxes of set theory Hilbert proposed in a serious of lectures in papers in the 1920s a special program. Its aim was to show that classical mathematics is consistent and that actual infinity, which seemed to generate the difficulties, plays in fact only an auxiliary role and can be eliminated from proofs of theorems talking only about finitary objects. To realize this program Hilbert suggested first of all to formalize the mathematics, i.e., to represent its main domains (including classical logic, set theory, arithmetic, analysis etc.) as a big formal system and investigate the latter (as a system of sequences of symbols transformed according to certain fixed formal rules) by finitary methods. Such formalization should be complete, i.e., axioms should be chosen in such a way that any problem which can be formulated in the language of a given theory can also be solved on the base of its axioms. Formalization yielded also another problem closely connected with completeness and called decision problem or decidability problem or *Entscheidungsproblem*: one could ask if a given formalized theory is decidable, i.e., if there exists a uniform mechanical method which enables us to decide in a finite number of steps (prescribed ahead) if a given formula in the language of the considered theory is or is not a theorem of it. Using notions from the recursion theory one can

[1]Originally published in *Modern Logic* 6 (1996), 37–66.  Reprinted with kind permission of the editor of *The Review of Modern Logic* (formerly *Modern Logic*).

[2]The upper bound of the period covered in the paper is fixed as 1963, i.e., the year in which A. Mostowski's paper "Thirty Years of Foundational Studies", *Acta Philosophica Fennica* **17** (1965), 1–180 was published.

In: Roman Murawski, *Essays in the Philosophy and History of Logic and Mathematics* (*Poznań Studies in the Philosophy of the Sciences and the Humanities*, vol. 98), pp. 211–231. Amsterdam/New York, NY: Rodopi, 2010.

make this definition more precise. A formal theory $T$ is said to be decidable if and only if the set of (Gödel numbers of) theorems of $T$ is recursive. Otherwise $T$ is called undecidable. If additionally every consistent extension of the theory $T$ (formalized in the same language as $T$) is undecidable then $T$ is said to be essentially undecidable. We should notice here that these definitions being completely standard today come from Alfred Tarski and were formulated in his paper *A general method in proofs of undecidability* published in the monograph Tarski-Mostowski-Robinson (1953).

In the 1920s and the 1930s a lot of results in this direction was obtained. Several general methods of proving completeness and decidability of theories were proposed and many theories were shown to be complete and decidable.

Kurt Gödel's results on incompleteness from 1931 (cf. Gödel 1931a) and Alonzo Church's result on undecidability (of the predicate calculus of first order and of Peano arithmetic and certain of its subtheories (cf. Church 1936a and 1936b) as well as results of J.B. Rosser on undecidability of consistent extensions of Peano arithmetic (cf. Rosser 1936) from 1936 indicated some difficulties and obstacles in realizations of Hilbert's program. One of their consequences was the fact that since the end of the 1930s logicians' and mathematicians' interests concentrated on proofs of undecidability rather than decidability of theories. General methods of proving undecidability were investigated and particular mathematical theories were shown to be undecidable.

Those general trends and tendencies have their reflection also in the history of logic in Poland. This is the subject of the present paper. It has the following structure. Section 2 will be devoted to the contribution to the study of decidability of theories due to Polish logicians and mathematicians. In particular the method of quantifier elimination studied by A. Tarski and his students together with various applications of it will be presented. Section 3 deals with investigations of undecidability of theories. Again we shall discuss some general methods of proving the undecidability of a theory developed by Polish logicians (in particular Tarski's method of interpretation will be considered) as well as several results on undecidability of particular theories. Section 4 will be devoted to the presentation of (forgotten and underestimated) results of Józef Pepis on mutual reducibility of decision problem for various classes of formulas. The final Section 5 will present generalizations and strengthenings of Gödel's incompleteness results due to Polish logicians.

## 2. Decidability of Theories

### 2.1. *Effective Quantifier Elimination*

The seminar led by A. Tarski at Warsaw University in the period 1927–1929 was devoted to the method of eliminating quantifiers. This method was initiated by L. Löwenheim in (1915) and used in fully developed form by Th. Skolem (1919) and C.H. Langford (1927a; 1927b). Skolem used it to show the decidability of the theory $T_0$ of the class of all "full" Boolean algebras, i.e., Boolean algebras of the form $\langle \mathcal{P}(A), \subseteq \rangle$, where $\mathcal{P}(A) = \{B : B \subseteq A\}$. Langford applied it to establish the decidability of the theory of a dense linear order: (a) without endpoints, (b) with the first element but no last element and (c) with first and last elements as well as the decidability (and completeness) of the theory of discrete orders having a first and no last element.

Elimination of quantifiers was studied at the seminar by Tarski and his students. Tarski described it as "a frequently used method, which consists in reducing the sentences to normal form and successively eliminating the quantifiers" (cf. Tarski 1936b; see also 1956d, p. 374 – this paper is the revised English translation of 1935 and 1936b).

Generally speaking this method can be described as follows: Let $T$ be a first-order theory in the language $L(T)$. We are looking for a set $\Phi$ of formulas in $L(T)$, called the set of basic formulas for $T$, such that for every formula $\varphi$ of $L(T)$ there exists a Boolean combination $\varphi^*$ of formulas from $\Phi$ having the same free variables as $\varphi$ and such that $T \vdash \varphi \equiv \varphi^*$. Of course one can always take as the basic set $\Phi$ the set of all formulas of the language $L(T)$. But we are looking for "good" basic sets consisting of "simple" formulas. Unfortunately there is no precise criterion for being "good" or "simple". Usually the following conditions are required: (1) $\Phi$ should be reasonably small and indispensable, (2) every formula in $\Phi$ should have some straightforward mathematical meaning, (3) there should exist an algorithm for reducing every formula $\varphi$ of the language $L(T)$ to its corresponding Boolean "representation". From the point of view of the decidability problems the set $\Phi$ should have one more property: (4) there should exist an algorithm which would tell us, given any basic sentence $\psi$, either that $\psi$ is a theorem of $T$ or that $\psi$ is refutable from $T$. If conditions (3) and (4) are met then we have both a completeness proof and a decision procedure for the theory $T$. Let us add that a method fulfilling the conditions (3) and (4) is sometimes called 'effective quantifier elimination' to distinguish it from other types of the elimination of quantifiers where there are no algorithms.

It seems that Tarski found this method adequate for his purposes and he did not try to generalize it but (together with his students) simply applied it to the study of various theories. In this way it became in his school really the method and a paradigm of how a logician should study an axiomatic theory.

What aims did Tarski and his students want to achieve using the method of quantifier elimination?

First they used it to characterize definability. Suppose namely that a structure $\mathcal{M}$ and a set of first-order sentences $T$ which are true in $\mathcal{M}$ are given. Suppose also that one has found a basic set $\Phi$ for the theory $T$. Then relations on $\mathcal{M}$ which are definable by first-order formulas (with or without parameters) are exactly Boolean combinations of the relations which are defined by formulas in $\Phi$. It seems that the first who made this point was Tarski in (1931). This paper introduces the decision procedure for the theory of reals but it does it in the guise of the way of describing the first-order definable relations on the reals. Tarski was greatly interested in the notion of a definable relation in a structure but his papers on decidability or completeness rarely mentioned this aspect of the results.

The second reason for using the method of quantifier elimination by Tarski and his collaborators was the fact that it could contribute to the study of decidability problems. One should notice here that in his papers published before the World War II Tarski seldom mentioned effective decidability. Describing his results he put emphasis on completeness proofs rather than on algorithmic decidability. On the other hand it was clear for him by 1930 that effective quantifier elimination could give (cf. p. 134 of 1956b, the English translation of 1931):

> a mechanical method which enables us to decide in each particular case whether a given sentence (of order 1) is provable or disprovable.

In (1939a) he refers to:

> The "effective" character of all positive proofs of completeness so far given – not only the problem of completeness but also the decision problem is solved in the positive sense for all the deductive systems mentioned above. (p. 176)

One can observe a very noticeable change of emphasis in Tarski's writings after the war. Now he stresses the importance of decidability results and gives them priority. On the first page of (1953) he speaks about the decision problem as "one of the central problems of contemporary metamathematics". Both of his abstracts (1949a) and (1949b) are opened by saying that he has found a decision procedure – and only later it is added that various consequences follow from the procedure. In his writings before the World War II it was just vice versa – the results on completeness were stressed at the first place and only at the end it was added that the proofs involve solving the decision problem.

In the monograph (1967) written in 1939 (and published in 1967) he says:

> It should be emphasized that the proofs sketched below have (like all proofs of completeness hitherto published) an "effective" character in the following sense: it is not merely shown that every statement of a given theory is, so to speak, in principle provable or disprovable, but at the same time a procedure is given which permits every such statement actually to be proved or disproved by means of proof of the theory. By the aid of such a proof not only the problem of completeness but also the *decision problem* is solved for the given system in a

> positive sense[11]. In other words, our results show that it is possible to construct a machine which would provide the solution of every problem in elementary algebra and geometry (to the extent described above). (p. 5)

And in Note 11 of (1967) we find the following words:

> It is possible to defend the standpoint that in all cases in which a theory is tested with respect to its completeness the essence of the problem is not in the mere proof of completeness but in giving a decision procedure (or in the demonstration that it is impossible to give such a procedure).

What were the reasons for this change? The first reason can be the fact that till the mid-1930s there was no precise definition of the term 'algorithm' and mathematicians doubted if there ever would be one. Hence it was impossible to state a theorem in the form "there is an algorithm for so-and-so". The works of A. Church, A. Turing, S.C. Kleene and J.B. Rosser and others and the development of recursion theory changed the situation. Tarski recognized it immediately and began to apply this theory in his papers.

The second source of Tarski's new perspective on his work on completeness/decidability problems was that he had become interested in proofs of undecidability publishing a lot of results during the years 1949–1953 (we discuss them in the next section).

Coming back to the aims Tarski and his students wanted to achieve using the method of quantifier elimination one should add one thing more. They saw namely that this method yielded much information not only on completeness and decidability but that it can be used to describe and classify all complete extensions of a given first-order theory $T$. In connection with this Tarski introduced in his seminar some key notions of model theory, in particular the notion of elementary equivalence.

To finish the general discussion of the method of quantifier elimination we want to consider the connection between completeness of a theory and its decidability (it was already mentioned above). We mean here the theorem stating that if a first-order theory $T$ based on a recursive set of axioms (in fact the term 'recursive' can be replaced by 'recursively enumerable') is consistent and complete then $T$ is decidable. The explicit formulation of it can be found in the paper by Antoni Janiczak (1950). He proved there the following

> THEOREM 1. *A complete, consistent theory T satisfying the conditions* (a')–(d') *is decidable.*

The conditions (a')–(d') say, resp., that the set of (Gödel numbers of) sentences of the language $L(T)$ of the theory $T$ should be recursive, the set of (Gödel numbers of) the axioms of $T$ should be recursively enumerable, that the arithmetized counterpart of the (metamathematical) relation: "a formula $\chi$ results from formulas $\varphi$ and $\psi$ by the deduction rule $\mathcal{R}$" is recursive and finally that there exists a recursive function $Neg$ such that if $x$ is a Gödel number of a formula $\varphi$ then $Neg(x)$ is the Gödel number of the negation of $\varphi$.

The key fact used in the proof of this theorem is the negation theorem stating that a relation is recursive if and only if it and its complement are recursively enumerable. One finds this result in Kleene's paper (1943) (Theorem V) and in the paper by Janiczak (1955a) (being a posthumous work prepared by A. Grzegorczyk from the notes left by the author who died prematurely in Warsaw on 5th July 1951).

At the end of Janiczak's paper (1950) it is written: "It is worth remarking, in connection with our theorem, that each decidable and consistent theory can be enlarged to a decidable, complete and consistent theory by the method of Lindenbaum [...]. This result is due to Tarski." And Janiczak refers here to Theorem II of Tarski's abstract *On essential undecidability* (1949c).

### 2.2. *Applications: Decidability of Particular Theories*

The first decidability results of Tarski came from 1926–1928 (cf. Tarski 1935 and 1936b). They were based on the work of Langford mentioned at the beginning of this section. More exactly Tarski proved the following theorems.

> THEOREM 2. *If T is the theory of dense linear orders then for any first-order sentence $\varphi$ in the language of T we can compute a Boolean combination of the sentences "There is a first element" and "There is a last element" which is equivalent to $\varphi$ modulo T.*

> THEOREM 3. *Let T be the theory of linear orders where every element except the first element has an immediate predecessor, and every element except the last element has an immediate successor. Then for any first-order sentence $\varphi$ in the language of T we can compute a Boolean combination of the sentences "There is a first element", "There is a last element" and "There are at most n elements" (for positive integers n) which is equivalent to $\varphi$ modulo T.*

In the Appendix to (1936b) Tarski listed all the complete theories of dense linear orders: they are the theories of the orders $\eta$, $1 + \eta$, $\eta + 1$ and $1 + \eta + 1$, where $\eta$ is the natural ordering of the rational numbers. The complete theories of linear orders are: theories of finite orders and the theories of the orders $\omega$, $\omega^*$, $\omega^* + \omega$ and $\omega + \omega^*$ (where $\omega$ is the natural ordering of the natural numbers and $\omega^*$ is its inverse). Those classifications were obtained with the help of the above quoted theorems.

Having solved the problem of completeness and decidability of the theory of dense linear orders and of the theory of discrete orders one should ask about the theory of well-orderings. The study of this problem has a long and interesting history.

In the late 1930s A. Tarski and his student Andrzej Mostowski developed the outline of a proof of the decidability of this theory described semantically as the set of sentences true in all well-ordered structured. They used the method of the elimination of quantifiers and by the summer of 1939 they reached a clear idea of the basic formulas. Their main aim was to prove the adequacy of certain axiom systems and to classify the complete extensions of the theory (decidability would be then a by-product of it). Many technical details remained to be worked out. The work was interrupted by World War II. Tarski escaped the German occupation of Poland and stayed in the USA, Mostowski spent the war time in Poland. Each began to work out the technical details. Unfortunately Mostowski's notes were destroyed in Warsaw in 1944 and Tarski's ones lost in the course of his many moves. Hence the work had to be started from the very beginning. They published the abstract (1949a) and made plans to reconstruct the proof. This was not realize and nothing more was done than writing down the specification of the basic formulas.

In 1964 Tarski assigned his student John Doner a task to work out some of the details. This was done in a rough form and again nothing happened till 1975. In this year Mostowski and Tarski met and made plans to finish the paper. Unfortunately Mostowski's unexpected death in August 1975 upset the plans. In this situation Tarski invited Doner to take up the work. The final result was the joint paper of Doner, Mostowski and Tarski *The elementary theory of well-ordering. A metamathematical study* (1978). One finds there a number of results not mentioned in Mostowski-Tarski (1949a).

The next theory studied by Tarski by the method of quantifier elimination was the theory of Boolean algebras. Tarski never published either the decision procedure or a proof of the classification of complete first-order theories of Boolean algebras. One can reconstruct the essential elements of them from his abstract (1949a). Tarski announced there that he had a decision procedure for the theory of Boolean algebras and he used it to classify all the complete first-order theories of Boolean algebras in terms of countably many algebraic invariants. Each invariant was expressible as a single first-order sentence. In the monograph (1948) he mentions on page 1 that he found this decision procedure in 1940.

In connection with these results one should note that already in the paper (1936b) Tarski published the analogue of Theorem 3 for atomic Boolean algebras. He even claimed that if we drop the assumption of atomicity, then there are just countably many completions of the axioms of "the algebra of logic". He seems to mean here that there are just countably many complete first-order theories of Boolean algebras. But it is not clear how could he have proved this result without having the full classification.

One should also stress that the proofs of the classification we have today (they are model-theoretic), i.e., proofs by Yu.L. Ershov and H.J. Keisler do not give unlike Tarski's a primitive recursive decision procedure (though they imply the decidability of the theory of Boolean algebras).

Discussing the problem of the decidability of the theory of Boolean algebras one should also mention the paper by S. Jaśkowski (1949).

The method of quantifier elimination was applied by Tarski also to the study of geometry. M. Presburger writes in the (1930) (footnote on p. 95) that in 1927–1928 Tarski proved the completeness of a set of axioms for the concept of betweeness ("*b* lies between *a* and *c*") and that of the equidistance ("*a* is as far from *b* as *c* is from *d*"). The method used by him was quantifier elimination. Tarski himself saw this result as "a partial result tending in the same direction" as his later theorem on real-closed fields (cf. 1948, footnote 4).

Discussing Tarski's results on decidability of geometry one should also mention his paper (1959). Using the results on real-closed fields (cf. Tarski 1948) (we shall discuss them later) he studied various theories of elementary geometry defining the latter by the words: "we regard as elementary that part of Euclidean geometry which can be formulated and established without the help of any set-theoretical devices" (p. 16). In particular he considered there the system $\mathcal{E}_2$ in the language containing predicates for the betweeness relation and for the equidistance relation and based on the following axioms: identity, transitivity, connectivity for betweeness, reflexivity, identity and transitivity for equidistance, Pasch's axiom, Euclid's axiom, five-segment axiom, axiom of segment construction, lower and upper dimension axioms and elementary continuity axiom. It is proved that the theory $\mathcal{E}_2$ is complete, decidable and is not finitely axiomatizable. It is also shown that a variant $\mathcal{E}_2''$ of $\mathcal{E}_2$ (it is a theory obtained from $\mathcal{E}_2$ by replacing the elementary continuity axiom, which is in fact a scheme of axioms, by a weaker single axiom) is decidable with respect to the set of its universal sentences.

Another domain where Tarski applied the method of quantifier elimination were fields. In the paper (1931) Tarski wrote that he had a complete set of axioms for the first-order theory of the reals in the language with primitive nonlogical notions 1, $\leqslant$ and $+$. The paper contains a sketch of the quantifier elimination procedure for this theory and the description of those relations on the reals which are first-order definable in the language with the indicated primitive notions.

The most important and most famous result of Tarski for fields concerns the theory in the richer language with $0, 1, +, \cdot, \leqslant$ as nonlogical primitive notions. We mean here his fundamental theorem.

> THEOREM 4. *To any formula $\varphi(x_1, \ldots, x_m)$ in the language with $0, 1, +, \cdot, \leqslant$ one can effectively associate: a quantifier free formula $\varphi^*(x_1, \ldots, x_m)$ in the same language and a proof of the equivalence $\varphi \equiv \varphi^*$ that uses only the axioms for real closed fields.*

The first announcement of this result can be found in Tarski's abstract (1930a) where he wrote:

> In order that a set of numbers $A$ be arithmetically definable it is necessary and sufficient that $A$ be a union of finitely many (open or closed) intervals with algebraic endpoints

(English translation – L. van den Dries 1988). This follows immediately from Theorem 4 for $m = 1$. No information on the proof can be found in the abstract. One should notice that the emphasis was put on definability (cf. our earlier remarks).

A precise formulation of the fundamental theorem and a clear outline of its proof were given in the monograph *The Completeness of Elementary Algebra and Geometry* (1967). This work was written in 1939 but the war made its publication impossible then (the paper reached the stage of page proofs but publication was interrupted by wartime developments). The title of it suggests a change of emphasis from definability to problems of completeness.

A full and detailed proof of Theorem 4 finally appeared in Tarski's work *A Decision Method for Elementary Algebra and Geometry* (1948) prepared for publication by J.C.C. McKinsey. The title of it reveals a second change of emphasis – this time from completeness to decidability. In the Preface to the second edition (from 1951) Tarski wrote:

> As was to be expected it reflected the specific interests which the RAND corporation found in the results. The decision method [...] was presented in a systematic and detailed way, thus bringing to the fore the possibility of constructing an actual decision machine. Other, more theoretical aspects of the problems discussed were treated less thoroughly, and only in notes. (p. 3)

Note that the results in Tarski (1948) were formulated in terms of the field of real numbers but they hold generally for real-closed fields – the latter are mentioned only in footnotes.

Real-closed fields and algebraically closed fields were discussed by Tarski in his abstract (1949b). One finds there a description (in an algebraic language) of each class of the form: all models of some complete first-order theory $T$ which are algebraically closed (or real-closed) fields. Tarski remarks also that he found a decision procedure for the theory of algebraically closed fields and says that his classification follows from this procedure. A decision procedure for real-closed fields based on an extension of Sturm's theorem to arbitrary systems of algebraic equations and inequalities in many unknowns is also mentioned. It implies that the theory of real-closed fields is consistent and complete and that any two models of this theory are elementarily equivalent.

As we mentioned earlier the method of effective quantifier elimination was used not only by Tarski but also by his students. Among results obtained by the latter one should mention here Mojżesz Presburger's result on decidability of the arithmetic of addition and Wanda Szmielew's result on decidability of the theory of abelian groups.

In the school year 1927/28 A. Tarski gave a course of lectures on first-order theories. During this course he presented a set of axioms for the theory of addition of natural numbers. It is formalized in a first-order language with $0, S$ and $+$ as the only nonlogical primitive notions (hence there is no multiplication). One calls it today Presburger arithmetic. Tarski formulated a problem of showing that the axioms were complete. It was solved by Presburger in May 1928 and published in (1930). The result was presented also as his master thesis.

The method used by Presburger was effective quantifier elimination of course. It was shown that as a set of basic formulas one can take the set consisting of formulas of the form: $\alpha x + a = b$, $\alpha x + a \leqslant b$, $b < \alpha x + a$, $\alpha x + a \equiv_n b$, where $a$ and $b$ are terms in which the variable $x$ does not occur free, $\alpha$ is a natural number, the symbol $\alpha x$ is an abbreviation for $\underbrace{x + \ldots + x}_{\alpha \text{ times}}$, the relations $< n$ and $\leqslant$ are defined in the usual way and the relation $\equiv_n$ is defined as follows:

$$x \equiv_n y \equiv \exists z\,(x = y + (\underbrace{z + \ldots + z}_{n}) \vee y = x + (\underbrace{z + \ldots + z}_{n})).$$

In this way one obtains the completeness and the decidability of the considered theory (notice that Presburger, like Tarski at that time, formulated his result in terms of completeness and did not mention decidability).

One should also add here that similar results for the theory of the successor (in the first-order language with the primitive nonlogical notions 0 and $S$ only) and for the theory of the multiplication (in the first-order language with $0, S$ and $\cdot$ only) of natural numbers were obtained by J. Herbrand (1928) and Th. Skolem (1930), resp. They used the method of quantifier elimination as well.

The second important result obtained in Tarski's school by the method of the effective elimination of quantifiers was the result of Wanda Szmielew on decidability of the theory of Abelian groups (cf. Szmielew 1949a, 1949b and 1955). She gave a classification of all complete first-order theories of Abelian groups. It was done by describing a set of algebraic invariants which were expressible by first-order sentences. Those results have various consequences (noted by the author, e.g., in Szmielew 1955). Using them one can obtain many examples of non-elementarily definable (the author says, non-arithmetical) classes, e.g., the class of all finite groups, the class of all simple groups, the class of all torsion groups and that of all torsion-free groups. The fundamental result on the existence of the basic set for the theory of Abelian groups implies also that there exist two infinite groups of the same power (for instance two denumerable groups) which are elementarily equivalent but non-isomorphic.

## 3. Undecidability of Theories

As we mentioned above the results of K. Gödel, J.B. Rosser and A. Church on undecidability and essential undecidability shifted the interests of logicians towards undecidability of theories. Polish logicians followed this shift and contributed to those researches as well. This section is devoted just to this. First, the work of Tarski on the general methods of establishing the (essential) undecidability of first-order theories will be discussed. Second, some applications of those methods (due to Polish logicians) will be indicated.

### 3.1. *General Methods of Proving Undecidability*

The most important work in this respect is the paper by A. Tarski *A General Method in Proofs of Undecidability* published as part one of the monograph Tarski-Mostowski-Robinson (1953) (cf. Tarski 1953a). It contains his results obtained during 1938–1939 (and summarized in the abstract 1949c).

Tarski distinguishes there two types of methods of proving undecidability: the direct method and the indirect one. The first one was applied by Gödel, Church and Rosser (in the above quoted papers). It uses the notion of recursive function and relation and is based on the fact that all recursive functions and relations are strongly representable in a considered theory. Hence it may be applied only to theories in which a sufficient number-theoretical apparatus can be developed.

The second method, indirect one, "consists in reducing the decision problem for a theory $T_1$ to the decision problem for some other theory $T_2$ for which the problem has previously been solved" (cf. Tarski 1953a, p. 4). This reduction can take place in two ways: to establish the undecidability of a theory $T_1$ one can try to show that either (1) $T_1$ can be obtained from some undecidable theory $T_2$ by deleting finitely many axioms (and not changing the language) or that (2) some essentially undecidable theory $T_2$ is interpretable in $T_1$.

Just the second way was elaborated by Tarski. Before describing Tarski's results let us notice here that the notion of interpretability, i.e., of definability of the fundamental notions of one theory in another theory , is a key ingredient in Tarski's investigations. Recall what has been said above on Tarski's motivations behind his work on the effective quantifier elimination. It should be added that the results on undecidability of theories were very often only one kind of consequences of Tarski's results on definability and interpretability.

In the paper (1953a) Tarski distinguishes interpretability and relative interpretability, each of them in two forms: full (in this case he is speaking simply about the appropriate interpretability without any adjectives) and weak.

Let $T_1$ and $T_2$ be two first-order theories. Assume that they have no nonlogical constants in common (by the appropriate change of symbols this assumption can always be fulfilled). The theory $T_2$ is said to be interpretable in the theory $T_1$ if and only if we can extend $T_1$ by adding to its axioms some possible definitions of nonlogical primitive notions of $T_2$ in such a way that the extension turns out to be an extension of $T_2$ as well. The theory $T_2$ is weakly interpretable in the theory $T_1$ if and only if $T_2$ is interpretable in some consistent extension of $T_1$ formalized in the same language $L(T_1)$. The main theorem concerning the undecidability is now the following one (cf. Tarski 1953a, Theorem 8):

> THEOREM 5. *Let $T_1$ and $T_2$ be two theories such that $T_2$ is weakly interpretable in $T_1$ or in some inessential extension of $T_1$. If $T_2$ is essentially undecidable and finitely axiomatizable then* (i) *$T_1$ is undecidable and every subtheory of $T_1$ which has the same constants as $T_1$ is undecidable;* (ii) *there exists a finite extension of $T_1$ which has the same constants as $T_1$ and is essentially undecidable.*

Recall that an extension $S$ of a theory $T$ is called inessential if and only if every constant of $S$ which does not occur in $T$ is an individual constant and if every theorem of $S$ is provable in $S$ on the bases of theorems of $T$.

Weak interpretability and the above Theorem 5 widen in a considerable way the range of applications of the method of interpretations in proving the undecidability of theories. A further widening is provided by the notion of the relative interpretability and weak relative interpretability. They are defined in the following way: A theory $T_2$ is said to be relatively interpretable (weakly relatively interpretable) in a theory $T_1$ if and only if there exists a unary predicate $P$ which does not occur in $T_2$ and such that the theory $T_2^{(P)}$ (obtained by relativizing $T_2$ to the predicate $P$) is interpretable (weakly interpretable) in $T_1$ in the sense explained above. It can be shown that for any theory $T$ and any unary predicate $P$ which is not a constant of $T$, the theory $T^{(P)}$ is essentially undecidable if and only if the theory $T$ is essentially undecidable.

It happens very often that one can easily show that a certain theory $T_2$ known to be essentially undecidable is relatively interpretable (or weakly relatively interpretable) in a given theory $T_1$, while the proof of interpretability or weak interpretability of $T_2$ in $T_1$ is either impossible or much more difficult. Hence the combination of (weak) relative interpretability and Theorem 3.1 is a proper strengthening of the method of interpretation.

### 3.2. *Applications: Undecidability of Particular Theories*

To apply the method of interpretation described above one has to have a finitely axiomatizable, essentially undecidable theory which is weak enough to be interpreted in theories even quite distant from it. Peano arithmetic and its extensions as well as various versions of set theory which were known in the late 1930s to be essentially undecidable could not play this role. Peano arithmetic is not finitely axiomatizable and set theory is too rich.

The problem was solved by A. Mostowski, A. Tarski and Raphael M. Robinson. In 1939 Mostowski and Tarski constructed a finitely axiomatizable and essentially undecidable subtheory $\overline{Q}$ of the arithmetic of natural numbers. It was closely related to the theory of non-densely ordered rings (cf. Mostowski-Tarski 1949b) where an analogous theory is described). Around 1949/50 R.M. Robinson and A. Tarski simplified this system and finally a simple, finitely axiomatizable and essentially axiomatizable theory $Q$ arose. It was described in the paper by A. Mostowski, R.M. Robinson and A. Tarski *Undecidability and Essential Undecidability in Arithmetic* (1953). It is the first-order theory in the language with $S, 0, +$ and $\cdot$ as nonlogical primitive notions and based on the following axioms:

$$Sx = Sy \longrightarrow x = y,$$

$$0 \neq Sx,$$

$$x \neq 0 \longrightarrow \exists y\,(x = Sy),$$

$$x + 0 = x,$$

$$x + Sy = S(x + y),$$

$$x \cdot 0 = 0,$$

$$x \cdot Sy = (x \cdot y) + x.$$

It is shown in Mostowski-Robinson-Tarski (1953) that $Q$ is essentially undecidable and that no axiomatic subtheory of $Q$ obtained by removing any one of its axioms is essentially undecidable. Essential undecidability of $Q$ was proved by a direct method based on ideas found in Tarski (1933a) and (1936a) as well as in Gödel (1931a). The result of Tarski stating that the diagonal function and the set of Gödel numbers of theorems of $Q$ cannot be both strongly represented in $Q$ was used (in fact this result holds for any consistent and axiomatizable extension of a fragment of $Q$ denoted in Mostowski-Robinson-Tarski (1953) by $R$). Since the diagonal function is recursive and hence strongly representable in $Q$, it follows that $Q$ is essentially undecidable.

It is worth noticing here that the key step in the proof of the above theorem is to show that all recursive functions and sets are strongly representable. It is based on a characterization of recursive functions found by Julia Robinson in (1950). And again the construction of the theory $Q$ arose by a keen insight into the semantical notion of definability (strong representability is a kind of definability).

Another example of a theory with properties similar to those of $Q$, i.e., finitely axiomatizable and essentially undecidable was constructed by Andrzej

Grzegorczyk in (1962a). In fact two theories $F$ and $F^*$ are given there. The theory $F$ is a first-order theory formalized in a language with two individual constants 0 and $S$ and one binary function symbol $|$. Its nonlogical axioms are the following:

(F1) $\forall x\,(0 \neq S \mid x)$,

(F2) $\forall x\,\forall y\,(S \mid x = S \mid y \longrightarrow x = y)$,

(F3) $\forall x\,(x = 0 \vee \exists y\,(x = S \mid y))$,

(F4) $\forall x\,\forall y\,\exists z\,\{z \mid 0 = x\ \&\ [z \mid (S \mid u) = y \mid (z \mid u)]\}$.

The theory $F^*$ is formalized in the same language as $F$ and the set of its nonlogical axioms consists of (F1)–(F3) plus the following axioms:

(F*4) $\forall f\,\forall a\,\exists g\,\{g \mid 0 = a\ \&\ \forall x\,(g \mid (S \mid x) = f \mid x)\}$,

(F*5) $\exists g\,\forall x\,(g \mid x = 0)$.

The theories $F$ and $F^*$ have common extensions but they are independent, i.e., (F*5) is not a theorem of $F$ and (F4) is not a theorem of $F^*$. Now using the fact that all recursive functions are strongly representable in $F$ it is proved that $F$ is essentially undecidable (hence the direct method is applied here). The essential undecidability of $F^*$ is proved by showing that a weak essentially undecidable set theory of R.L. Vaught can be interpreted in it (the theory of Vaught is obtained by a simplification of the theory considered by Szmielew and Tarski in (1952).

After the publication of the paper (1953) by Mostowski, Robinson and Tarski the research activity concerning undecidable theories sharply increased. There were at least three centers of them: Berkeley (where researches were led under the leadership of Tarski), Princeton (where Gödel was a member of the Institute for Advanced Study and where Church, Rosser and Kleene had done their pioneering work in the theory of recursive functions) and Novosibirsk (under the leadership of A.I. Mal'cev). The reason for this increase of interest was the fact that the method of interpretation together with the finitely axiomatizable and essentially undecidable system $Q$ opened the way for various applications. We shall indicate here some of them restricting ourselves – according to the subject of this paper – only to those due to Polish logicians.

Some applications were mentioned already in the paper by Mostowski, Robinson and Tarski (1953). In particular it was shown there that certain theories of integers as well as various algebraical theories are undecidable. More precisely it was proved (Theorem 12) that the arithmetic $J$ of arbitrary integers (in the language with $+$ and $\cdot$ only) and all its subtheories (in the same language) are undecidable and that there are finitely axiomatizable subtheories of $J$ which are essentially undecidable. The same holds for the theory $J^<$ of integers formalized in the language with $+, \cdot$ and $<$. Both theories $J$ and $J^<$ are defined semantically as the set of all sentences (in the indicated languages) true in the structure $\langle I, +, \cdot \rangle$ (or, resp., $\langle I, <, +, \cdot \rangle$), where $I$ is the set of all integers and the functions and relation have their usual meaning.

Another group of results in Mostowski-Robinson-Tarski (1953) concerns undecidability of various algebraical theories. In particular it is proved that the elementary theories of rings, commutative rings, integral domains, ordered rings and ordered commutative rings, without or with unit, are undecidable (Corollary 13) and that the elementary theories of non-densely ordered rings and non-densely ordered commutative rings, without or with unit, are essentially undecidable (Theorem 14).

A. Tarski proved in 1946 the undecidability of the elementary theory of groups – this result was announced in (1949d) and expounded fully in the paper *Undecidability of the Elementary Theory of Groups* published in Tarski-Mostowski-Robinson (1953) (cf. Tarski 1953b). Using (relative) interpretability of various systems of arithmetic of integers it is proved that the theory of groups $G$ (formalized in the language with $\cdot$ as the only nonlogical constant) as well as every subtheory of $G$ (in the same language as $G$) are undecidable and that there exists a finitely axiomatizable extension of $G$ which has the same nonlogical constant as $G$ and which is essentially undecidable.

It should be noticed here that a weaker result in the same direction was announced by S. Jaśkowski in (1948).

A. Tarski proved in (1949e) the undecidability of the following theories: the theory of modular lattices, the theory of arbitrary lattices, the theory of complemented modular lattices and the theory of abstract projective geometries. Again the method of interpretation was used. It was also notice that the indicated theories are not essentially undecidable since the theories of Boolean algebras and of real projective geometry are decidable (cf. previous section).

A. Tarski and W. Szmielew considered undecidability of various weak fragments of set theory (cf. Szmielew-Tarski 1952 and Tarski 1953a). In particular they proved (by interpreting the system $Q$) that a small fragment $S$ of set theory is essentially undecidable. The theory $S$ is formalized in the language with two nonlogical constants: $E$ (= being a set) and the membership relation $\in$, and based on the set of axioms stating that: (i) any two sets with the same elements are identical, (ii) there is a set with no elements and (iii) for any two sets $a$ and $b$ there is a set $c$ consisting of those and only those elements which are elements of $a$ or are identical with $b$. From the theorem of Tarski and Szmielew it follows that every consistent theory which is an extension of $S$ is essentially undecidable, hence all axiomatic systems of set theory (with $E$ and $\in$ as nonlogical constants) which are known from the literature are essentially undecidable. The result can be extended to systems of set theory formulated in the language with $\in$ only.

A. Tarski and Lesław W. Szczerba considered undecidability of various geometrical theories. In particular in Tarski's paper (1959) it is shown that the system $\mathcal{E}_2'$ of geometry obtained from the system $\mathcal{E}_2$, described in the previous section, by supplementing it with a small fragment of set theory is essentially undecidable. In (1979) Szczerba and Tarski studied among others undecidability of various systems of affine geometry.

Using the general method of interpretability of Tarski together with results of Mostowski, Robinson and Tarski on the theory $Q$, A. Grzegorczyk considered in (1951) the undecidability of some topological theories. In particular he proved that:

(1) there exists an elementary theory $T$ of closure algebra which is undecidable and finitely axiomatizable and such that each theory of closure algebra consistent with $T$ is undecidable (the undecidability of the closure algebra was also proved by another methods by S. Jaśkowski in 1939, cf. Jaśkowski 1948),

(2) there exists an elementary theory $T$ of the algebra of closed sets such that $T$ is essentially undecidable and finitely axiomatizable and such that every Brouwerian algebra consistent with $T$ is undecidable (one of consequences of this theorem is the undecidability of the abstract algebra of projective geometry and of the general lattice-theory – those results were obtained by another method by Tarski in 1949e),

(3) the algebra of bodies is undecidable,

(4) every algebra of convexity true in an Euclidean space $E_n$, for $n \geqslant 2$, is undecidable,

(5) every semi-projective algebra true in the Euclidean space $E_n$, $n \geqslant 2$, is undecidable.

The main idea of proofs of the above theorems is that the arithmetic $Q$ can be interpreted as an arithmetic of finite sets.

Applying the method of interpretability and using the undecidability of the theory of non-densely ordered rings (see above) Antoni Janiczak proved the undecidability of some simple theories of relations and functions. These results were contained in his master thesis submitted, shortly before his unexpected death in July 1951, to the Faculty of Mathematics of the University of Warsaw. They were published in the paper (1953) prepared for print by A. Mostowski with the assistance of A. Grzegorczyk. Janiczak proved the undecidability of the theory of two equivalence relations, of the theory of two equivalence relation such that their intersection is the identity relation, of the theory of one equivalence relation and one bijection and of the theory of 1–1 relation and one function (one-many relation). It is also mentioned in Janiczak's paper (1953) that the theory of one equivalence relation is decidable (this can be shown by the method of quantifier elimination).

## 4. Reducibility Results

Another approach to decidability problems (*Entscheidungsproblem*) was represented by Józef Pepis (a mathematician active at the University of Lvov, killed by the Gestapo in August 1941). He distinguishes explicitly three aspects of it (cf. Pepis 1937): tautology decision problem (*Allgemeingültigkeitsproblem*), satisfiability decision problem (*Erfüllbarkeitsproblem*) and deducibility decision problem (*beweistheoretisches Entscheidungsproblem*). The first problem consists of finding a uniform mechanical method – or proving that there is no such method – which would enable us to decide in a finite number of steps if a given formula is a tautology. In the second case one asks if there exists a uniform and mechanical method of deciding in a finite number of steps if a given formula can be realized, i.e., if it has a model. Hence this aspect has a semantical character. The last case concerns syntactical properties – one asks here if there exists a method with the indicated properties which would enable us to decide in a finite number of steps if a given formula is a theorem of a considered theory, i.e., it it can be deduced from the set of its axioms. Observe that so far we were interested and we discussed mainly the third aspect. Note also that all those aspects are equivalent, i.e., a positive (or negative) solution to one of them yields a positive (or negative) solution to the others. Hence one can speak here simply about the decidability problem. Pepis says in (1937) that the most convenient approach to *Entscheidungsproblem* is just the second one, i.e., the decidability problem for satisfiability – and therefore he concentrates himself just on it.

On the other hand one can distinguish between a direct approach and an indirect one. The first one consists of solving the decision problem for a given particular theory, the latter – in reducing of a given general decision problem to the decision problem in some particular cases. Pepis was interested just in the second approach and all his papers are devoted to the study of various reducibility procedures.

He published four papers on decidability and reducibility. In all of them the first-order predicate calculus (formalized in a language with propositional variables and identity relation) (*enger logischer Funktionenkalkül* as Pepis used to call it) was studied from the point of view of reducibility of the satisfiability decision problem for one class of formulas to the satisfiability decision problem for another class of formulas.

The first paper was published in 1936 (cf. Pepis 1936). Results contained in it were generalized in Pepis' doctoral dissertation (1937) submitted to the Jan Kazimierz University in Lvov. The third paper (1938a) contains new results on reducibility which generalize the results of Ackermann (1936) and Kalmár (1936). The fourth paper (1938b) is devoted to the introduction and discussion of a certain new, simple and general reduction procedure.

It is impossible to quote here all the results of Pepis. We shall only indicate – as a sample – some examples to show of what type they are. The phrase: 'By considering the satisfiability decision problem for the first-order predicate calculus we can restrict ourselves without the loss of generality to formulas with the given property $E$' means that any formula of the first-order predicate calculus is equivalent – from the point of view of satisfiability – to a formula with the property $E$.

Pepis proved among others that by considering the satisfiability decision problem for the first-order predicate calculus we can restrict ourselves without the loss of generality to the following formulas:

1. formulas in the prenex normal form in which a unique 3-ary predicate occurs and which possess the Skolem prefix of the following form
$$\forall x_1 \, \forall x_2 \, \forall x_3 \, \forall x_4 \, \exists y_1 \, \exists y_2 \ldots \exists y_n,$$
2. formulas in the prenex normal form with the prefix
$$\forall x \, \forall y \, \exists z \, \forall x_1 \ldots \forall x_n$$
and such that the matrix contains only two (one unary and one 3-ary) predicates,
3. formulas in the prenex normal form with the prefix
$$\forall x_1 \, \forall x_2 \ldots \forall x_n \, \exists y$$
and such that the metrix contains only two (one unary and one 3-ary) predicates,
4. formulas in the prenex normal form with the prefix
$$\exists x \, \forall y \, \exists z \, \forall u_1 \, \forall u_2 \ldots \forall u_n$$
and such that the metrix contains only one unary, one binary and one 3-ary predicates,
5. formulas of the form
$$\forall y \, \forall z \, \exists x \, \Phi(x, y, z) \;\&\; \forall x_1 \, \forall x_2 \ldots \forall x_n \, \mathcal{A}(x_1, x_2, \ldots, x_n)$$
where $\Phi$ is a predicate and the formula $\mathcal{A}$ contains, besides $\Phi$, one further (unary) predicate,
6. formulas of the form
$$\begin{aligned} &\forall y \, \forall z \; \exists x \; (R_1(x, y) \,\&\, R_2(x, y))\& \\ &\qquad\qquad \& \; \forall x_1 \, \forall x_2 \ldots \forall x_n \, \mathcal{A}(x_1, x_2, \ldots, x_n) \end{aligned}$$
where $R_1$ and $R_2$ are predicates and the formula $\mathcal{A}$ contains, besides $R_1$ and $R_2$, one further (unary) predicate and symbols $R_1$ and $R_2$ occur in $\mathcal{A}$ only negatively.

Results of Pepis lost partially their meaning in the light of the theorem of Church (cf. 1936b) which showed that the first-order predicate calculus is undecidable and in this way gave the negative solution to its decision problem.

Nevertheless their value consists in the indication of reducibility properties for various classes of formulas. On the other hand it seems that Pepis did not accept the Church thesis and he did not share the opinion that recursive functions comprise all effective methods (see his paper 1937, p. 169–170). Hence he was convinced that the results of Church did not solve definitely the problem and treated the decidability problem for the predicate calculus as still opened.

Reducibility methods similar to those used by J. Pepis were applied by Stanisław Jaśkowski to study decision problem for various mathematical theories. In particular in (1948) certain reductions of the decision problem for the first-order predicate calculus to decision problems for various topological and group-theoretical expressions are announced. In the paper (1954) Jaśkowski proved that the decision problem for the predicate calculus is equivalent to the problem whether or not for every $\Theta$ of the interval $(-1, 1)$, the system of ten ordinary differential equations given by him explicitly possesses a real solution over the interval $(-1, 1)$ satisfying the particular initial condition. At the end of the paper it is stated that the negative solution to the former (which follows from the theorem of Church) yields the negative solution to the latter.

In the paper Jaśkowski (1956) some generalizations of Pepis' results to algebraic structures can be found. A short proof of one of the reducibility theorems of Pepis is also given. The algebraic structures considered by Jaśkowski are free groupoids. It is shown in particular that in the case of a free groupoid $\mathcal{F}$: (1) the satisfiability problem for the class of elementary sentences is reducible to the decision problem for the class of first-order sentences with the prefix

$$\exists E\, \forall x_1 \ldots \forall x_n$$

in $\mathcal{F}$, where $E$ is a unary predicate, (2) the tautology problem for the class of elementary sentences is reducible to the decision problem for the class of first-order sentences with the prefix

$$\forall E\, \exists x_1 \ldots \exists x_n$$

in $\mathcal{F}$.

## 5. New Proofs of the Incompleteness Theorem

Discussing the contribution of Polish logicians to decidability problems one should also mention works devoted to generalizations of Gödel's classical theorems on incompleteness as well as scientific articles for the general public presenting results on decidability.

We start with a paper by A. Tarski (1939b). An enlarged system of logic is considered there, the enlargement being obtained by adding rules of inference of a "non-finitary" ("non-constructive") character. The existence of undecidable statements in such systems is shown. The author emphasizes the part played by the concept of truth in relation to problems of this nature. One should also note certain kinship of those results with the results of Rosser (1936).

Another author whose contribution to the classical incompleteness theorems should be mentioned here is A. Mostowski. In his paper (1949a) one finds an interesting construction of a new undecidable sentence. The main property of this sentence is that it is set-theoretical in nature, is stronger than Gödel's sentence and is not effective. Nevertheless its content is distinctly mathematical and intuitive. Its construction does not use the arithmetization of syntax and the diagonal process – as it was the case by Gödel and other authors. Instead of them Mostowski uses some set-theoretical lemmas and Skolem-Löwenheim theorem. Mostowski's undecidable sentence is stronger than Gödel's one in the sense that the latter ceases to be undecidable if one adds to the system the infinite $\omega$-rule. The former has not this property – there is no "reasonable" rule of inference which, added to the considered system, would decide it. The proof of its undecidability is non-finitary – it rests on the axioms of the Zermelo-Fraenkel set theory including the axiom of choice (in fact it can be eliminated from the proof) and an axiom ensuring the existence of at least one inaccessible cardinal. Mostowski's undecidable statement expresses a fact concerning real numbers, more precisely, it states that a **CA**-set is not empty.

Another contribution of Mostowski to the discussed domain is his paper (1961). A notion of a free formula is introduced there. If $\varphi$ is a formula with one numerical free variable then $\varphi$ is said to be free for a system $S$ if for every natural number $n$, the formulas $\varphi(\overline{0}), \ldots, \varphi(\overline{n})$ are completely independent, i.e., every conjunction formed of some of those formulas and of the negations of the remaining ones is consistent with $S$ ($\overline{n}$ denotes here the $n$th numeral, i.e., $\overline{0}$ is the term 0 and $\overline{n+1}$ is the term $S\overline{n}$). It is proved that free formulas exist for certain systems $S$ and some of their extensions. Even a more general result is obtained: given a family of extensions of $S$ satisfying certain very general assumptions there exists a formula which is free for every extension of this family. It should be noted here that the method of the proof applies not only to systems based on usual finitary rules of inference but also to systems with the infinitary $\omega$-rule.

Mostowski wrote also two important popular papers devoted to the incompleteness results. One was published in Polish in 1946 (cf. Mostowski 1946), the other in English in 1952 (cf. Mostowski 1952). Both enjoyed considerable popularity. Its aim was to present "as clearly and as rigorously as possible the famous theory of undecidable sentences created by Kurt Gödel in 1931" (cf. Mostowski 1952, p. v). Though based on a classical material they brought some new ideas. In particular in the book (1952) the theory of $\mathcal{R}$-definability was developed. It presents a simultaneous generalization of the theory of definability and that of the general recursiveness of functions and relations. This theory proves to be a very convenient tool – one can express in it in a clear way the assumptions which are the common source of the various proofs of Gödel's incompleteness theorem.

To finish this section we want to mention a paper by Andrzej Ehrenfeucht (1961). The notion of a separable theory is studied there and some interrelations between separability and essential undecidability of theories are established. To be more precise, a theory $T$ is said to be separable if and only if there exists a recursive set $X$ of formulas such that (1) if $\varphi$ is a theorem of $T$ then $\varphi \in X$ and (2) if $\neg\varphi$ is a theorem of $T$ then $\varphi \notin X$. It can be easily seen that every inseparable theory is essentially undecidable but not vice versa. In fact an essentially undecidable but separable theory is constructed in the paper. It is also shown that an axiomatizable theory $T$ is inseparable if and only if for any recursive family $\{T_i\}$ of axiomatizable consistent extensions of $T$ there is a closed formula $\varphi$ undecidable in each $T_i$. This result establishes the relation between the theorem of Grzegorczyk, Mostowski and Ryll-Nardzewski (cf. Grzegorczyk-Mostowski-Ryll-Nardzewski 1958) stating the inseparability of Peano arithmetic and the result of Mostowski (cf. Mostowski 1961) which shows the existence of an undecidable sentence for any recursively enumerable family of extensions of Peano arithmetic.

## 6. Conclusions

In this way we came to the end of our survey of the results due to Polish logicians and devoted to the decidability theory. One can easily see that Polish mathematicians and logicians were from the very beginning in the main stream of investigations devoted to the *Entscheidungsproblem*. Moreover, they contributed to the development of this field in a significant way. Especially Tarski and his students (and later students of his students) were active here. They not only solved the problem of decidability in the case of many particular theories by establishing their decidability or undecidability, but they also developed general methods for such proofs, which became classical and standard.

Though decidability problems have also a philosophical character and research in this field can be described in terms of the study of the "cognitive power" pertaining to logical means of proof, it seems that such a philosophical motivation was not the main factor stimulating the activity of Polish logicians. Tarski and his students were advocates of the separation of logical research from philosophical studies. For them logic and foundations of mathematics constituted a separate field with its own problems and methods, a field developing independently of other branches of mathematics and philosophy.

# 16

# CONTRIBUTION OF POLISH LOGICIANS TO PREDICATE CALCULUS

The paper is devoted to the presentation of the main contributions of Polish logicians to the classical predicate calculus.[1] The following subjects will be discussed: works of M. Wajsberg on formulas satisfied in finite domains and on degrees of completeness of various calculi, the natural deduction of S. Jaśkowski, algebraic methods in proofs of the completeness theorem and Mostowski's work on generalized quantifiers. We shall not present studies on the theory of satisfaction and truth and on the general theory of deduction systems.

## 1. Wajsberg's Work on the Predicate Calculus

Logicians belonging to the Lvov-Warsaw school did not pay much attention in their investigations directly to the classical predicate calculus. An exception here was Mordechaj Wajsberg (1902– ?). He wrote two papers devoted to this calculus. They were written in 1932 and 1933 during his stay in Kowl (in Volhynia) where he was working as a teacher.

In the first of those papers, i.e., in the paper *Untersuchungen über den Funktionenkalkül für endliche Individuenbereiche* (1933) Wajsberg considered formulas of the restricted functional calculus satisfied in finite domains.[2] A formula is said to be $k$-true if and only if it is valid in a domain of individuals with exactly $k$ elements (this notion was introduced by P. Bernays). If the formula is

[1]The upper bound of the period covered in the paper is fixed as 1963, i.e., the year in which A. Mostowski's paper "Thirty Years of Foundational Studies", *Acta Philosophica Fennica* **17** (1965), 1–180 was published.

[2]The restricted functional calculus is simply the functional calculus of the first order, i.e., the first-order logic with function symbols and individual variables only. The terminology was introduced by D. Hilbert and W. Ackermann in *Grundzüge der theoretischen Logik* (1928). They considered also the extended functional calculus, i.e., the functional calculus of higher order in which one can use quantifiers not only over individual variables but also over functional variables.

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exactly $k$-true, it implies that it is not at the same time $k$ + 1-true. A formula is called a class formula if and only if it contains at most one free variable. The main theorem proved in (1933) states that any $k$-true formula results from some exactly $k$-true formula. It is a consequence of the following lemmas:

(1) There is a certain exactly $k$-true class formula from which each $k$-true formula derives.

(2) From any exactly $k$-true class formula each $k$-true class formula derives.

(3) From each exactly $k$-true formula a certain exactly $k$-true class formula derives.

In the paper (1933–1934) Wajsberg studied the degree of completeness of certain mathematical and logical theories, in particular of the restricted functional calculus, the extended functional calculus and the calculus of *Principia Mathematica* (with the axiom of infinity and the axiom of reducibility for all types) as presented by K. Gödel in (1931a). By a degree of completeness of a given theory Wajsberg understands the number of super-systems of this theory. It is shown that the degree of completeness of the three above mentioned theories is equal to the continuum. Wajsberg proves it by constructing countable sets of formulas with the property that no element of such a set is derivable from the set of the remaining elements.

## 2. Natural Deduction

A system of natural deduction of Stanisław Jaśkowski was an answer to the problem raised in 1926 by J. Łukasiewicz. Jaśkowski formulates this problem in the following way (cf. Jaśkowski 1934, p. 5):

> In 1926 Prof. J. Łukasiewicz called attention to the fact that mathematicians in their proofs do not appeal to the theses of the theory of deduction, but make use of other methods of reasoning. The chief means employed in their method is that of an arbitrary supposition. The problem raised by Mr. Łukasiewicz was to put those methods under the form of structural rules and to analyze their relation to the theory of deduction.

The first results towards the solution of this problem were obtained by S. Jaśkowski in 1926 at Łukasiewicz's seminar. They were presented at the First Polish Mathematical Congress in Lvov in 1927 (and mentioned in the proceedings of the Congress). The full solution was contained in the booklet *On the rules of suppositions in formal logic* published in 1934 as the first volume of the new series of books "Studia Logica" (Publications devoted to logic and its history. Editor Prof. Dr. J. Łukasiewicz).

Jaśkowski developed in this work a system of natural deduction. The main idea of it is formulated and explained by him in the following way (1934, p. 6)[3]:

> We intend to analyze a practical proof by making use of the method of suppositions. How can we convince ourselves of the truth of the proposition "$CpCCpqq$"? We shall do it as follows.
>
> *Suppose p*. This supposition being granted *suppose* $Cpq$. Thus we have assumed "$p$" and "if $p$, then $q$". Hence $q$ follows. We then observe that "$q$" is a consequence of the supposition "$Cpq$", and obtain as a deduction: "if $p$ implies $q$, then $q$", *i.e.* $CCpqq$. Thus having proposed "$p$", we have deduced this last proposition; from this fact, we can infer $CpCCpqq$.

To increase the clarity of such reasonings Jaśkowski introduces prefixes denoting which propositions are consequences of a given supposition. They contain numbers classifying the suppositions. The number 1 corresponds to the first supposition (in the example given above – to the supposition $p$). One writes the number before the supposition to which it corresponds and before all expressions which are assumed under the conviction that this supposition is true. If within the scope of validity of a given supposition an expression appears then its prefix must begin with the number of that supposition. The supposition itself obtains its own number and consequently its prefix is a sequence of numbers separated by dots. Hence in our example the prefix of the supposition $Cpq$ is 1.1. The word "suppose" is symbolized by the letter "$S$" written down immediately after the prefix. Thus the sketch of demonstration given in the above quotation can be written symbolically as follows[4]:

$1.S\,p$
$1.1.S\,Cpq$
$1.1.q$
$1.CCpqq$
$CpCCpqq$

We should stress that Jaśkowski (following Leśniewski, cf. Leśniewski 1929, p. 59) speaking about expressions, theses etc. treated always a given inscription as a material object. Hence two inscriptions having the same appearance but written down in different places are never taken as identical. They can only be said to be equiform with each other.

To formulate the structural rules of his system Jaśkowski introduced the notion of a domain. A domain of a supposition $\alpha$ is the class consisting of the supposition $\alpha$ and of all expressions which in other theses are preceded by initial part equiform to the prefix of $\alpha$. The absolute domain (shortly: domain) is

[3] Jaśkowski used in his work Łukasiewicz's bracket-free symbolism. Hence in particular he writes $Cpq$ for the formula $p \to q$, $CCpqq$ for the formula $(p \to q) \to q$ and $CpCCpqq$ for the formula $p \to [(p \to q) \to q]$.

[4] It should be noted that in the first presentation of the system in 1926 Jaśkowski used another symbolism.

the class of all theses belonging to the system. Note that at the beginning the absolute domain is empty. It grows when the system is being developed. This conception also reveals the influence of Leśniewski.

If two domains $D$ and $D'$ are given and $D$ is the domain of a supposition $\alpha$ and $D'$ either the absolute domain or the domain of a supposition $\beta$ whose prefix is equiform with an initial part of the prefix of $\alpha$, then we say that $D$ is a subdomain of $D'$. A proposition belonging to a domain is said to be valid in every subdomain of that domain. A given subdomain $D$ of a domain $D'$ is called an immediate subdomain if and only if $D$ is not the subdomain of any subdomain of $D'$.

The rules are now the following:

- **Rule I.** To every domain $D$, it is allowed to subjoin an expression composed successively:

    1. of a number not equiform with the initial number of any other element of the domain $D$,
    2. of a dot,
    3. of a symbol "$S$",
    4. of a proposition.

- **Rule II.** If in the domain $D$ of a supposition $\alpha$, a proposition $\beta$ is valid, it is allowed to subjoin a proposition of the form "$C\alpha\beta$" to the domain whereof $D$ is an immediate subdomain.
- **Rule III.** Given a domain $D$ in which two propositions are valid, one of them being $\alpha$ and the other being composed successively:

    1. of a symbol "$C$",
    2. of a proposition equiform with $\alpha$,
    3. of a proposition $\beta$,

  it is allowed to subjoin to the domain $D$ a proposition equiform with $\beta$.
- **Rule IV.** Given a subdomain $D$ of a proposition composed successively of a symbol "$N$" and of a proposition $\alpha$, if two propositions $\beta$ and $\gamma$ are valid in $D$ such that $\gamma$ is composed successively of a symbol "$N$" and of a proposition equiform with $\beta$, it is allowed to subjoin a proposition equiform with $\alpha$ to that domain whereof $D$ is an immediate subdomain.

The system defined in this way has no axioms. To explain how we proceed in this formalism we give some examples following Jaśkowski (1934, p. 12–13). The theses are marked by numbers. To the right of each thesis, the number of the rule used in obtaining it and the number of theses to which we appeal are given.

| | | |
|---|---|---|
| 1 | 1.*Sp* | I |
| 2 | 1.1.*SCpq* | I |
| 3 | 1.1.*q* | III 2, 1 |
| 4 | 1.*CCpqq* | II 2, 3 |
| 5 | *CpCCpqq* | II 1, 4 |
| 6 | 2.*SCNpNq* | I |
| 7 | 2.1.*Sq* | I |
| 8 | 2.1.1.*SNp* | I |
| 9 | 2.1.1.*Nq* | III 6, 8 |
| 10 | 2.1.*p* | IV 8, 7, 9 |
| 11 | 2.*Cqp* | II 7, 10 |
| 12 | *CCNpNqCqp* | II 6, 11 |
| 13 | 1.2.*Sq* | I |
| 14 | 1.*Cqp* | II 13, 1 |
| 15 | *CpCqp* | II 1, 14 |
| 16 | 1.3.*SNp* | I |
| 17 | 1.3.1.*SNq* | I |
| 18 | 1.3.*q* | IV 17, 1, 16 |
| 19 | 1.*CNpq* | II 16, 18 |
| 20 | *CpCNpq* | II 1, 19 |
| 21 | 3.*SCpq* | I |
| 22 | 3.1.*SCqr* | I |
| 23 | 3.1.1.*Sp* | I |
| 24 | 3.1.1.*q* | III 21, 23 |
| 25 | 3.1.1.*r* | III 22, 24 |
| 26 | 3.1.*Cpr* | II 23, 25 |
| 27 | 3.*CCqrCpr* | II 22, 26 |
| 28 | *CCpqCCqrCCpr* | II 21, 27 |
| 29 | 4.*SCpCqr* | I |
| 30 | 4.1.*SCpq* | I |
| 31 | 4.1.1.*Sp* | I |
| 32 | 4.1.1.*Cqr* | III 29, 31 |
| 33 | 4.1.1.*q* | III 30, 31 |
| 34 | 4.1.1.*r* | III, 32, 33 |
| 35 | 4.1.*Cpr* | II 31, 34 |
| 36 | 4.*CCpqCpr* | II 30, 35 |
| 37 | *CCpCqrCCpqCpr* | II 29, 36 |
| 38 | 5.*SCNpp* | I |
| 39 | 5.1.*SNp* | I |
| 40 | 5.1.*p* | III 38, 39 |
| 41 | 5.*p* | IV 39, 40, 39 |
| 42 | *CCNppp* | II 38, 41 |

| | | |
|---|---|---|
| 43 | $6.SCCpqp$ | I |
| 44 | $6.1.SNp$ | I |
| 45 | $6.1.1.Sp$ | I |
| 46 | $6.1.1.CNpq$ | III 20, 45 |
| 47 | $6.1.1.q$ | III 46, 44 |
| 48 | $6.1.Cpq$ | II 45, 47 |
| 49 | $6.1.p$ | III 43, 48 |
| 50 | $6.p$ | IV 44, 49, 44 |
| 51 | $CCCpqpp$ | II 43, 50 |

The system of natural deduction for the propositional calculus can be extended to the predicate calculus. One does it by extending the formalism described above. Thus in particular in any domain $D$, any term whose prefix is equiform to an initial part of the prefix of the thesis containing the supposition or the term belonging to $D$ is called valid. A proposition $\alpha$ is said to be significant in the domain $D$ if every free variable of $\alpha$ is equiform with some term valid in $D$. The symbol "$T$" is analogous to the symbol of supposition "$S$". The rules are now the following:

- **Rule Ia.** Given a domain $D$, it is allowed to subjoin to it any expression composed successively:
    1. of a number not equiform with the initial number of any element of the domain $D$,
    2. of a dot,
    3. of the symbol "$S$",
    4. of a proposition significant in the domain $D$.

Rules IIa, IIIa, IVa are the same as Rules II, III and IV, resp., formulated above but they are applied now to the language of the predicate calculus.

- **Rule Va.** It is allowed, to any domain $D$ in which
    1. a term $\zeta$ and
    2. a proposition composed of a quantifier "$\Pi$", of a variable $\eta$ and of a proposition $\alpha$

  are valid, to subjoin a proposition which differs, as to its form, from $\alpha$ only in this respect that all variables bounded in $\eta$ are replaced by symbols equiform with $\zeta$ no one of which is a bounded variable (this rule corresponds to the rule of omitting of the universal quantifier),
- **Rule VIa.** If in the domain $D$ of a term $\zeta$ a proposition $\alpha$ is valid, it is allowed to subjoin a proposition of the form "$\Pi\zeta\alpha$" to that domain whereof the domain $D$ is an immediate subdomain (this rule correspond to the rule of generalization).

- **Rule VIIa.** Given a domain $D$, it is allowed to subjoin to it any expression composed successively:
  1. of a number not equiform with the initial number of any element of the domain,
  2. of a dot,
  3. of the symbol "$T$" and
  4. of a term not equiform with any term valid in the domain $D$.

Again the system has no axioms. We provide an example of a deduction in it (to the right of each thesis the number of the rule used in obtaining it and the numbers of theses to which we appeal are given) (cf. Jaśkowski 1934, p. 30–31):

| | | |
|---|---|---|
| 1 | $1.S\Pi x\Pi y\varphi xy$ | Ia |
| 2 | $1.1.Tz$ | VIIa |
| 3 | $1.1.\Pi y\varphi zy$ | Va 1, 2 |
| 4 | $1.1.\varphi zz$ | Va 2, 3 |
| 5 | $1.\Pi z\varphi zz$ | VIa 2, 4 |
| 6 | $C\Pi x\Pi y\varphi xy\Pi z\varphi zz$ | IIa 1, 5 |
| 7 | $1.1.1.Tv$ | VIIa |
| 8 | $1.1.1.\varphi zv$ | Va 3, 7 |
| 9 | $1.1.\Pi v\varphi zv$ | VIa 7, 8 |
| 10 | $1.\Pi z\Pi v\varphi zv$ | VIa 2, 9 |
| 11 | $1.2.Tx$ | VIIa |
| 12 | $1.2.1.Ty$ | VIIa |
| 13 | $1.2.1.\Pi v\varphi yv$ | Va 10, 12 |
| 14 | $1.2.1.\varphi yx$ | Va 13, 11 |
| 15 | $1.2.\Pi y\varphi yx$ | VIa 12, 14 |
| 16 | $1.\Pi x\Pi y\varphi yx$ | VIa 11, 15 |
| 17 | $C\Pi x\Pi y\varphi xy\Pi x\Pi y\varphi yx$ | II 1, 16 |
| 18 | $2.S\Pi xC\varphi x\psi x$ | Ia |
| 19 | $2.1.S\Pi x\varphi x$ | Ia |
| 20 | $2.1.1.Tx$ | VIIa |
| 21 | $2.1.1.C\varphi x\psi x$ | Va 18, 20 |
| 22 | $2.1.1.\varphi x$ | Va 19, 20 |
| 23 | $2.1.1.\psi x$ | III 21, 22 |
| 24 | $2.1.\Pi x\psi x$ | VIa 20, 23 |
| 25 | $2.C\Pi x\varphi x\Pi x\psi x$ | II 19, 24 |
| 26 | $C\Pi xC\varphi x\psi xC\Pi x\varphi x\Pi x\psi x$ | II 18, 25 |
| 27 | $3.Sp$ | Ia |
| 28 | $3.1.Tx$ | VIIa |
| 29 | $3.\Pi xp$ | VIa 28, 27 |
| 30 | $Cp\Pi xp$ | IIa 27, 29 |

The Jaśkowski type systems can be developed for different logical calculi, e.g., for systems with additional connectives, for positive logic, for intuitionistic logic, etc.

It can be proved that the formalization given by the system of natural deduction is equivalent to the classical one, i.e., one can prove that a formula is a tautology of the classical axiomatic propositional (predicate) logic if and only if it is a thesis of the system of natural deduction for the propositional (predicate) logic.

The system of natural deduction of Jaśkowski belongs certainly to the most important achievements of Polish logic. It is a pity that it is – in a sense – forgotten and not used. Instead of it another formalism is applied today. We mean the formalism of natural deduction elaborated independently by Gerhard Gentzen in (1935). Nevertheless the natural deduction of Jaśkowski was used in the Warsaw School by A. Tarski and S. Leśniewski in their works written in the twenties.

## 3. Algebraic Methods in the Classical Predicate Calculus

The algebraic modeling for the first-order calculi was introduced by Andrzej Mostowski in (1948b). The main purpose of this paper was to outline a general method which permits one to prove the intuitionistic non-deducibility of many formulas. The method consists in using the connections between intuitionistic logic and the so-called Brouwerian lattices (introduced and examined by J.C.C. McKinsey and A. Tarski in 1946).

Mostowski's work initiated the algebraic approach to classical as well as to non-classical logic. Since our paper is devoted to the classical predicate calculus we shall discuss only the former one.

One should mention here first of all papers by Helena Rasiowa and Roman Sikorski. In the paper (1950) they gave a new proof of the completeness theorem of Gödel stating that if a formula $\varphi$ of the first-order predicate calculus is valid in the domain of positive integers then it is provable.

As it was usual at that time, Rasiowa and Sikorski formulated their theorems for the functional calculus in which we have individual variables, functional variables and constants (connectives, quantifiers and technical signs). Today one uses rather the full predicate calculus with individual variables, predicates, function symbols and constants. The results formulated for one case can be of course reformulated for the other (and the choice of the type of calculus is the matter of convenience and taste only).

Three ideas were used in the Rasiowa-Sikorski's proof of the completeness theorem: Mostowski's algebraic interpretation of a formula $\varphi$ as a functional the values of which belong to a Boolean algebra, Lindenbaum's construction of a

Boolean algebra from formulas of the functional calculus and a theorem on the existence of prime ideals in Boolean algebras.

Let $I$ be the set of positive integers and $B_0$ the two-element Boolean algebra (with elements 0 and 1). Let $\varphi = \varphi(x_{i_1}, \ldots, x_{i_n}, F^{k_1}_{j_1}, F^{k_m}_{j_m})$ be a formula of the first-order functional calculus. It can be interpreted as an $(I, B_0)$ functional, i.e., as a function whose values belong to $B_0$ and which has $n$ arguments running over $I$ and $m$ arguments running over the set $\mathcal{F}$ (where $\mathcal{F}$ is the set of all functions whose arguments run over $I$ and whose values belong to $B_0$). The interpretation is defined as follows: (1) the individual variables $x_{i_p}$ are interpreted as variables running over $I$, (2) the functional variables $F^{k_p}_{j_p}$ are interpreted as variables running over $\mathcal{F}^{k_p}$ (= the set of $k$-argument functions from $\mathcal{F}$), (3) the operations of disjunction, negation and existential quantifier are interpreted as the Boolean operations in the algebra $B_0$. The functional obtained in this way will be denoted as $\Phi_\varphi$. One can show that:

(1) For every formula $\varphi$ from the functional calculus the $(I, B_0)$ functional $\Phi_\varphi$ assumes the value 1 (0) if and only if $\varphi$ $(\neg\varphi)$ is satisfiable.

(2) A formula $\varphi$ is valid in the set $I$ of all positive integers if and only if the $(I, B_0)$ functional $\Phi_\varphi$ is identically equal $1 \in B_0$.

The second idea used in the proof is the Lindenbaum's construction of a Boolean algebra from formulas of the functional calculus. It consists of equivalence classes of formulas with respect to the following relation: $\varphi \sim \psi$ if and only if $\varphi \longrightarrow \psi$ and $\psi \longrightarrow \varphi$ are provable (in the functional calculus).

The third idea is the existence of prime ideals in Boolean algebras preserving appropriate sums and containing appropriate elements. The proof of this is topological and uses the well-known category method.

Applying those three ideas Rasiowa and Sikorski proved the following theorem: if a formula $\varphi$ is not provable then the $(I, B_0)$ functional $\Phi_\varphi$ assumes the value 0 (the zero element of $B_0$). They proceeded as follows: suppose that $\varphi$ is not provable. Let $p^*$ be a prime ideal of the Lindenbaum's algebra $B^*$ preserving appropriate sums and such that the equivalence class $E(\varphi)$ of $\varphi$ belongs to $p^*$. The existence of such an ideal follows from the general theorem on the existence and from the fact that the unit of $B^*$ is the class of all provable formulas and that the set of appropriate sums is countable. Then $B_0 = B^*/p^*$ is a two-element Boolean algebra and one can find arguments for which the functional $\Phi_\varphi$ assumes the value 0.

The algebraical method was also used by Rasiowa and Sikorski to give a simpler proof of the Skolem-Löwenheim theorem (cf. Rasiowa-Sikorski 1951). It can also be generalized to the case of other functional calculi, e.g., to the functional calculi of Heyting and Lewis (cf. Rasiowa 1951).

The general applicability of the algebraic treatment of the notion of satisfiability was shown in the paper by Rasiowa and Sikorski (1953). They studied there systematically the notion of satisfiability and validity in the general case, i.e., for a functional calculus $\mathcal{S}^*$ which is not exactly specified (only the fulfilment of some general assumptions is required).

## 4. Generalized Quantifiers

The language of the first-order predicate calculus is a useful tool for mathematics. Many of mathematical notions and objects can be expressed in this language – but not all of them. In this situation one of the solutions can be provided by higher order logics. Unfortunately this approach has some serious defects. Those logics are extremely difficult to investigate. This is because one uses in them the notion of set and membership and in this way gets involved in all the difficulties connected with set theory. Hence higher order logics cannot be complete in any sense.

Another solution is to strengthen the first-order logic. A. Mostowski proposed in (1957b) to do it by adding generalized quantifiers.

Let $T$ be a function defined on pairs of cardinal numbers and with values in the set $\{0, 1\}$. To every such function Mostowski adjoins a quantifier $Q_T$ defined semantically in the following way: if $\varphi$ is a formula (for simplicity assume that it has only one free variable) and $\mathcal{A}$ is a structure then the formula $Q_T x\varphi$ is satisfied in $\mathcal{A}$ if and only if $T(\mathrm{card}(\varphi^{\mathcal{A}}), \mathrm{card}(\neg\varphi^{\mathcal{A}})) = 1$, where $\varphi^{\mathcal{A}} = \{a \in |\mathcal{A}|\colon \mathcal{A} \models \varphi[a]\}$ and $\mathrm{card}(A)$ denotes the cardinality of the set $A$. The definition is a generalization of the classical notion of quantifier. If $T$ is a function such that $T(\kappa, \lambda) = 1$ if and only if $\kappa \geqslant 1$ then $Q_T$ is the existential quantifier. Similarly for the function $T$ such that $T(\kappa, \lambda) = 1$ if and only if $\lambda = 0$ describes the universal quantifier. New quantifiers which can be obtained in the indicated general way are for example the quantifiers $Q_\alpha$ and the Chang quantifier $Q_c$. They are defined as follows. If $\alpha$ is an ordinal number then the quantifier $Q_\alpha$ is described by the function $T_\alpha$ such that $T_\alpha(\kappa, \lambda) = 1$ if and only if $\kappa \geqslant \aleph_\alpha$. The Chang quantifier is determined by the function $T_c$ satisfying the condition $T_c(\kappa, \lambda) = 1 \equiv \kappa + \lambda = \kappa$.

Let $L_Q$ denote the first-order language enriched by the quantifier $Q$. The main problem which should be solved is the problem of axiomatizability of the new quantifier. Mostowski proved in (1957b) that the logic $L_{Q_0}$ cannot be axiomatized by a recursively enumerable set of axioms. The proof is based on the idea that in the language $L_{Q_0}$ one can give a categorical description of the standard model of arithmetic. The theorem on non-axiomatizability remains true even for the set of sentences true in all countable structures.

Mostowski proved also in (1957b) that if the first-order logic is enriched by a generalized quantifier which is not definable by means of classical quantifiers then the Löwenheim-Skolem theorem no longer holds. This result was strengthened ten years later by Lindström who gave in (1969) an interesting characterization of the first-order logic.

Despite of the above negative result Mostowski showed in (1957b) that every structure contains – elementarily in the sense of $L_{Q_0}$ – a countable structure. He also proved that for countable structures there exists only one non-trivial generalized quantifier.

Problems raised by Mostowski in (1957b) were studied by numerous logicians: G. Fuhrken and A. Slomson investigated the compactness of languages with additional quantifiers, R.L. Vaught showed that $L_{Q_1}$ can be axiomatized, H.J. Keisler gave a finite system of axioms for $L_{Q_1}$ and proved the axiomatizability of $L_{Q_c}$.

Mostowski returned to the study of languages with generalized quantifiers in the paper (1968). He considered there the Craig interpolation lemma and the Beth definability theorem for certain extensions of the first-order logic and proved that: (1) for logics with additional quantifiers the interpolation lemma is false, (2) for the logic $L_{Q_0}$ the definability lemma is false.

Discussing Mostowski's researches on the generalizations of the first-order logic one should stress a fact which is very important from the methodological point of view. In the comment on the theorem on non-axiomatizability he says in (1957b, p. 12):

> In spite of this negative result we believe that some at least of the generalized quantifiers deserve a closer study and some deserve even to be included into systematic expositions of symbolic logic. This belief is based on the conviction that the construction of formal calculi is not the unique and even not the most important goal of symbolic logic.

We must remember that in those days the tradition of the Hilbert school was still very strong. Hence Mostowski's opinion could give rise to a serious controversy. His attitude which allows non-effective and infinitistic methods (demonstrated not only in the case of generalized quantifiers but also in introducing the operation of $\beta$-consequence in the second order arithmetic) opened ways for new fields of study in the foundations of mathematics.

## 17

# THE ENGLISH ALGEBRA OF LOGIC IN THE 19TH CENTURY[1]

The aim of this essay is to study works and achievements of English logicians of the 19th century. We shall consider the role they played in the development of mathematical logic, in particular their contribution to the formalization of logic and to the mechanization of reasoning. We shall present and discuss first of all works of A. De Morgan, G. Boole, W. S. Jevons and J. Venn indicating their meaning and significance for the development of mathematical logic.

Works of English logicians of the 19th century grew out of earlier ideas and attempts of G. W. Leibniz, G. Ploucquet, J. H. Lambert, L. Euler. The idea of the mathematization of logic and the development of the formal algebra in the 19th century were sources of the algebra of logic established by De Morgan, Boole and Jevons. It was in fact the beginning of the mathematical logic. The old idea of a logical calculus which would enable the analysis of logical reasoning with the help of a procedure similar to the procedure of solving equations in algebra was realized.

### 1. De Morgan's Syllogistic and the Theory of Relations

We shall begin the discussion of the development of logic in England in 19th century by studying the idea of quantification of the predicate.

In traditional logic (since Aristotle) the most important role was played by syllogisms. Aristotle defined them as formal arguments in which the conclusion follows necessarily from the premises. His analysis centered on a very specific type of argument. He considered namely statements of the form: all $S$ is $P$ (universal affirmative), no $S$ is $P$ (universal negative), some $S$ is $P$ (particular af-

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firmative), and some $S$ is not $P$ (particular negative), abbreviated later, resp., as follows: $SaP$, $SiP$, $SeP$, $SoP$ and called the categorical sentences. Aristotle observed that one can build valid schemas of inference consisting of two premises and a conclusion being categorical sentences – they are called categorical syllogisms. If we assume that every term in a syllogism stands for a nonempty class then we get that 24 of 256 possible combinations are valid inferences.

Aristotle and his medieval followers greatly exaggerated the importance of the syllogism. Nevertheless syllogism formed the main part of logic until the beginning of mathematical logic in the 19th century. Various attempts to reshape and to enlarge the Aristotelian syllogism were undertaken. Let us mention here attempts of F. Bacon, Ch. von Sigwart and W. Schuppe. The most famous of those attempts was the "quantification of the predicate" by the Scottish philosopher Sir William Hamilton (1788–1856) presented in his book *Lectures on Metaphysics and Logic* published in 1860. He noticed that the predicate term in each of Aristotle's four basic assertions $SaP$, $SiP$, $SeP$, $SoP$ is ambiguous in the sense that it does not tell us whether we are concerned with all or part of the predicate. Hence one should increase the precision of those four statements by quantifying their predicates. In this way we get eight assertions instead of Aristotle's four, namely:

$$\begin{aligned}
&\text{all } S \text{ is all } P\,,\\
&\text{all } S \text{ is some } P\,,\\
&\text{no } S \text{ is all } P\,,\\
&\text{no } S \text{ is some } P\,,\\
&\text{some } S \text{ is all } P\,,\\
&\text{some } S \text{ is some } P\,,\\
&\text{some } S \text{ is not all } P\,,\\
&\text{some } S \text{ is not some } P\,.
\end{aligned}$$

Using those eight basic propositions we can combine them to form 512 possible moods of which 108 prove to be valid. The usage of statements with quantified predicates allows us higher precision than it was possible before. For example, the old logic would treat "All men are mortal" and "All men are featherless bipeds" as identical in form; whereas in the new system we see at once that the first statement is an example of "All $S$ is some $P$" and the second is an example of "All $S$ is all $P$". But there arose some problems. It was difficult to express those new statements with quantified predicates in a common speech. Without developing a really complete and precise system of notation one finds oneself forced to apply words in a clumsy and barbarous way. Hamilton was aware of it and attempted to remedy the obscurity of phrasing by devising a curious system of notation. Though it was really curious and rather useless in practice it was important for two reasons: it had the superficial appearance of a diagram and it led Hamilton to the idea that by transforming the phrasing of any valid syllogism with quantified predicates it may be expressed in statements of equality. The latter suggested that logical statements might be reduced to something analogous to algebraic equations and so gave encouragement to those who were seeking a suitable algebraic notation. Some logicians are of the opinion that this was Hamilton's only significant contribution to logic (cf. Gardner 1958).

The idea of quantification of the predicate in the syllogism can be found also in papers of another English logician Augustus De Morgan (1806–1871). He was a mathematician (contrary to W. Hamilton) and worked not only in logic but also in algebra and analysis. We shall not discuss his mathematical works here but concentrate on his logical achievements. Nevertheless we want to mention that it was just De Morgan who introduced the notion of "mathematical induction" which was popularized later thanks to the book on algebra written by I. Todhunter (1820–1884).

A. De Morgan's earlier logical works (i.e., works written before 1859) were devoted to the study of the syllogism. He introduced independently a system more elaborated than Hamilton's one. Hamilton accused him of plagiarism and for many years the two men argued with each other in books and magazine articles. It was, as M. Gardner (1958) writes "perhaps the bitterest and funniest debate about formal logic since the time of the schoolmen, though most of the humor as well as insight was on the side of De Morgan" (p. 134). This debate had also serious consequences. Namely it caused G. Boole's renewed interest in logic which led him to write the book *Mathematical Analysis of Logic* published in 1847 (we shall discuss it in the next section).

Trying to reshape and to enlarge the Aristotelian syllogism (cf. the book *Formal Logic, or the Calculus of Inference Necessary and Probable*, 1847) De Morgan observed that in almost all languages there are so called positive and negative terms. Even if in a language there are no special words indicating this dichotomy, nevertheless every notion divides the universe of discourse into two parts: elements having properties indicated by the given term and those which do not have those properties. Hence if $X$ denotes a certain class of objects then all elements of the universe which are not $X$ can be described as not-$X$. The latter is denoted by De Morgan by $x$. In this way the difference between positive and negative terms disappears and they possess equal rights. This enabled De Morgan to consider – instead of two terms of the traditional syllogistic $X$, $Y$ – four pairs of terms: $X, Y$; $x, y$; $X, y$; $x, Y$ which give 16 logical combinations, 8 of which are different. He introduced various types of notation for them. The first two consisted of letters and resembled the traditional notation: $a, i, e, o$; the latter two consisted of systems of parentheses. We shall present them in the following table (the notation in the last column was introduced in De Morgan 1856, p. 91):

| | | | | |
|---|---|---|---|---|
| all $X$ is $Y$ | $A$ | $A_1$ | $X)Y$ | $X))Y$ |
| no $X$ is $Y$ | $E$ | $E_1$ | $X.Y$ or $X)y$ | $X).(Y$ or $X))y$ |
| some $X$ is $Y$ | $I$ | $I_1$ | $XY$ | $X()Y$ |
| some $X$ is not $Y$ | $O$ | $O_1$ | $X : Y$ or $Xy$ | $X(.(Y$ or $X()y$ |
| all $x$ is $y$ | $a$ | $A'$ | $x)y$ or $Y)X$ | $x))y$ or $X((Y$ |
| no $x$ is $y$ | $e$ | $E'$ | $x)Y$ or $x.y$ | $x))Y$ or $x(.)Y$ |
| some $x$ is $y$ | $i$ | $I'$ | $xy$ | $x()y$ or $X)(Y$ |
| some $x$ is not $y$ | $o$ | $O'$ | $xY$ or $x : y$ or $Y : X$ | $x()Y$ or $x).)Y$ |

We see that De Morgan's symbols introduced to express the old and new types of syllogism were not algebraic. But he found that his symbols can be manipulated in a way that resembled the familiar method of manipulation of algebraic formulae (cf. De Morgan's letter to Boole from 16 October 1861 – Letter 70 in Smith 1982, pp. 87–91).

The above statements were called by De Morgan simple. He established the following connections between them:

$$\begin{aligned}
&X)Y = X.y = y)x\,;\\
&X.Y = X)y = Y)x\,;\\
&XY = X : y = Y : x\,;\\
&X : Y = Xy = y : x\,;\\
&x)y = x.Y = Y)Y\,;\\
&x.y = x)Y = y)X\,;\\
&xy = x : Y = y : X\,;\\
&x : y = xY = Y : x\,.
\end{aligned}$$

The sign = was used by De Morgan in two meanings. Here it means "equivalent" but in other contexts it was used also as a symbol for "implies".

Denoting by + the conjunction De Morgan established the following complex statements:

$$\begin{aligned}
&D = A_1 + A' = X)Y + x)y\,;\\
&D_1 = A_1 + O' = X)Y + x : y\,;\\
&D = A' + O_1 = xy + X : Y\,;\\
&P = I_1 + I' + O_1 + O'\,;\\
&C = E_1 + E' = X.Y + x.y\,;\\
&C_1 = E_1 + I' = X.Y + xy\,;\\
&C' = E' + I_1 = x.y + XY.
\end{aligned}$$

Using the contemporary set-theoretical symbolism we see that $D$ is simply $X = Y$, $D_1$ is $X \subset Y$, $D'$ is $X \supset Y$, $C$ is $X = y$, $C_1$ is $X \subset y$, $C'$ is $X \supset y$, $P$ means that neither $X \cap Y$, nor $X \cap y$, nor $x \cap Y$ nor $x \cap y$ is empty.

Having introduced those fundamental relations De Morgan developed his theory of syllogism. He considered not only syllogism with simple premises but also with complex ones (which he called complex syllogism).

We shall not present here all details of De Morgan's syllogistic. Let us note only that he used simple terms $X$, $Y$ etc. as syllogistic terms as well as complex ones: $PQ$ (being $P$ and $Q$; $P \cap Q$ in set-theoretical terms), $P * Q$ (being $P$ or $Q$ or both; $P \cup Q$), $U$ (the universe), $u$ (empty set). He stated the following properties:

$$XU = X\,; \quad Xu = u\,; \quad X * U = U\,; \quad X * u = X\,.$$

Having introduced operations which we call today the negation of a product and sum he established the following connections (called today De Morgan's laws):

negation of $PQ$ is $p * q$,
negation of $P * Q$ is $pq$.

He noted also the law of distributivity:

$$(P * Q)(R * S) = PR * PS * QR * QS\,.$$

We see that developing the theory of syllogism De Morgan came to the idea of what we call today Boolean algebra. He did it independently of G. Boole (in the next section we shall discuss in details connections and interdependencies of their works and compare their achievements).

De Morgan not only extended the traditional syllogistic but introduced also new types of syllogisms. In *Budget of Paradoxes* (1872) he summarized his work under six heads, each propounding a new type of syllogism: relative, undecided, exemplar, numerical, onzymatic and transposed (cf. also De Morgan 1860). Since those considerations did not lead to new developments in mathematical logic we shall not go into details here.

De Morgan's considerations of syllogism were not very original. Much more original were his later works – and it is perhaps his most lasting contribution to logic. In the paper "On the Syllogism no IV, and on Logic in General" (De Morgan 1861) from 1859 (published in 1864) he moved beyond the syllogism to investigate the theory of relations. Although he was not the first to study relations in logic, he was probably the first who gave this subject a concentrated attention. He may be considered as a real founder of the modern logic of relations. His ideas and conceptions were later on developed by various logicians, first of all by Ch. S. Peirce, E. Schröder, G. Peano, G. Cantor, G. Frege and B. Russell. Ch.S. Peirce wrote that De Morgan "was one of the best logicians of all time and undoubtedly a father of the logic of relatives" (cf. Ch.S. Peirce 1933–1934, vol. 3, p. 237).

In "On the Syllogism no IV" (1864b) De Morgan noted that the doctrine of syllogism, which he had discussed in his *Formal Logic* of 1847 (cf. De Morgan 1847) and in earlier papers was only a special case in the theory of the composition of relations. Hence he went to a more general treatment of the subject. He stated that the canons of syllogistic reasoning were in effect a statement of the symmetrical (De Morgan called it convertible) and transitive character of the relation of identity. He suggested symbols for the converse and the contradictory (he said "contrary") of a relative and for three different ways in which a pair of relatives may be combined (by "relative" he meant what some logicians had called a relative term, i.e., a term which applies to something only in respect of its being related to something else). Those ways can be expressed by the following phrases: (i) $x$ is an $l$ of an $m$ of $y$ (e.g., "John is a lover of a master of Peter"), (ii) $x$ is an $l$ of every $m$ of $y$, (iii) $x$ is an $l$ of none but an $m$ of $y$. De Morgan showed that the converse of the contradictory and the contradictory of the converse are identical. He set out in a table the converse, the contradictory and the converse of the contradictory of each of the three combinations and proved that the converse or the contradictory or the converse of the contradictory of each such combination is itself a combination of one of the kinds discussed.

Papers of De Morgan, though containing new and interesting ideas were not easy to read. They were full of ambiguities and technical details which made them difficult to study. Nevertheless they contributed in a significant way to the development of mathematical logic. We shall come back to De Morgan and his works in the next section comparing his achievements with those of Boole.

## 2. Boole and His Algebra of Logic

One of the most important figures in the history of mathematical logic (not only in England) was George Boole (1815–1864). He was interested in logic already in his teens, working as un usher in a private school at Lincoln and educating himself by extensive reading. From that time came his idea that algebraic formulae might be used to express logical relations. His renewed interest in logic was caused by the publication (in periodicals) of letters on the controversy between Sir William Hamilton and Augustus De Morgan on the priority in adoption of the doctrine of the quantification of predicates (cf. the previous section). Boole and De Morgan were in correspondence over a long period (1842–1864). They exchanged scientific ideas and discussed various problems of logic and mathematics.

The main logical works of G. Boole are two books: *The Mathematical Analysis of Logic, Being an Essay Toward a Calculus of Deductive Reasoning* (1847) and *An Investigation of the Laws of Thoughts, on which are Founded the Mathematical Theories of Logic and Probabilities* (1854).

While working on the pamphlet *Mathematical Analysis of Logic* G. Boole knew already A. De Morgan and they were exchanging letters. De Morgan was working then on his *Formal Logic*. He was meticulous in ensuring that neither could be placed in a position in which he could be accused of plagiarism. He wrote to Boole in a letter of 31st May 1847:

> [...] I would much rather not see your investigations till my own are quite finished; which they are not yet for I get something new every day. When my sheets are printed, I will ask for your publication: till then please not to sent it. I expect that we are more likely to have something in common than Sir W.H. (= William Hamilton) and myself.
>
> I should have sent my paper on syllogism [...] to you by this post: but I remembered that you might have the same fancy as myself – to complete your own first. Therefore when you choose to have it, let me know (cf. Smith 1982, Letter 11, p. 22).

Hence we may conclude that they wrote their works completely independently. There is a story that their books reached the shop on the same day (in November 1847).

The second book of G. Boole *An Investigation of the Laws of Thought* was not very original. It was the result of Boole's studies of works of philosophers on the foundations of logic. The chief novelty of the book was the application of his ideas to the calculus of probabilities. There was no important change on the formal side in comparison with *Mathematical Analysis of Logic*.

We should mention here also his paper "On the Calculus of Logic" (1848) which contained a short account of his ideas from the pamphlet from 1847. It may be supposed that it was more widely read among mathematicians than *Mathematical Analysis of Logic*.

Boole was not satisfied with the exposition of his ideas in his books. He was working towards the end of his life on the new edition of the *Laws of Thought* preparing various improvements. From the drafts published in (Boole 1952) it follows that what he had in mind was a development of his epistemological views rather than any alteration of the formal side of his work. He mentions in particular a distinction between the logic of class (i.e., his calculus of logic) and a higher, more comprehensive, logic that cannot be reduced to a calculus but may be said to be "the Philosophy of all thought which is expressible in signs, whatever the object of that thought" (cf. Boole 1952, p. 14).

Coming now to the detailed analysis of Boole's achievements we must start from the observation that what stimulated his interest in mathematical logic was not only the dispute between De Morgan and Hamilton but also the discussion of the nature of algebra shortly before he wrote his papers (let us mention here the papers of Peacock, Sir William Rowan Hamilton, De Morgan or Gregory – a personal friend of Boole and the editor of "Cambridge Mathematical Journal"). Boole's aesthetic interest in mathematics led him to value very highly all attempts to achieve abstract generality.

Boole started from the following ideas (which were at least implicitly in papers of his contemporaries): (i) there could be an algebra of entities which are not numbers in any ordinary sense, (ii) the laws which hold for types of numbers up and including complex numbers need not all be retained in an algebraic system not applicable to such numbers. He saw that an algebra could be developed as an abstract calculus capable of various interpretations. The view of logic he had can be seen from the opening section of his *Mathematical Analysis of Logic*:

> They who are acquainted with the present state of the theory of Symbolical Algebra, are aware that the validity of the processes of analysis does not depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination. Every system of interpretation which does not affect the truth of the relations supposed, is equally admissible, and it is thus that the same processes may, under one scheme of interpretation, represent the solution of a question on the properties of numbers, under another, that of a geometrical problem, and under a third, that of a problem of dynamics or optics [...] We might justly assign it as the definitive character of a true Calculus, that it is a method resting upon the employment of Symbols, whose laws of combination are known and general, and whose results admit of a consistent interpretation. That to the existing forms of Analysis a quantitative interpretation is assigned, is the result of circumstances by which those forms were determined, and is not to be construed into a universal condition of Analysis. It is upon the foundation of this general principle, that I purpose to establish the Calculus of Logic, and that I claim for it a place among the acknowledged forms of Mathematical Analysis, regardless that in its objects and in its instruments it must at present stand alone. (p. 9)

What is really new in Boole's works is not the idea of an unquantitative calculus – it was already by Leibniz and Lambert – but a clear description of the essence of the formalism in which the validity of a statement "does not depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination" (Boole 1847, p. 11). Boole indicates here that a given formal language may be interpreted in various ways. Hence he sees logic not as an analysis of abstracts from real thoughts but rather as a formal construction for which one builds afterword an interpretation. This is really quite new in comparison with the whole tradition including Leibniz. Boole summerizes his position in the last paragraph of the paper "On the Calculus of Logic" (Boole 1848) as follows:

> The view which these enquires present of the nature of language is a very interesting one. They exhibit it not as a mere collection of signs, but as a system of expressions, the elements of which are subject to the laws of the thought which they represent. That these laws areas rigorously mathematical as the laws which govern the purely quantitative conceptions of space and time, of number and magnitude, is a conclusion which I do not hesitate to submit to the most exact scrutiny. (p. 185)

One can say that the relation of Boole to Leibniz is the same as was the relation of Aristotle to Plato. In fact, we find by Boole, as it was by Aristotle, not only the ideas, but also a concrete implementation of them, a concrete system. We are going to describe it now.

First of all we must mention that what was built by Boole was not a system in our nowadays meaning – he did not distinguish between properties of operations which are assumed and properties which can be proved or derived from the assumptions.

Symbols $x$, $y$, $z$ etc. denoted classes in Boole's papers. He did not distinguish very sharply between class symbols and adjectives. Sometimes he called the letters $x$, $y$, $z$ etc. elective symbols thinking of them as symbols which elect (i.e., select) certain things for attention. The symbol = between two class symbols indicated that the classes concerned have the same members. He introduced three operations on classes: (i) intersection of two classes denoted by $xy$ (i.e., the class consisting of all the things which belong to both those classes), (ii) the exclusive union of two classes, i.e., if $x$ and $y$ are two mutually exclusive classes then $x + y$ denotes the class of things which belong either to the class denoted by $x$ or to the class denoted by $y$, (iii) subtraction of two classes, i.e., if every element of the class $y$ is an element of the class $x$ then $x - y$ denoted the class of those elements of $x$ which are not elements of $y$. He introduced also special symbols for two classes which form, so to say, limiting cases among all distinguishable classes, namely the universe class, or the class of which everything is a member (denoted by 1) and the null class, or the class of which nothing is a member (denoted by 0). The introduction of those classes involves an interesting novelty with respect to the tradition coming from Aristotle who confined his attention to general terms which were neither universal in the sense of applying to everything nor yet null in the sense of applying to nothing. Boole denoted by 1 what De Morgan called the universe of discourse, i.e., not the totality of all conceivable objects of any kind whatsoever, but rather the whole of some definite category of things which are under discussion.

Having introduced the universe class Boole wrote $1 - x$ for the complement of the class $x$ and abbreviated it as $\overline{x}$.

The operation of intersection of classes resembles the operation of multiplication of numbers. But there is an important peculiarity observed by Boole. Namely if $x$ denotes a class then $xx = x$ and in general $x^n = x$. Boole says in *Mathematical Analysis of Logic* that it is the distinguishing feature of his calculus. The analogy between intersection and multiplication suggests also the idea of introducing to the calculus for classes on operation resembling division of two numbers. Boole writes that this analogue may be abstraction. Let $x, y, z$ denote classes related in the way indicated by the equation $x = yz$. We can write $z = x/y$ expressing the fact that $z$ denotes a class we reach by abstracting from the membership of the class $x$ the restriction of being included in the class $y$. But this convention has two limits. First the expression $x/y$ can have no meaning at all in the calculus of classes if the class $x$ is not a part of the class $y$ and secondly, the expression $x/y$ is generally indeterminate in the calculus of classes, i.e., there may be many different classes whose intersections with the class $y$ are all coextensive with the class $x$.

The introduced system of notation suffices to express the $A$, $E$, $I$ and $O$ propositions of traditional logic (provided that $A$ and $E$ propositions are taken without existential import). If the symbol $x$ denotes the class of $X$ things and the symbol $y$ the class of $Y$ things, then we have the following scheme:

| | |
|---|---|
| all $X$ is $Y$ | $x(1 - y) = 0$, |
| no $X$ is $Y$ | $xy = 0$, |
| some $X$ is $Y$ | $xy \neq 0$, |
| some $X$ is not $Y$ | $x(1 - y) \neq 0$. |

Observe that the first two propositions are represented by equalities while the other two by inequalities. Boole preferred to express all the traditional types of categorical propositions by equations and therefore he wrote

| | | |
|---|---|---|
| some $X$ is $Y$ | as | $xy = v$, |
| some $X$ is not $Y$ | as | $x(1 - y) = v$. |

The letter $v$ seems to correspond to the English word "some". It is said to stand for a class "indefinite in all respects but one" – namely that it contains a member or members. But this convention is unsatisfactory. In Boole's system the letter $v$ can be manipulated in some respects as though it were a class symbol. This may suggest mistaken inferences. For example one may be tempted to infer from the equations: $ab = v$ and $cd = v$ that $ab = cd$. Boole himself did not fall into such fallacies by putting restrictions on the use of the letter $v$ which are inconsistent with his own description of it as a class symbol. It seems that a reason and purpose of introducing the symbol $v$ is the desire to construct a device for expressing all Aristotelian inferences, even those depending on existential import.

We find by Boole the following formulae indicating the connections between various operations on classes (those formulae were assumed by him, explicitly or implicitly, as premises):

(1) $xy = yx$,
(2) $x + y = y + x$,
(3) $x(y + z) = xy + xz$,
(4) $x(y - z) = xy - xz$,
(5) if $x = y$ then $xz = yz$,
(6) if $x = y$ then $x + z = y + z$,
(7) if $x = y$ then $x - z = y - z$,
(8) $x(1 - x) = 0$.

The formulae (1)–(7) are similar in form to rules of ordinary numerical algebra. The formula (8) is different. But Boole pointed out that even it can be interpreted numerically. He wrote (Boole 1854, pp. 37–38):

> Let us conceive, then, of an Algebra in which the symbols $x$, $y$, $z$ etc. admit indifferently of the values 0 and 1, and of these values alone. The laws, the axioms, and the processes, of such an Algebra will be identical in their whole extent with the laws, the axioms, and the processes of an Algebra of Logic. Differences of interpretation will alone divide them. Upon this principle the method of the following work is established.

Those words (and similar in other places) may suggest that Boole's system is a two-valued algebra. But this is a mistake. In fact Boole did not distinguish sharply between his original system (fulfilling rules (1)–(8)) and a narrower system satisfying additionally the following principle:

(9) either $x = 1$ or $x = 0$.

If we interpret the system in terms of classes then the principle (9) is not satisfied and if we interpret it numerically then the system satisfies even (9).

In both his books *The Mathematical Analysis of Logic* and *An Investigation of the Laws of Thought* Boole suggested a convention that the equations $x = 1$ and $x = 0$ may be taken to mean that the proposition $X$ is true or false, resp. Consequently the truth-values of more complicated propositions can be represented by combinations of small letters, e.g., the truth-value of the conjunction of the propositions $X$ and $Y$ by $xy$ and the truth-value of their exclusive disjunction by $x + y$. This enables us to interpret the Boole's system in terms of the truth-values of propositions with symbols 1 and 0 for truth and falsity respectively. This interpretation was worked out by Boole himself (without use of the phrase 'truth-value' which was invented later by G. Frege). He wrote in *The Mathematical Analysis of Logic* (pp. 49–50):

> To the symbols $X$, $Y$, $Z$ representative of Propositions, we may appropriate the elective symbols $x$, $y$, $z$ in the following sense. The hypothetical Universe, 1, shall comprehend all conceivable cases and conjectures of circumstances. The elective symbol $x$ attached to any subject expressive of such cases shall select those cases in which the Proposition $X$ is true, and similarly for $Y$ and $Z$. If we confine ourselves to the contemplation of a given Proposition $X$, and hold in abeyance any other consideration, then two cases only are conceivable, viz. first that the given Proposition is true, and secondly that it is false. As these two cases together make up the Universe of Proposition, and as the former is determined by elective symbol $x$, the latter is determined by the elective symbol $1 - x$. But if the other considerations are admitted, each of these cases will be resoluble into others, individually less extensive, the number of which will depend on the number of foreign considerations admitted. Thus if we associate the Propositions $X$ and $Y$, the total number of conceivable cases will be found as exhibited in the following scheme.

| | Cases | Elective expressions |
|---|---|---|
| 1st | $X$ true, $Y$ true | $xy$ |
| 2nd | $X$ true, $Y$ false | $x(1 - y)$ |
| 3rd | $X$ false, $Y$ true | $(1 - x)y$ |
| 4th | $X$ false, $Y$ false | $(1 - x)(1 - y)$ |

> [...] And it is to be noted that however few or many those circumstances may be, the sum of the elective expressions representing every conceivable case will be unity.

In *Laws of Thought* Boole abandoned this interpretation and proposed that the letter $x$ should be taken to stand for the time during which the proposition $X$ is true. This resembles opinions of some ancient and medieval logicians.

In Boole (1847) and, in a more clear way, in Boole (1854) we find also another interpretation. In *Mathematical Analysis of Logic* Boole says that the theory of hypothetical propositions could be treated as part of the theory of probabilities and in *Laws of Thought* we have an interpretation according to which the letter $x$ stands for the probability of the proposition $X$ in relation to all the available information, say $K$. Hence we have:

(10) if $X$ and $Y$ are independent then
$Prob_K(X \text{ and } Y) = xy$,

(11) if $X$ and $Y$ are mutually exclusive then
$Prob_K(X \text{ or } Y) = x + y$.

We see that Boole's system may have various interpretations and Boole himself anticipated that fact. The essence of his method is really the derivation of consequences in abstract fashion, i.e., without regard to interpretation. He made this clear in the opening section of *Mathematical Analysis of Logic*. If he sometimes allows himself to think of his symbols as numerical, that is merely a concession to custom.

The fundamental process in the formal elaboration of Boole's system is so called development. Let $f(x)$ be an expression involving $x$ and possibly other elective symbols and algebraic signs introduced above. Then we have $f(x) = ax + b(1 - x)$. It can be easily seen that $a = f(1)$ and $b = f(0)$. Hence

$$f(x) = f(1)x + f(0)(1 - x)\,.$$

It is said that this formula gives the development of $f(x)$ with respect to $x$.

If we have an expression $g(x, y)$ with two elective symbols $x,\ y$ (and the usual symbols of operations), then according to this method we obtain the following development of it:

$$\begin{aligned} g(x, y) &= g(1, y)x + g(0, y)(1 - x),\\ g(1, y) &= g(1, 1)y + g(1, 0)(1 - y),\\ g(0, y) &= g(0, 1)y + g(0, 0)(1 - y), \end{aligned}$$

and consequently using the rule (3):

$$g(x, y) = g(1, 1)xy + g(1, 0)x(1 - y) + g(0, 1)(1 - x)y + g(0, 0)(1 - x)(1 - y).$$

Factors $x$, $1 - x$ in the development of $f(x)$ and $xy$, $x(1 - y)$, $(1 - x)y$, $(1 - x)(1 - y)$ in the development of $g(x, y)$ are called constituents. We see that for one class $x$ we have 2 constituents, for two classes $x,\ y$ we have 4 constituents, and in general for $n$ classes we have $2^n$ constituents. The sum of all constituents of any development is equal 1, their product is equal 0. Hence they may be interpreted as a decomposition of the universe into a pairwise disjoint classes. The development of an expression is treated by Boole (cf. Boole 1847, p. 60) as a degenerate case under Maclaurin's theorem about the expansion of functions in ascending powers (he relegates this notion to a footnote in Boole (1854, p. 72)).

The development of elective expressions served Boole to define two other operations, namely solution and elimination. Assume that we have an equation $f(x) = 0$ where $f(x)$ is an expression involving symbol $x$ and maybe other elective symbols. We want to write $x$ in terms of those other symbols. Using development of $f(x)$ one has:

$$f(1)x + f(0)(1 - x) = 0,$$

$$[f(1) - f(0)]x + f(0) = 0,$$

$$x = \frac{f(0)}{f(0) - f(1)}\,.$$

Observe that Boole introduces here the operation of division to which he has assigned no fixed interpretation. But he suggests here a special method of elimination of this difficulty. Developing the expression

$$\frac{f(0)}{f(0) - f(1)}$$

we get a sum of products in which the sign of division appears only in the various coefficients, each of which must have one of the four forms: $\frac{1}{1}, \frac{0}{0}, \frac{0}{1}, \frac{1}{0}$. The first is equal 1 and the third 0. The fourth can be shown not to occur except as coefficient to a product which is separately equal 0, hence it is not very troublesome. The second $\frac{0}{0}$ is curious. Boole says that it is a perfectly indeterminate symbol of quantity, corresponding to "all, some, or none". Sometimes he equates it with the symbol $v$ but it is not quite correct. It is better to use another letter, say $w$. Hence we have

$$x = \frac{1}{1}p + \frac{0}{0}q + \frac{0}{1}r + \frac{1}{0}s,$$

$$x = p + wq.$$

We can interpret it saying that the class $x$ consists of the class $p$ together with an indeterminate part (all, some, or none) of the class $q$. In an unpublished draft (cf. Boole 1952, pp. 220–226) Boole wrote that the coefficients $\frac{1}{1}, \frac{0}{0}, \frac{0}{1}, \frac{1}{0}$ should be taken to represent respectively the categories of universality, indefiniteness, non-existence and impossibility.

To describe now the rules for elimination assume that an equation $f(x) = 0$ is given. We want to know what relations, if any, hold independently between $x$ and other classes symbolized in $f(x)$. Solving this equation we get

$$x = \frac{f(0)}{f(0) - f(1)}\,.$$

Hence

$$1 - x = -\frac{f(1)}{f(0) - f(1)}\,.$$

But $x(1 - x) = 0$, hence

$$-\frac{f(0)f(1)}{[f(0) - f(1)]^2} = 0\,,$$

i.e.,

$$f(0)f(1) = 0\,.$$

We have now two cases: (i) the working out of the left side of this equation yields $0 = 0$, (ii) it yields something of the form $g(y, z, \ldots) = 0$. In the case (i) we conclude that the original equation covers no relations independent of $x$, in the case (ii) we establish that the full version of the equation $f(x) = 0$ covers some relation or relations independent of $x$.

In all those reasonings we see that they involve expressions for which there is no logical interpretation. But Boole has adopted a rule according to which if one is doing some calculations in a formal system then it is not necessary that all formulas in this calculating process have a meaning according to a given interpretation – it is enough that the first and the last ones have such a meaning.

Boole's calculations were sufficient to represent all kinds of reasonings of traditional logic. In particular syllogistic reasonings might be presented as the reduction of two class equations to one, followed by elimination of the middle term and solution for the subject term of the conclusion. Consider, for example, the following reasoning: every human being is an animal, every animal is mortal, hence every human being is mortal. Let $h$ denote the class of human beings, $m$ the class of mortals and $a$ the class of animals. We have by our premises:

$$h(1 - a) = 0\,,$$
$$a(1 - m) = 0\,.$$

Hence

$$h - ha + a - am = 0\,.$$

Developing this with respect to $a$ we get

$$(h - h1 + 1 - 1m)a + (h - h0 + 0 - 0m)(1 - a) = 0,$$
$$(1 - m)a + h(1 - a) = 0\,.$$

Eliminating now $a$ we have

$$(1 - m)h = 0\,.$$

This means that every human being is mortal.

What was the meaning and significance of Boole's ideas for the development of mathematical logic? First of all we must note that he freed logic from the domination of epistemology and so brought about its revival as an independent science. He showed that logic can be studied without any reference to the processes of our minds. On the other hand Boole's work can be seen as "one of the first important attempts to bring mathematical methods to bear on logic while retaining the basic independence of logic from mathematics" (cf. Evra 1977, p. 374). The chief novelty in his system is the theory of elective functions and their development. Those ideas can be met already by Philo of Megara but Boole was the first who treated these topics in a general fashion. What more, Boole's methods can be applied in a mechanical way giving what is called today a decision procedure. Note also that the symbolism was by him only a tool – we find here no overestimation of the role of a symbolic language. Despite of all imperfections of Boole's methods (e.g., the usage of logically uninterpretable expressions) one must admit that Boole was an important figure in the transitional epoch between the traditional, purely syllogistic logic and the contemporary post-Fregean logic. To give the full account of the development one should mention here also Hugh McColl (1837–1909) who in the paper "The Calculus of Equivalent Statements" (1878–1880) has put forward suggestions for a calculus of propositions in which the asserted principles would be implications rather than equations (as it was by Boole). He proposed an algebraic system of the propositional calculus in the spirit of Boole. This was an attempt to overcome the lacks of Boole's systems and may be treated as a climax in the development of mathematical logic before Frege (who proposed a complete calculus of propositions).

We finish this section with some remarks comparing works and achievements of Boole with those of De Morgan. As we have mentioned above they were in contact over a long time and exchanged scientific ideas. Boole's *Mathematical Analysis of Logic* and De Morgan's *Formal Logic* were written and published simultaneously. Though they worked on their books independently (cf. remarks on this at the beginning of this chapter) their ideas were similar. De Morgan wrote on 27 November 1847 in a draft of a letter to Boole (not sent): "Some of our ideas run so near together, that proof of the physical impossibility of either of us seeing the other's work would be desirable to all those third parties who hold that where plagiarism is possible $1 = a$ whenever $a$ is 0" (cf. Smith 1982, p. 24). In a version sent to Boole we find the following words indicating what was the similarity of their approaches: "There are some remarkable similarities between us. Not that I have used the connection of algebraical laws with those of thought, but that I have employed mechanical modes of making transitions, with notation which represents our head work" (cf. Smith 1982, p. 25).

Studying their correspondence we come to the conclusion that while in the earlier years the influence flowed from De Morgan to Boole, later the direction has changed. We see nearly always De Morgan reacting to Boole's ideas, rather that vice versa. The influence of De Morgan on Boole was either slight or delayed. G.C. Smith (1982) states explicitly that "it was Boole who had the most original ideas and most vigorous intellect" (p. 123). Nevertheless the most important fact was that they both stimulated each other by a steady interaction of ideas.

De Morgan estimated very highly Boole's works. He wrote: "When the ideas thrown out by Mr Boole shall have borne their full fruit, algebra, though only founded on ideas of number in the first instance, will appear like a sectional model of the whole form of thought" (cf. De Morgan 1861, p. 346).

## 3. The Logical Works of Jevons

Boole's works were rather ignored by most contemporary British logicians or damned with faint praise (cf. Gardner 1958). One of persons who saw their importance was William Stanley Jevons (1835–1882), a logician and economist. He regarded Boole's achievements as the greatest advance in the history of logic since Aristotle. But he noticed also defects of Boole's system. He believed that it was a mistake that Boole tried to make his logical notation resemble algebraic notation. "I am quite convinced that Boole's forms [...] have no real analogy to the similar mathematical expressions" – as he stated in a letter (cf. *The Letters and Journals of W. Stanley Jevons*, 1886b, p.350). He saw also the weakness in Boole's preference for the exclusive rather than the inclusive interpretation of "or".

His own system was supposed to overcame all those defects. Jevons devised a method called by him the "method of indirect inference". He wrote: "I have been able to arrive at exactly the same results as Dr. Boole without the use of any mathematics; and though the very simple process which I am about to describe can hardly be said to be strictly Dr. Boole's logic, it is yet very similar to it and can prove everything that Dr. Boole proved" (cf. Jevons 1870a, p. 247).

We shall describe now the system of Jevons. He introduced the following operations performed on classes: intersection, complement and the inclusive union + (denoted by him originally by ·|· to distinguish it from Boole's exclusive union). The null class was denoted by 0 and the universe class by 1. The expression $X = Y$ indicated the equality of the classes $X$ and $Y$, i.e., the fact that they have the same elements. To express the inclusion of $X$ in $Y$ Jevons wrote $X = XY$. He noted the following properties of those operations:

$$
\begin{aligned}
&XY = YX\,,\\
&X + Y = Y + X\,,\\
&X(YZ) = (XY)Z\,,\\
&X + (Y + Z) = (X + Y) + Z\,,\\
&X(Y + Z) = XY + XZ\,.
\end{aligned}
$$

He used also the principle of identity $X = X$, of contradiction $Xx = 0$ (Jevons symbolized a complement using a lower-case letter – he borrowed this convention from De Morgan, cf. section 1) as well as *tertium non datur* $X + x = 1$ and the rule of substitution of similars. The last rule is used by him to verification of inferences. Take for example the following reasoning (known in the traditional logic as the mood Barbara):

$$\begin{array}{cccc} \text{All} & S & \text{is} & M \\ \text{All} & M & \text{is} & P \\ \hline \text{All} & S & \text{is} & P \end{array}$$

or symbolically

$$\begin{array}{c} SaM \\ MaP \\ \hline SaP \end{array}$$

The first premise is written by Jevons as $S = SM$, the second as $M = MP$. Substituting $M$ for $MP$ in the first premise we get $S = S(MP) = (SM)P$. But $S = SM$, hence $S = SP$, i.e., $SaP$.

Jevons developed his logic using properties of operations and Boole's idea of constituents. The easiest way to explain it is to give an example. Consider the following syllogistic premises: "All $A$ is $B$" and "No $B$ is $C$". First we write a table exhausting all possible combinations of $A$, $B$, $C$ and their negations (cf. Boole's constituents):

$$\begin{array}{c} ABC \\ ABc \\ AbC \\ Abc \\ aBC \\ aBc \\ abC \\ abc\,. \end{array}$$

Jevons called such a list an "abecedarium" and later a "logical alphabet". The premise "All $A$ is $B$" tells us that the classes $Abc$ and $AbC$ are empty, similarly from "No $B$ is $C$" it follows that $ABC$ and $aBC$ are empty. The final step is now to inspect the remaining classes, all consistent with the premises, to see what we can determine about the relation of $A$ to $C$. We note that "No $A$ is $C$" (there are no remaining combinations containing both $A$ and $C$). In this way we got a reasoning known in the traditional logic as the mood Celarent. Similarly, under the additional assumption that none of the three classes $A$, $B$, $C$ is empty, we can conclude that "Some $A$ is not $C$", "Some not-$A$ is $C$" etc. What is important here is the fact that Jevons' system was not limited to the traditional syllogistic conclusions (and he was very proud of it).

Jevons, as most of other logicians of that time, confined his attention almost exclusively to class logic. He combined statements of class inclusion with conjunctive or disjunctive assertions but almost never worked with truth-value relations alone. He preferred also, like Boole, to keep his notation in the form of equations (writing for example $A = AB$ to express the fact that all $A$ is $B$). This preference for "equational logic" – as Jevons called it – was one of the reasons of the fact that he did not consider problems of a truth-value nature (though his method operates very efficiently with such problems).

The methods of Boole and of Jevons were suitable for a mechanization. Jevons devised a number of laborsaving devices to increase the efficiency of his method. The most important were his "logical abacus" and his "logical machine". The first, described in Jevons (1869) "consisted of small rectangular wooden boards, all the same size, and each bearing a different combination of true and false terms. The boards were lined up on a rack. An arrangement of pegs on the side of each board was such that one could insert a ruler under the pegs and quickly pick out whatever group of boards one wished to remove from the rack" (Gardner 1958, p. 167).

Jevons' logical machine was described by him in (Jevons 1870b) and in (Jevons 1874). It was built for him by a young clockmaker in Salford in 1869. In 1870 Jevons demonstrated his machine at a meeting of the Royal Society of London. In 1914 his son gave it to the Science Museum, South Kensington, London but in 1934 it was transferred to the Oxford Museum of the History of Science, Old Ashmolean Building, Oxford, where it is now on display.

Jevons' logical machine resembles a miniature upright piano about 3 feet high. On the face of it are openings through which one can see letters representing the 16 possible combinations of four terms and their negations (the constituents). Each combination forms a vertical row of four terms. The keyboard consists of 21 keys arranged in the following way:

| | | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| FINIS | ·\|· | d | D | c | C | b | B | a | A | COPULA | A | a | B | b | C | c | D | d | ·\|· | FULL STOP |

We have 5 "operational keys" and 16 keys representing terms, positive and negative. The "copula" is pressed to indicate the sign of equality connecting left and right sides of an equation. The "full stop" is pressed after a complete equation has been fed to the machine. The "finis" key restores the machine to its original condition. The keys "·|·" represent the inclusive "or". They are used whenever the "or" relation occurs within either the left or right sides of an equation.

The machine is operated by pressing keys in the order indicated by the terms of an equation. If we have for example an equation $A = AB$ (i.e., "All $A$ is $B$") then we press keys in the following order: $A$ (on the left), copula, $A$ (on the right), $B$ (on the right), full stop. This action automatically eliminates from the face of the machine all combinations of terms inconsistent with the proposition just fed to the machine. Additional equations are handled exactly in the same manner. After all premises have been fed to the machine, its face is then inspected to determine what conclusions can be drawn.

Jevons' machine had no practical use. One of the reasons was the fact that complex logical questions seldom arise in everyday life. It could be used, say, as a classroom device for demonstrating the nature of logical analysis. The machine itself had also several defects (what is quite understandable for it was the first of its kind): (1) since statements should be fed to the machine in a clumsy equational form, it is made unnecessarily complicated, (2) there was no efficient procedure of feeding "some" propositions to it, (3) it was impossible to extend the mechanism to a large number of terms (Jevons planed to build a machine for ten terms but abandoned the project when it became clear that the device would occupy the entire wall space of one side of his study), (4) the machine merely exhibits all the consistent lines of the logical alphabet but it does not perform the analysis of them to indicate the desired conclusion.

The Jevons' idea of building a logical machine was developed after him. Several other devices were proposed. We want to mention here only Allan Marquand (1853–1924) and his machine from 1881–1882, Charles P. R. Macaulay whose machine (built in 1910) combined the best features of both Jevons' and Marquand's machines being an extremely compact and easily operated device and Annibale Pastore (1868–1956) and his machine constructed in 1903. They were all mechanical devices and ancestors of modern electrical machines. The first electrical analogue of Jevons' machine was suggested by A. Marquand in 1885 and in 1947 an electrical computer was constructed at Harvard by T.A. Kalin and W. Burkhart for the solution of Boolean problems involving up to twelve logical variables (i.e., propositions or class letters).

What was the import of Jevons' contribution to the development of logic? First of all he developed the ideas and methods of Boole removing some of their defects, e.g., introducing the inclusive interpretation of "or" what made possible a great simplification and what was welcome by all later writers on the algebra of logic. He did also very much to mechanize Boole's methods inventing various devices which facilitated the practical usage of them.

## 4. Venn and Logical Diagrams

Talking about the mechanization of Boole's methods and about Jevons we must pay some attention to John Venn (1834–1923) and to the idea of logical diagrams. Venn did not have a high opinion of Jevons as a logician. He wrote (1881): "Excellent as much of Jevons' work is, I cannot but hold that in the domain of logic his inconsistencies and contradictions are remarkable" (p. 78). There was a strong rivalry between the two men. Venn dismissed Jevons' logic machine as essentially trivial. He developed an idea of logical diagrams which were, in his opinion, a better and more efficient device to solve logical problems.

Before discussing Venn's diagrams in detail we say some words about his logic in general.

In Venn's opinion the aim of symbolic logic was to build a special language which would enable "to extend possibilities of applying our logical processes using symbols" (cf. Venn 1881, p. 2).

He used Latin letters as symbols of classes, 0 and 1 as symbols of the null and universe classes, resp., introduced complement, intersection, union, subtraction and division of classes. By $x - y$ he meant the class remaining after elimination from the class $x$ the elements of the class $y$ provided that $y$ is a part of $x$. Subtraction was a converse operation to the union. Similarly division was considered as a converse to the intersection. It was not determined uniquely. The expression $x > 0$ denoted the fact that the class $x$ is not empty. It was a negation of the expression $x = 0$. The equation $x = y$ was understood by Venn as the sentence "there are no entities which belong to $x$ but do not belong to $y$ and there are no entities belonging to $y$ but not to $x$". He wrote it as $x\overline{y} + \overline{x}y = 0$. Venn formulated also several properties of operations (not proving them at all).

As one of the main aims of symbolic logic Venn considered solution of logical equations and elimination of variables. Contrary to Boole and Jevons he considered not only equations but also inequalities. Solving such problems he used algebraical methods as well as diagrams. The latter is one of his main achievements in symbolic logic and we want now to study it carefully.

One can define a logical diagram as a two-dimensional geometric figure with spatial relations that are isomorphic with the structure of a logical statement. Historically the first logic diagrams probably expressed statements in what today is called the logic of relations. The tree figure was certainly known to Aristotle, in medieval and Renaissance logic one finds the so called tree of Porphyry which is another example of the type of diagram. We meet trees also by Raymond Lull, we see diagrams in various "squares of opposition" (showing certain relations of immediate inference from one class proposition to another), in "pons asinorum" of Petrus Tartaretus.

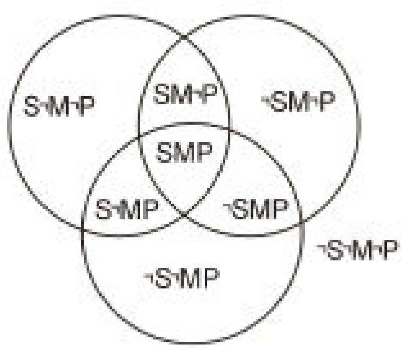
As a first important step toward a diagrammatic method sufficiently iconic to be serviceable as a tool for solving problems of class logic we may consider the use of a simple closed curve to represent a class. It is impossible to say who used it as first. We must mention here J.Ch. Sturm and his *Universalia Euclidea* (1661), G. W. Leibniz (who used circles and linear diagrams), J.Ch. Lange (and his *Nucleus logicae Weisianae* (1712), J.H. Lambert (and his linear method of diagramming explained in *Neues Organon* 1764) and L. Euler. The latter introduced them into the history of logical analysis (cf. Euler 1768–1772).

J. Venn's method is a modification of Euler's. It has elegantly overcome all limitations of the latter. Venn first published his method in an article from 1880 and then discussed it more fully in his book from 1881.

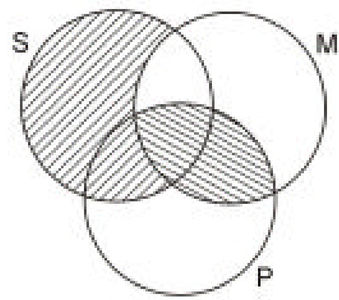
To describe the Venn's method let us show first how does it apply to a syllogism. Consider for example the following reasoning (the mood Celarent):

$$\frac{\begin{array}{c}\text{All } S \text{ is } M\\ \text{No } M \text{ is } P\end{array}}{\text{No } S \text{ is } P.}$$

Draw three circles intersecting each other and representing, resp., $S$ (subject), $M$ (middle term), $P$ (predicate). All the points inside a given circle are regarded as members of the class represented by the given circle, all points outside it – as members of the complement of this class. We get the following picture:

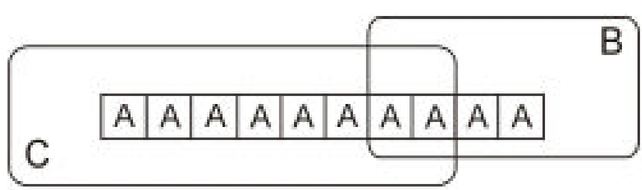



It represents simply the decomposition of the universe (represented by the plane) into 8 constituents (we use the symbol ¬ to denote the negation). If we wish now to show that a given compartment is empty we shade it. If we wish to show that it has members, we place a small $x$ inside it. If we do not know whether an element belongs to one compartment or to an adjacent one, we put $x$ on the border between the two areas. Hence to diagram our first premise "All $S$ is $M$" we must shade all compartments in which we find $S$ and $\neg M$ (because we interpret it to mean the class of things which are $S$ and $\neg M$ is empty), i.e., compartments $S\neg M\neg P$ and $S\neg MP$. The second premise "No $M$ is $P$" says that all compartments containing $MP$ are empty. Hence we shade compartments $SMP$ and $\neg SMP$. We get the following diagram:

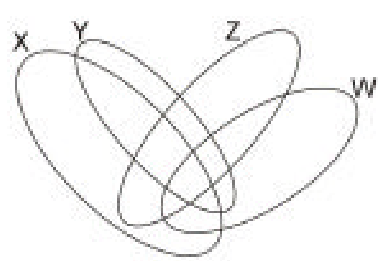



One sees that all areas containing both $S$ and $P$ are empty; hence we may conclude "No $S$ is $P$". Consequently our scheme is correct. If we assume that $S$ is not an empty class, we may also conclude that "Some $S$ is not $P$".

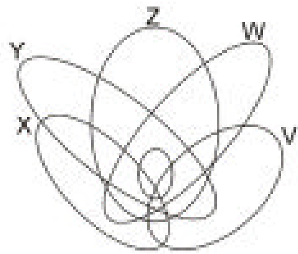
Simply modification of Venn's diagram allow us to take care of numerical syllogism in which terms are quantified by "most" or by numbers. We must change at least one of the circles to a rectangle. Here is an example indicating how one can diagram the syllogism: there are ten $A$'s of which four are $B$'s; eight $A$'s are $C$'s; therefore at least two $B$'s are $C$'s.




One of the merits of Venn's system of diagrams is that it can be extended in principle to take care of any number of terms, but, of course, as the number of terms increases, the diagram becomes more involved. For four terms we use ellipses to diagram all the constituents:



For five terms Venn proposed the following diagram:




The diagram has a defect that the class $Z$ has a shape of a doughnut – the small ellipse in the center being outside $Z$ but inside $W$ and $Y$. Beyond five terms Venn proposed to divide a rectangle into the desired number of subcompartments, labelling each with a different combination of the terms.

The method of Venn's diagrams has been developed and improved. We mention here only diagrams of Allan Marquand (lying on the border line between a highly iconic system of Venn and a noniconic system of notation) (cf. Marquand 1881), Alexander Macfarlane (cf. Macfarlane 1885, 1890), W.J. Newlin (and his method of deviding a square) (cf. Newlin 1906), W.S. Hocking (cf. Hocking 1909) and diagrams of an English logician and mathematician Lewis Carroll (whose proper name was Dodgson Charles Lutwidge; 1832–1898). He explained his method in two books: *The Game of Logic* (1886) and *Symbolic Logic* (1896). It was similar to Marquand's divisions of a square – apparently Carroll was not familiar with Marquand's work. Carroll's method easily took care of syllogisms with mixtures of positive and negative forms of the same term, it could be extended to $n$ terms (in *Symbolic Logic* Carroll pictured a number of these extensions including a 256-cell graph for eight terms).

Venn's diagrams can be used for solving problems not only in the class logic but also in the propositional calculus. We must only interpret them in a different way: each circle stands now for a proposition which may be either true or false, the labels in the various compartments indicate possible or impossible combinations of true and false values of the respective terms, we shade a compartment to indicate that it is an impossible combination of truth values (an unshaded compartment indicates a permissible combination). Using this procedure we can solve with the help of diagrams various problems of propositional logic which concern simple statements without parentheses. In the latter case some insolvable difficulties arise.

Nevertheless diagrams developed by J. Venn (and later improved by others) proved to be a fruitful and useful method in the algebra of logic. It is not true that J. Venn only improved slightly Euler's method by changing shapes of figures. The main difference between Euler and Venn lies in the fact that Venn's method was based on the idea of decomposition of the universe into constituents (which we do not have by Euler). Diagrams served Venn not only to illustrate solutions obtained in another way but they were in fact a method of solving logical problems.

## 5. Conclusions

Having presented the ideas and works of English logicians of 19th century we come now to the conclusions. What was the meaning and significance of their achievements, what was their contribution to the development of mathematical logic?

Mathematical logic of the 19th century was developed in connection with mathematics, i.e., mathematics was a source of patterns, a model for logic, served as an ideal which was imitated. Mathematical methods – mainly the methods of formal algebra – were applied to logic. Therefore the main form of logic in the 19th century was the algebra of logic. The analogy between logic and algebra which led to the origin of the algebra of logic was the following: a solution of any problem by solving an appropriate equation is in fact a derivation of certain consequences from the initial conditions of the problem. Hence the idea of extending this method to problems of unquantitative character. This tendency had a source and a good motivation also in Leibniz's works and ideas (cf. his project *characteristica universalis*).

In this way, starting from the traditional syllogistic logic, developing the ideas of Leibniz and applying some ideas of formal algebra, the logicians of the 19th century came to the idea of the algebra of logic. Those ideas were developed first of all just by English logicians. De Morgan may be treated here as a forerunner and Boole was the man who established the main principles and tools. The problem was to develop, in analogy to the algebra, a method of expressing unquantitative information in the form of equations and to establish rules of transformation of those equations. This led to the construction of systems called today Boolean algebras. They were formulated in the language of classes as extension of concepts. Later another interpretation – namely the logic of propositions – was developed. In this way logic was freed from the domination of epistemology and so it became an independent science. It was shown that logic can be studied without any reference to the processes of our mind.

The idea of the algebraization of logic led also to the search for mechanical methods of solving logical problems. Various methods based on the usage of logical machines and diagrams were proposed (Jevons, Venn, Carroll and others). Considerations of Boole and De Morgan led also to the development of the general theory of relations.

We may say that English logicians played the first fiddle in mathematical logic in the 19th century. Of course their contributions did not contain the ideas in a perfect and final form. They had various drawbacks and defects. Later logicians developed them removing the imperfections (let us mention here the works of Peirce, Gergonne and Schröder). Nevertheless the works of English logicians formed the main step in the transition epoch between the traditional syllogistic logic and the prehistory of mathematical logic on the one hand and the Fregean period on the other. In the former the very idea of mathematical logic arose, mainly due to Leibniz. The latter was characterized by new aims of logic – it was then developed to build foundations of mathematics as a science, mathematics and mathematical methods were the subject of logical studies. The transitional period, called in the literature the Boolean (!) period (cf. Bocheński 1962) was necessary to create mathematical logic as an independent science. It was done by using mathematics as a template and by applying mathematical, in particular algebraic, methods. This period was started with De Morgan's *Formal Logic* and Boole's *The Mathematical Analysis of Logic* (1847) and ended with Schröder's *Vorlesungen über die Algebra der Logik* (1890–1905). Works of English logicians pointing out the directions of research and establishing the fundamental ideas and methods were crucial in this period.

# 18

# THE DEVELOPMENT OF SYMBOLISM IN LOGIC AND ITS PHILOSOPHICAL BACKGROUND[1]

(Co-authored by Thomas Bedürftig)

## 1. Introduction

The aim of the paper is to give an overview of the development of symbolism in logic. The turning point in this development were the 19th and 20th centuries – we will indicate the most important moments in this process. The development of the logical symbolism cannot be treated independently of the development of symbols in mathematics and both are strictly connected with the symbolic culture which is one of the elements distinguishing human beings and animals. Therefore we shall start with some general remarks on the rôle played by symbols in the development of culture. Then we shall briefly analyze the importance of symbols in the development of mathematics. This will enable us to indicate some general rules of introducing and developing mathematical symbols as well as reciprocal influences between the development of mathematics and the development of symbolism. Similar interdependencies can be observed also in logic which is our main subject.

We shall start by considerations concerning the beginnings of logical symbolism in the antiquity and in the Middle Ages. Then the idea of *characteristica universalis* of G.W. Leibniz and the idea of algebraization of logic of G. Boole will be discussed. Both those ideas were crucial in the process of developing a modern logical notation which was done mainly by G. Peano and B. Russell. We shall mention also the system notation of G. Frege and J. Łukasiewicz. At the end the system of notation used in logic nowadays will be considered.

[1]Originally published as a joint paper with Th. Bedürftig under the title "Die Entwicklung der Symbolik in der Logik und ihr philosophischer Hintergrund", *Mathematische Semesterberichte* 42 (1995), 1–31.  Translated into English by Th. Bedürftig and R. Murawski. 

In: Roman Murawski, *Essays in the Philosophy and History of Logic and Mathematics* (*Poznań Studies in the Philosophy of the Sciences and the Humanities*, vol. 98), pp. 271–299. Amsterdam/New York, NY: Rodopi, 2010.

We will be interested not so much in particular systems of symbols proposed by various authors as in philosophical background of the process of introducing symbolic language into logic. We would like both to stress the crucial moments of the process and to exhibit the philosophical motivations behind the search by various logicians and mathematicians for a good and adequate symbolism.

We will restrict ourselves to the development of the symbolic language of logic and we will not consider the metalogical symbolism. We understand here logic in the narrower sense, i.e., as propositional calculus and predicate calculus and not in the broader sense as, e.g., Russell did (*logica magna*).

## 2. Culture and Symbols

The culture created and developed by human beings is symbolic, i.e., the communication within it is based on the usage of symbols. Observe that the ability to use symbols, to call things and events and to represent them by names, is characteristic for human beings. Only a human being possesses the so called symbolic initiative, i.e., she/he can associate certain symbols with things and ideas as well as establish relations between them and operate on them on the level of concepts. Animals do not have this ability – they have only a symbolic reflex, i.e., they have the ability to react on specific symbolic impulses. It is clear that only a human being can build symbols and use them consciously. Thanks to this ability she/he can – by using appropriate notions – structure and convey her/his observations, experiences and considerations. It is just symbolization that makes the whole culture not only possible but that creates also conditions necessary to its persistence and development.

Any natural language can serve as an example of a symbolic system. In logic and mathematics language plays not only the role of communication but it enables us to make statements shorter and clearer and this consequently helps to make reasoning more precise and clear.

## 3. The Development of Mathematical Symbolism

In mathematics, which is one of the products of cultural activity of human beings, one can spot similar tendencies as in other cultural systems. Hence the great rôle played here by symbols and symbolization. We see them already at the very beginnings of mathematics. The first efforts to write down numbers (what was connected with the process of counting) are just examples of using symbols.

One can distinguish three important steps on the way to obtain a good and useful notation: (1) the occurrence of the idea of a place-value system, (2) the invention of zero, and (3) the cipherization. The idea of a place-value system, i.e., of a system in which the value of a symbol depends on the place on which it stand, can be seen for the first time in Babylonian (about 2100 B.C.) and Mayan cultures. The special symbol for zero was necessary to indicate that at a given place of a number written in a place-value system there is nothing and no value should be associated with it. At the beginning one left simply the appropriate place empty. But this method could not be applied in the case of the last place in a number. Therefore there was a necessity of introducing a special symbol. This appeared in Babylon about 400–300 B.C. and about the beginning of the Christian era by Mayan. Note that this symbol for zero denoted only an empty place and not a number! Only some centuries later zero became the status of a number (as one, two, three, etc.). By cipherization we mean the introduction of symbols denoting numbers which form the base of a given system. Hindu-arabic numerals used today are the final stage of a long process (cf. Ifrah 1989) leading from complicated systems of signs on clay tablets in Mesopotamia through systems of combining sticks in China and the idea of using letters of Greek alphabet as signs for numbers.

One can say that a place-value system of notation for numbers is an achievement of human beings which may be compared with the introduction of an alphabet. Both those inventions replaced complicated symbolisms by an easy method understandable for everybody. A useful notation system for numbers was important first of all for practical reasons – it made counting and calculations easier what was significant, e.g., in business and land survey. On the other hand it was important also in theoretical studies developed especially in Greece. It helped for example to achieve greater precision in formulating problems and to perform various calculating operations. All that stimulated further development of number theory and arithmetic what then inspired search for still better symbolism.

One should mention here Diophantus, the Greek mathematician from the 3rd century B.C. One finds by him the first systematic usage of algebraic symbols. In his work *Arithmetica* he used symbols for variables (similarly as by Aristotle and Euclid) and their powers, there appears a power with a negative exponent as well as notation for constant coefficients in an equation. Diophantus used also two new symbols – for an identity and for the subtraction (it is not clear whether the latter symbol denoted an operation or symbolized minus by a negative number). Note that his symbols were rather abbreviations than symbols in our sense of the word but they made possible for him to use formulas and to transform them almost mechanically without laborious mental operations They enabled him also to consider more complicated and difficult problems.

The next milestone in introducing symbols in mathematics formed works by F. Viète (1540–1603), a French lawyer. As one of the first he replaced in the theory of equations numbers by letters – they denoted arbitrary coefficients. He used also signs + and − in our present meaning and "$A$ quadratum" for $A^2$. All this stimulated the development of algebra and enabled René Descartes (1596–

1650) to apply algebraic methods to geometrical considerations (analytic geometry) unifying those two branches of mathematics. One finds by Descartes a lot of modern symbols, e.g., the formula

$$\frac{1}{2}\,a + \sqrt{\frac{1}{4}\,aa + bb}\;.$$

The only difference between this and our present notation is *aa* instead of our $a^2$.

The mathematician and logician who not only introduced many symbols used today (e.g., symbols for derivatives and differential $dx$, $dy$, $\frac{dy}{dx}$, the symbol for integral $\int$ as well as symbols = and $\cdot$) but who as first considered the problem of symbolization of mathematics and logic in a general setting was Gottfried Wilhelm Leibniz (1646–1716) – we return to him later.

## 4. Some General Remarks on the Regularities in the Development of Mathematical Symbolism

Considering the development of the mathematical symbolism one notice some regularities – they can be seen also in the development of the symbolism in logic. Therefore we would like to indicate them now.

In the whole history of mathematics one can see the tendency to find (to construct) an appropriate and universal symbolism. One of the reasons was the growing abstractness of considerations as well as the complexity of considered problems. And new symbols could be introduced only when the domain has been developed enough. Moreover, the dependencies are here reciprocal: the growth of a domain enables and forces new and more adequate symbolism and *vice versa*, a good symbolism enables the further development of the domain. A good example is here the algebraic notation of Viète and the evolution of modern algebra in the 16th and 17th centuries. Bell writes in (1945, p. 123):

> Unless elementary algebra had become "a purely symbolic science" by the end of the sixteenth century, it seems unlikely that analytic geometry, the differential and integral calculus, the theory of probability, the theory of numbers, and dynamics could have taken root and flourished as they did in the seventeenth century. As modern mathematics stems from these creation of Descartes, Newton and Leibniz, Pascal, Fermat and Galileo, it may not be too much to claim that the perfection of algebraic symbolism was a major contributor to the unprecedented speed with which mathematics developed after the publication of Descartes' geometry in 1637.

Another example (that became classical today) of the reciprocal influence of a good symbolism and the development of mathematics is Leibniz's $d$-notation for the differential and integral calculus. Symbols $dx$, $dy$, $\frac{dy}{dx}$ as well as the symbol $\int$ were better and more suggestive than the Newton's notation $\dot{x}$, $\ddot{x}$, etc. and

$\dot{x}$, $\ddot{x}$, etc. for derivatives and integrals, resp. The notation of Leibniz has been adopted on the continent and the notation of Newton in Britain. And this was one of the reasons of the weak development of the English mathematics in the 18th century in comparison with the development of mathematics in France and Germany.

Observe that Leibniz's notation as well as his calculus in general had no adequate theoretical foundations and background. What should the differentials $dx$, $dy$ in fact be? Leibniz treated them on the one hand as fixed quantities and on the other as quantities that are smaller than any given quantity and simultaneously are not equal zero. Though this was in fact contradictory, it did not stop him to apply them and to use the $d$-notation because it was useful. Hence there arose the necessity to clear those inconsistencies – and it has been done in the so called nonstandard analysis of Abraham Robinson in the sixties of 20th century! In this way a useful though not fully justified symbolism led to the new domain of mathematics. One can see on this example that a convenient symbolism can overlive even in the case when there fails a sufficient mathematical and philosophical justification.

As a good and adequate symbolism stimulates the growth of a domain, in the same way a bad one can hamper its development. As an example of this phenomenon can serve the calculus of fractions in the ancient Egypt and its symbolism. It hampered the development of mathematics there for a long time.

Introducing and manipulating on symbols leads to the evolution of mathematics and in particular force the introduction of new symbols. As an example recall here the history of the symbol for zero and of the concept of the number zero. It emerged from the sign for an empty place in a place-value representation of numbers and was a symbol for nothing. Another example is the history of introducing complex numbers, in particular of $\sqrt{-1}$. This symbol appeared by the solution of equations of 6th degree and denoted a non-existing number, something "unreal", fictive, imaginary. Further researches and in particular the fact of recognition of an object like $\sqrt{-1}$ and its status as a number made possible, e.g., the recognition of the fundamental theorem of algebra according to which any equation of the $n$th degree has exactly $n$ roots. In this way the complex numbers have been introduced – they are today a convenient instrument being taken for granted.

The choice of this or that symbolism depends on individual as well as on general cultural factors. The acceptance of symbols proposed by a given mathematician depends not only on their usefulness and soundness but also on the position of him/her in the world of mathematicians. One can find many examples of this phenomenon in the history of mathematics. For example, the symbols $\pi$, $e$ and $i$ have been introduced by the great and highly estimated mathematician of the 18th century Leonhard Euler (1707–1783). In fact other letters could be chosen here. The fact that just those symbols have been generally accepted was a consequence not only of their usefulness but also of the position of Euler among mathematicians.

## 5. The Beginnings of Logical Symbolism

We can move now to the proper subject of our considerations, i.e., to the development of symbolism of logic. There are three kinds of motivation inspiring the development of symbolism in logic (and mathematics): (1) the attempt to create an ideal artificial language as a substitute for an imprecise colloquial language; (2) a tendency to reduce logic to the study of properties of language or, in extreme cases, to the theory of signs; (3) a nominalistic tendency according to which abstract terms do not denote something but are only empty signs. One or more of those tendencies can be seen by all mathematicians and logicians trying to create a symbolism in mathematics or logic.

The first tendency can be seen, e.g., by Aristotle (384–322 B.C.). In *Analytics* he used simple symbols – in fact letters of the Greek alphabet – to replace sentences or properties in his syllogistic reasonings. His aim was the precision of expressions. Observe that for Aristotle symbolism was not connected with nominalistic ideas.

By the Stoics we see clearly linguistic tendencies. Their formal logic (in which they used ordinals as symbols of propositions) was the study of the formal structure of language. They treated expressions of the language as instruments connecting intellectual acts on the one hand and objects of the world on the other. Relations between symbols were considered as being necessary – they saw in them the reflection of the necessary relations between objects.

In medieval logic – which first developed the ideas of Aristotle and which was restricted mainly to syllogistic – one can see some semiotic tendencies – compare, e.g., Abelard (1079–1142), the later nominalists, Ockham (about 1300–1349). One considered there first of all the classical categorical propositions and the logical connections between them. From that time come the abbreviations $a, e, i, o$ for the affirmative and negative universal and particular cases that led to the traditional notation of categorical propositions:

| | | | |
|---|---|---|---|
| instead of | *all S is P* | one wrote | $SaP$, |
| instead of | *no S is P* | one wrote | $SeP$, |
| instead of | *some S are P* | one wrote | $SiP$, |
| instead of | *some S are not P* | one wrote | $SoP$. |

The abbreviations $a, e, i, o$ are letters that occur in the words 'affirmo' (I state) and 'nego' (I deny). For the general affirmative proposition *all S is P* one took the first vowel of the word 'affirmo' and in the particular affirmative proposition *some S are P* the second one. Similarly by the negative propositions and the word 'nego'.

In later medieval logic one finds the opinion that the foundations of grammar and logic can be found in the general theory of signs.

While considering medieval logic one should mention an attempt by Raymond Lull (about 1235 – about 1315) to create an ideal symbolic language. Unfortunately the level of science, and of mathematics in particular, was too law at that time – for example there was still no algebra – and this attempt failed. We see here the concrete illustration of a general rule which can be observed in the development of symbolism in logic: this or that symbolism cannot appear until the general situation is mature enough, i.e., the proper domain and related domains must achieve an appropriate level. The relations here are reciprocal. The development of a domain enables and forces a search for a new adequate symbolism and, *vice versa*, a good symbolism affords possibilities for the development of the domain itself.

## 6. Leibniz and the Idea of *characteristica universalis*

It was not until the 17th century, with the intense development of natural sciences and mathematics, that appropriate conditions for the development of logical symbolism were created. The general principles of a symbolism were explicitly formulated for the first time by G.W. Leibniz and expressed in his project *characteristica universalis*.

Leibniz's broad scientific interests were strongly connected with his rationalism and concern for precision and firmness of knowledge. It led him eventually to a general theory of a logical language. As Bourbaki (1960, pp. 13–14) writes (cf. also Leibniz 1875–1890, Bd. 7, p. 185):

> [Leibniz] from his childhood acquainted with scholastic logic, was enchanted by the idea (due to Raymond Lull) of a method which would reduce all notions used by human beings to some primitive notions constituting in such a way "an alphabet of human thoughts", and form combinations of them in a mechanical way, to obtain true sentences.

He presented those dreams and plans in his attempt to design a universal symbolic language *characteristica universalis*. The universality of this language should be twofold – it should make possible to write down all scientific concepts and should enable scientists of all nationalities to communicate with each other.

*Characteristica universalis* was supposed to be a system of signs fulfilling the following conditions:

1. There is to be a one-one correspondence between the signs of the system

(provided they are not signs of empty places for variables) and ideas or concepts (in the broadest sense).

2. Signs must be chosen in such a way that if an idea (thought) can be decomposed into components then the sign for this idea (thought) will have a parallel decomposition.
3. One must devise a system of rules for operating on the signs such that if an idea $M_1$ is a logical consequence of an idea $M_2$ then the "picture" of $M_1$ can be interpreted as a consequence of the "picture" of $M_2$. (This is a sort of completeness condition.)

According to these conditions all simple concepts corresponding to simple properties ought to be expressed by single graphical signs, complex concepts – by combinations of signs. This was based on a fundamental general assumption that the whole possible vocabulary of science can be obtained by combinations of some simple concepts. The method of constructing concepts Leibniz called *ars combinatoria*. It was a part of a more general method – of a calculus – which should enable people to solve all problems in a universal language. It was called *mathesis universalis*, *calculus universalis*, *logica mathematica*, *logistica*.

Leibniz hoped that *characteristica universalis* would, in particular, help to decide any philosophical problem. He wrote (cf. Leibniz 1875–1890, Bd. 7, pp. 198–201):

> And when this comes [i.e., when the idea of a universal language is realized – R.M.] then two philosophers wanting to decide something will proceed as two calculators do. It will be enough for them to take pencils, go to their tablets and say: *Calculemus*! (Let us calculate!).

Leibniz claimed that (cf. Couturat 1901, pp. 84–85):

> he owed all his discoveries in mathematics exclusively to his perfect way of applying symbols, and the invention of the differential calculus was just an example of it.

Leibniz did not succeed in realizing his idea of *characteristica universalis*. One of the reasons was that he treated logical forms intentionally rather than extensionally. This could not be reconciled with the attempt to formalize logic completely and to transform it into a universal mathematics of utterly unquantified generality. Another source of difficulty was his conviction that the combination of symbols must be a necessary result of a detailed analysis of the whole of human knowledge. Hence he did not treat the choice of primitive fundamental notions as a matter of convention. His general metaphysical conceptions induced a tendency to search for an absolutely simple and primitive concepts (an analogue of monads), the combinations of which would lead to the rich variety of notions.

## 7. Boole and De Morgan and the Idea of an Algebraization of Logic

Real progress in the development of symbolism in logic begins with the works of two 19th century English mathematicians George Boole (1815–1864) and Augustus De Morgan (1806–1871). Their main achievement was the initiation of the so called algebraization of logic. G. Boole in his books *Mathematical Analysis of Logic* (1847) and *An Investigation of the Laws of Thought on which are Founded the Mathematical Theories of Logic and Probabilities* (1854) realized for the first time the idea (found already in Leibniz) that algebraical formulas can be used to express some logical relations. He wrote in (1847):

> One mustn't any longer connect logic with metaphysics but with mathematics. [...] The domain which comes into being as a result of the study of logic as an exact science is in fact the same as that which results from the study of analysis. (p. 34)

What was really new in Boole's approach was not the attempt to apply the idea of a calculus to logic, nor the idea of a "non-quantitative calculus" – those ideas were already by Leibniz – but the clear description of the essence of this calculus, i.e., of formalism. According to Boole it is a procedure the validity of which does not depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination. His leading idea was that transformations of expressions of the language depend not on the meaning (interpretation) of symbols but only on laws of combining them which are independent of any interpretations. Boole stressed clearly the possibility of interpreting the same formal system in different ways.

But he was far from exaggerating the rôle of a symbolic language. He wrote explicitly that (cf. 1847, pp. 4–5):

> logic is possible thanks to the existence of general concepts in our mind – thanks to our ability to imagine some classes and to describe particular elements of them by common names. Hence the theory of logic is strongly connected with the theory of language. An attempt to express logical theorems by symbols, which are combined according to laws based on the laws of mental processes which they represent, would be in this sense a step towards establishing a philosophical language.

In the books mentioned above Boole developed a theory which was an extension of syllogistic. Its theoretical significance and range of applications turned out to be considerably greater. Its current version is called Boolean algebra.

Systems of Boole can be interpreted both as the calculus of classes (algebra of sets) and as a propositional calculus. The classical categorical propositions of syllogistic can be written there as follows:

Let $x$ and $y$ be classes of things which fall under the concepts $X$ and $Y$, resp. Then

| | | |
|---|---|---|
| *every X is Y* | is written as | $x(1 - y) = 0$, |
| *no X is Y* | is written as | $xy = 0$, |
| *some X are Y* | is written as | $xy = v$, |
| *some X are not Y* | is written as | $x(1 - y) = v$, |

where 1 denotes the universal class, 0 – the empty class and $V$ – a class "indefinite in all respects but one" (Boole 1847, pp. 48–49) and it is assumed that $v$ contains at least one element. The symbol $v$ is used for the word 'some'.

Boole used this a bit strange and inconsistent symbolic in order to write down all classical categorical propositions as equations. The symbol $v$ was used in this symbolism in such a way as it would denote a class. But this could lead to errors. For example, from the equations $ab = v$ and $cd = v$ one could come to the false conclusion that $ab = cd$. Boole eliminated such errors by putting some restrictions on the usage of the symbol $v$. But those restrictions were inconsistent with the thesis that $v$ is a symbol of a class.

If $X$ and $Y$ denote propositions then Boole writes down in his symbolism the four cases of combinations of 'true' and 'false' in an appropriate way. He writes (1847, p. 49):

> To the symbols $X, Y, Z$, representative of Propositions, we may appropriate the elective symbols $x, y, z$, in the following sense. The hypothetical Universe, 1, shall comprehend all conceivable cases and conjectures of circumstances. The elective symbol $x$ attached to any subject expressive of such cases shall select those cases in which the Proposition $X$ is true, and similarly for $Y$ and $Z$.

We get the following four cases:

| | | |
|---|---|---|
| 1. $X$ true and $Y$ true | is written as | $xy$, |
| 2. $X$ true and $Y$ false | is written as | $x(1 - y)$, |
| 3. $X$ false and $Y$ true | is written as | $(1 - x)y$, |
| 4. $X$ false and $Y$ false | is written as | $(1 - x)(1 - y)$. |

It is characteristic that we do find by Boole neither a symbol for implication (*if* … *then* …) nor for negation. Both are defined (by composed formulas). The symbol + is used to denote disjunction (in the exclusive sense).

De Morgan published in 1847 the book *Formal Logic*. This was in the same year as Boole published his *Mathematical Analysis of Logic* – a legend says that both appeared in bookshops on the same day! In *Formal Logic* De Morgan developed the syllogistic, formulated some ideas of the algebra of logic and the theory of relations. He proposed symbols to denote the converse of a relation, the contradictory relation and three ways in which two relations can be composed.

Charles Saunders Peirce in America and Ernst Schröder in Germany extended and developed further the algebraic tradition in logic whose formalization began with Boole and De Morgan.

## 8. Peano and the Development of Logical Symbolism

The most important steps in the development of symbolism in logic were taken by the Italian mathematician Giuseppe Peano (1858–1932) at the turn of the century. He considered the problem of symbolism several times, advocated the use of symbolic language in mathematics and proposed a very good and clear symbolism. Many of his symbols were adopted, sometimes in a slightly modified form, by modern logic and mathematics.

Peano's work was strongly influenced by Leibniz's *characteristica universalis*. Peano knew the works of Leibniz and often mentioned his ideas. He also encouraged one of his students and later an assistant, G. Vacca, to study carefully the manuscripts of Leibniz in Hannover which was a fruitful project. Peano treated the realization of Leibniz ideas as one of the main aims of mathematical sciences.

In the creation of a symbolic language of mathematics and logic Peano often claimed that (cf. 1896-1897):

> symbols represent not words but ideas. Therefore one must use the same symbol whenever the same idea appears, even if in colloquial language they are not represented by the same expression. In this way we establish a one-one correspondence between ideas and symbols; such a correspondence cannot be found in colloquial speech. This ideography is based on theorems of logic discovered since the time of Leibniz. The shape of symbols can be changed, i.e., one can change some signs representing the basic ideas, but there cannot exist two essentially different ideographies. (p. 573)

Symbolic language enables us not only to be more precise and clear, but it helps us also to understand things better, it

> enables the pupils of a secondary school to solve easily problems which in the past could be solved only by great minds such as Euclid or Diophantus

– as Peano wrote in (1915, p. 168). The other advantages of symbolism he saw are: (1) the shortness of expressions needed to state theorems and proofs, (2) the small number of basic expressions with the help of which one builds further more complicated expressions (he wrote in (1915, p. 169) that "in colloquial language one needs about a thousand words to express the logical relations; in the symbolic language of mathematical logic ten symbols are enough. [. . . ] Professor Padoa has reduced the number of necessary symbols to three."), (3) the symbolism facilitates reasonings, helps even to obtain new results. Moreover, in some cases the usage of a symbolic language is simply necessary because we have no suitable expressions in the colloquial language to state some ideas or constructions. (With such a necessity were confronted for example, according to Peano (1915), A.N. Whitehead and B. Russell when they were writing *Principia Mathematica* – "the greatest work written completely with the help of symbols" – p. 167).

Peano needed a symbolic language to express theorems clearly and precisely. It enabled him also to order various parts of mathematics by supplying them with suitable axiomatizations. We must mention here first of all his famous work *Arithmetices principia nova methodo exposita* (1889a). This small booklet with a Latin title (to be precise one should say: with almost Latin title – the word *arithmetices* is only a transliteration of an appropriate Greek word; in Latin it should be *arithmeticae*), written also in Latin (more precisely: in *Latino sine flexione* – an artificial language invented by Peano) contained the first formulation of Peano's postulates for natural numbers. Peano wrote at the beginning of the work:

> I have represented by signs all ideas which appear in the foundations of arithmetic. With the help of this representation all sentences are expressed by signs. Those signs belong either to logic or to arithmetic. [...] By this notation every sentence become a shape as precise as an equation in algebra. Having sentences written in such a way we can deduce from them other sentences – it is done by a process similar to a looking for a solution of an algebraic equation. This was exactly the aim and the reason for writing this paper. (p. 3)

Another domain of mathematics in which Peano used his symbolism and the axiomatic-deductive method was geometry – his most important work here was the booklet *I principii di geometria logicamente esposti* (1889b).

The idea of presenting mathematics in the framework of an artificial symbolic language and of deriving then all theorems from some fundamental axioms was also basic to Peano's famous project *Formulario*. It was announced in 1892 in the journal *Rivista di Matematica*. Peano founded this journal in 1892 and here his main logical papers and those of his students were published. The aim of this project was to publish all known mathematical theorems. This plan was to be carried out by using, of course, the symbolic language introduced by Peano. A special journal *Formulario Matematico* was founded to publish the results of this project. It was edited by Peano and his collaborators: Vailanti, Castellano, Burali-Forti, Giudice, Vivanti, Betazzi and others. Five volumes appeared: Introduction 1894, vol. I – 1895, vol. II – 1897–1899, vol. III – 1901, vol. IV – 1903, vol. V – 1908. The last volume contained about 4200 theorems! Peano even bought a small printing office (in 1898 from Faa di Bruno for 407 lire) and studied the art of printing. One of the results of the project was a further simplification of the mathematical symbolism. Peano treated *Formulario* as a fulfillment of Leibniz's ideas. He wrote:

> After two centuries, this "dream" of the inventor of the infinitesimal calculus has become a reality [...]. We have the solution to the problem posed by Leibniz. (1915, p. 168)

Let us consider now Peano's symbolism in detail. In the calculus of classes the expression $A = B$ meant that the classes $A$ and $B$ are equal. The symbol $A \cap B$ denoted the product (common part) of $A$ and $B$, the symbol $A \cup B$ their sum (union) and $-A$ or $\overline{A}$ the complement of the class $A$. The symbol $\bullet$ denoted the universal class and the symbol $\circ$ the empty class.

At the beginning Peano made no distinction between the propositional calculus and the calculus of classes. Hence the sign $\cap$ was read by him as "and", was called conjunction or logical multiplication and simultaneously $A \cap B$ was the product of classes $A$ and $B$. Similarly for $\cup$ – it was meant as disjunction or logical addition "or" and simultaneously $A \cup B$ was called the sum of $A$ and $B$. The sign $-$ was read as "not" and was called the negation. Only later, in the work *Arithmetices principia* (1889) one finds the distinction between both calculus though the same symbols are still used. The proper meaning became clear by distinguishing classes and propositions. In *Arithmetices principia* there appeared signs $\bigvee$ and $\bigwedge$ which denote – as Peano explains – "truth or identity" and "falsehood or absurdity", resp. The symbol $\supset$ denoted consequence: when $a$ and $b$ are sentences then the symbol $a \supset b$ denotes "$b$ follows from $a$". The same symbols denoted also inclusion. In this case Peano read the expression $a \supset b$ as "$b$ is a subset of $a$".

In the work *Arithmetices principia* one finds for the first time the symbol $\varepsilon$ as a sign for "being an element of". The letter $\varepsilon$ is the first letter of the Greek word $\dot{\varepsilon}\sigma\tau\acute{\iota}$ which means "to be". Later this symbol has been transformed and today one writes $\in$. If $p$ is a sentence then the expression $x \ni p$ denotes the class of all those $x$'s that fulfil $p$. If one denotes this class by $a$ then we get:

$$x \ni (x \varepsilon a) = a,$$

$$x \varepsilon (x \ni p) = p.$$

The class consisting of a single element $x$ was denoted by Peano by $\iota x$ and defined in the following way:

$$\iota x = y \ni (y = x).$$

Peano used also a convenient notation for the universal quantifier. If $a$ and $b$ were two formulas with free variables $x, y, \ldots$ then the expression $a \supset_{x,y,\ldots} b$ meant "for all $x, y, \ldots$ if $a$ then $b$".

One should notice that Peano was the first who distinguished between free and bounded variables – he called then, resp., "effective" (*effetivo*) and "apparent" (*apparente*).

Peano applied an interesting system of signs for parentheses. Instead of brackets (, ), [, ] etc. Peano used a system of dots: $.$, $:$, $.\!\cdot\!.$, $::$, etc. He explains the application of those dots in the following way (cf. 1889a):

> To understand a formula one should take into account that the signs that are not separated by dots are connected. Further one should connect expressions separated by one dot, then those separated by two dots etc. For example: let $a, b, c, \ldots$ be expressions. Then $ab.cd$ should be understood as $(ab)(cd)$ and the expression $ab.cd : ef.gh.\!\cdot\!.k$ as $(((ab)(cd))((ef)(gh)))k$.

As an example of Peano's notation let us give his axioms for the theory of natural numbers. They were written as follows:

$$1 \, \varepsilon \, N,$$

$$a \, \varepsilon \, N \, . \supset . \, a + 1 \, \varepsilon \, N,$$

$$a, b \, \varepsilon \, N \, . \ \supset : \ a = b \, . \ = \ . \, a + 1 = b + 1,$$

$$a \, \varepsilon \, N . \supset . \, a + 1 - = 1,$$

$$k \, \varepsilon \, K \therefore 1 \, \varepsilon \, k \therefore x \, \varepsilon \, N \, . \, x \, \varepsilon \, k \ : \supset_x \ . \, x + 1 \, \varepsilon \, k \ : : \supset \ . N \supset k.$$

The sign $N$ denotes here "positive integral", 1 denotes "one", $a$+1 – the successor of $a$ and $K$ the domain of all classes. Today we would write those axioms in the following way:

$$1 \in N,$$

$$(a \in N) \longrightarrow (a + 1 \in N),$$

$$(a, b \in N) \longrightarrow [(a = b) \equiv (a + 1 = b + 1)],$$

$$(a \in N) \longrightarrow (a + 1 \neq 1),$$

$$[(k \in K) \ \& \ (1 \in k) \ \& \ \forall x (x \in N \ \& \ x \in k \longrightarrow x + 1 \in k)] \longrightarrow (N \subset k).$$

[Remark: As a symbol for equivalence we have chosen the sign $\equiv$ and we use the universal quantifier $\forall x$ that was used by Peano only in connection with the symbol of implication (consequence) $\supset_x$.]

## 9. Russell and the Problem of a Symbolic Language

The symbolism proposed by Peano was simple, clear and transparent. As already mentioned above, it was adopted almost without changes by modern logic (and mathematics). This was mainly due to the English mathematician, logician and philosopher Bertrand Russell (1872–1970) who applied it in the fundamental work – written together with the English-American mathematician and philosopher Alfred North Whitehead (1861–1947) – *Principia Mathematica* (vol. I – 1910, vol. II – 1912, vol. III – 1913). This work was in a sense the beginning of modern formal logic. Russell's influence and the significance of *Principia* as well as features of Peano's symbolism were the factors which made Peano's symbolism commonly adopted and used. Slight modifications introduced by Russell and other logicians consisted of small changes in the shape of some symbols.

Russell saw in symbolism not only a useful tool which enables us to express logical and mathematical ideas in short, precise and clear way but was also aware of the deeper significance of the rôle it plays in logic and mathematics. In (1949) he wrote:

> It is not easy for the lay mind to realize the importance of symbolism in discussing the foundations of mathematics, and the explanation may perhaps seem strangely paradoxical. The fact is that symbolism is useful because it makes things difficult. (This is not true of the advanced parts of mathematics, but only of the beginnings.) What we wish to know is what can be deduced from what. Now, in the beginnings, everything is self-evident; and it is very hard to see whether one self-evident proposition follows from another or not. Obviousness is always the enemy to correctness. Hence we invent some new and difficult symbolism in which nothing seems obvious. Then we set up certain rules for operating on the symbols and the whole thing becomes mechanical. In this way we find out what must be taken as premiss and what can be demonstrated or defined. For instance, the whole of arithmetic and algebra has been shown to require three indefinable notions and five non-demonstrable propositions. But without a symbolism it would have been very hard to find this out. It is so obvious that two and two are four, that we can hardly make ourselves sufficiently sceptical to doubt whether it can be proved. And the same holds in other cases where self-evident things are to be proved. (pp. 4–5)

Russell's attitude towards logical symbolism was more complicated than one would judge by the above words. We mean here he problem of relations between symbols and reality. In an earlier work *The Principles of Mathematics* (1903) he claimed that all symbols have meaning and express an objective existence of objects (which he identified, in a Platonic way, with concepts). He wrote in (1903): "Every word occurring in a sentence must have a meaning" (p. 17). Later he changed his mind somewhat allowing for example classes which may be "incomplete symbols, which needn't have any meaning" (1910–1913, vol. I, p. 67). In *Principia* he wrote also: "Hence the classes, if we introduce them, are simply symbolical and linguistical conventions and not real objects" (1910–1913, vol. I, p. 187). But he always returned to the position of realism. In the lectures of 1918 he claimed: "In correct symbolism there will always be a fundamental identity between the structure of a fact and its symbol; the complexity of a symbol exactly corresponds to the complexity of the fact which it symbolizes" (1918–1919, p. 504).

## 10. Symbolism of Frege

Contemporarily with Peano and Russell lived the German logician Gottlob Frege (1848–1925). He was a creator of another symbolism, different from that of Peano and Russell. 'Different' means here not only the fact that it used other signs and symbols but that it was based on a completely different ideas. Frege's symbolism, unlike Boole's or Peano's, was not based on that of mathematics. For Frege the aim of logic was to provide foundations for arithmetic and in this way for the whole of mathematics. In this situation the use of an arithmetical symbolism in logic would create an ambiguity of symbolic expressions. Hence the logical symbolism should be based on another principles.

According to Frege all symbols have meaning. The meaning of a sign which is a subject in the linguistical sense is an object. In a symbolic language every expression must denote an object and one cannot introduce any symbol without meaning. Expressions of such a language denote either objects or functions. The word 'object' (*Gegenstand*) must be understood here in a narrower sense – it denotes a described individual, a class or the logical value (which means here "truth" (*das Wahre*) and "falsity" (*das Falsche*)). But sometimes Frege used the word 'object' more generally speaking about "saturated objects" (i.e., objects in a proper sense) and about "unsaturated objects" (i.e., functions). By calling functions "unsaturated objects" he meant that they are not completely defined. They have one or more unsaturated places. In particular, functions with one logical place whose values are truth values he called concepts, and functions with more unsaturated places – relations. Those functions were for him timeless and spaceless ideal objects.

The starting point of Frege's semantical considerations was the analysis of sentences in the logical sense, more exactly, analysis of signs for sentences. The sense of such a sign is the idea that something is so or not so. "Idea" is here for Frege not an individual act or result of an individual psychic operation. The idea does not need – as Frege writes – any human supporter. "Ideas are independent of thinking [...], they are not generated by thinking but grasped by it" (1879, p. 4). They exist in a similar way as real objects but – contrary to them – they are timeless and spaceless.

A sign for a sentence is an expression containing names, i.e., "saturated signs", in a predicative symbol. This predicative symbol (Frege says about "part" (*Theil*)) has its own meaning. Frege writes that this part being unsaturated has its counterpart in the world of meanings and this counterpart is called concept (*Begriff*). Hence according to Frege a concept is a truth-value function that is given by "unsaturated meaning" of the predicative part of a sentence.

Using the general notions of function and argument in his logical analysis of sentences Frege resigns from the old distinction between subject and predicate. Predicates are functions whose values are truth-values and subjects are their arguments – and according to this idea his formal language of arithmetic is constructed.

Frege introduced his symbolism in the book *Begriffsschrift, eine der arithmetischen nachgebildete Formelsprache des reinen Denkens* (1879). It contained the first formalized axiomatic system, namely the propositional calculus based on implication and negation (as the only connectives) and the first complete analysis of quantifiers as well as axioms for them. This work opened a new period in the history of formal logic; the year 1879 – the year of its publication – is considered as the most important date in the history of logic. The reception of the work by Frege's contemporaries was poor – they did not take it seriously or misunderstood it. Its meaning was recognized by Peano and Russell.

Frege's symbolism was two-dimensional. This was a departure from the usual practice – previously people have expressed (and we are doing so in our colloquial speech) their thoughts in a linear one-dimensional form. The idea of using a two-dimensional symbolism was revolutionary. It considerably extended the ways of expressing thoughts. But it turned out to be too revolutionary – and consequently it was not understood properly and not accepted.

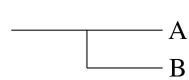
Since in the modern logic the Peano-Russell symbolism is commonly used today it may be useful to give here some account of Frege's notational ideas.

Frege begins the description of his symbolism by introducing a sign of contents and a sign of judgement. If *A* is a sign or a combination of signs indicating the contents of the judgement then the expression — *A* means: "the circumstance that *A*" or "the proposition that *A*", and the symbol $\vdash\!\!\!-\!\!\!-$ *A* expresses the judgement that *A*. In (1879) Frege wrote:

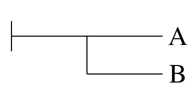
> Let $\vdash\!\!\!-\!\!\!-$ stand for (*bedeutet*) the judgement "opposite magnetic poles attract each other"; then —*A* will not express (*ausdrücken*) this judgement; it is to produce in the reader merely the idea of the mutual attraction of opposite magnetic poles, say in order to derive consequences from it and to test by means of these whether the thought is correct. (p. 2)

Hence the sign $\vdash\!\!\!-\!\!\!-$ is the common predicate for all judgements.

The symbol



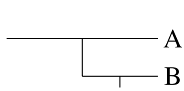
means: "the circumstance that the possibility that *A* is denied and *B* is affirmed does not take place". If we add to it the vertical stroke then we get



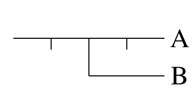
which stands for the judgement "the possibility that that *A* is denied and *B* is affirmed does not take place". Hence it is the symbol of the material implication "if *B* then *A*".

Negation was expressed by Frege by adding a small vertical stroke to the sign of contents —, hence by the symbol —┬— and ⊢┬— .

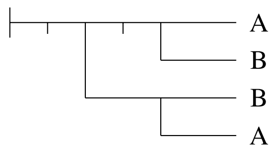
In Frege's symbolism there are no special symbols for further connectives. They were expressed by combinations of symbols for implication and negation. So in particular the disjunction "or" and the conjunction "and" were expressed, resp., as follows:



and

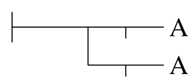


The equivalence is symbolized as follows:

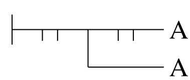



Let us write some more formulas in this symbolic language:

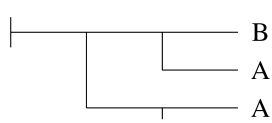
1. *tertium non datur* $A \vee \neg A$:



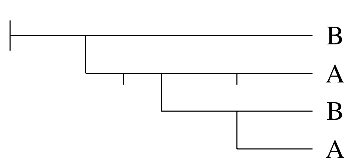

2. law of contradiction $\neg(A \wedge \neg A)$:



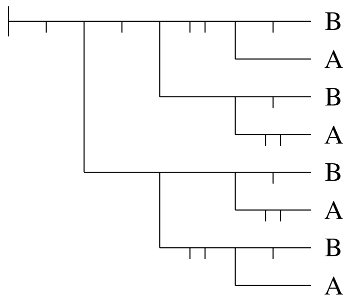

3. law of Duns Scottus $\neg A \rightarrow (A \rightarrow B)$:




4. *modus ponens* $(A \rightarrow B) \wedge A \rightarrow B$:

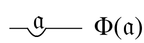



To indicate how much space does one need to write down formulas in Frege's symbolism and how complicated it sometimes is, let us write down in this language one of De Morgan's laws, namely the formula $\neg(A \wedge B) \longleftrightarrow (\neg A \vee \neg B)$:

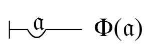



A further symbol introduced by Frege in (1879) was the symbol ≡ for the identity. The expression $\vdash\!\!-\!\!-\!\!-$ ($A \equiv B$) means that the signs $A$ and $B$ have the same conceptual contents and consequently in any context $A$ can be replaced by $B$ and $B$ by $A$. Hence the sign ≡ symbolizes the equivalence between sentences or the identity of objects.

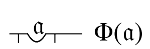
Frege introduced the universal quantifier $\forall\mathfrak{a}\Phi(\mathfrak{a})$ in the following way:




and

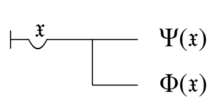



The existential quantifier is defined by the universal one and the negation in the following way:

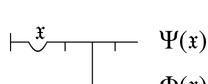



As an example let us write the classical categorical propositions in Frege's symbolic language:

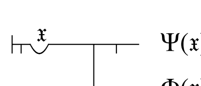
All Φ are Ψ:

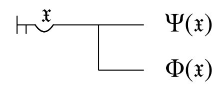



No Φ is Ψ:



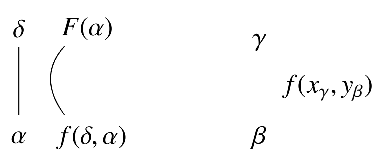

Some Φ are Ψ:



Some Φ are not Ψ: 

As we mentioned above Frege's idea of a two-dimensional notation turned out to be too revolutionary. One must admit that in fact this symbolism, in comparison with Peano's notation, was complicated and "hard". Frege himself (he had been in correspondence with Peano) acknowledged that Peano's symbolism was really simpler, more concise and convenient but simultaneously he wrote in a letter to him that this convenience "is not the highest good and aim" (1976, p. 246).

As indicated above Frege introduced symbols only for negation and implication as well as for the universal quantifier. In consequence one had to translate in an appropriate way propositions containing other connectives or the existential quantifier and by this translation their sense was usually obscured. Many symbols proposed by Frege did not emphasize and stress the sense of an expression, moreover, they often obscured it. As an example let us quote the following expressions:



which were read by Frege, resp., as: "the circumstance that the property $F$ is hereditary in an $f$-sequence" and "$y$ follows $x$ in an $f$-sequence".

It is clear today that Frege's symbolism could not survive – it was simply too monstrous. And we can only wonder that despite all the criticism Frege stood by it obstinately. Being convinced that he has invented things really new and much better than anything done so far by other logicians he depreciated them and disregarded their achievements (for example not referring to them). The result was that his own works had a very bad reception and were not accepted by his contemporaries. He was dismissed as a "gifted ignoramus" (as John Venn called him). As a consequence the symbolism proposed by Peano and Russell won and is used today.

## 11. Łukasiewicz's Notation

Some decades after Peano and Russell, new notation for the propositional calculus was proposed by Polish logician Jan Łukasiewicz (1878–1956). Its characteristic feature is that one does not use any parentheses in it and one writes connectives before their arguments. This notation is called today bracket-free or Łukasiewicz's notation, in USA one calls it Polish notation.

Łukasiewicz came to the idea of this notation – as he confessed – in 1924. The very idea to write connectives before their arguments came not from him but from Leon Chwistek (1884–1944) – Polish logician working in Cracow and Lvov. He talked about this idea in a lecture held in Warsaw at the beginning of the twenties. The principles of Łukasiewicz's notation go beyond the ideas of Chwistek and this justifies its name.

In Łukasiewicz's notation one denotes the connectives by capital Latin letters. In particular: $N$ denotes negation, $C$ – implication, $K$ – conjunction, $A$ – disjunction and $E$ – equivalence. So we write $Np$ instead of the usual $\neg p$, $Cpq$ instead of $p \to q$, $Kpq$ instead of $p \wedge q$, etc.

A translation of expressions with parentheses into Łukasiewicz's notation is not difficult. We explain this on two examples. Consider the law of Duns Scottus $p \to (\neg p \to q)$. One starts from the smallest subformula and moves step by step to longer subformulas. So we have: $Np$ for $\neg p$ and $CNpq$ for $\neg p \to q$. The law of Duns Scottus can be written now as:

$$CpCNpq.$$

As another example consider the formula

$$(\neg p \vee q) \to [q \to \neg(r \wedge \neg p)].$$

We write:

$$\begin{array}{rcl} \neg p \vee q & \text{as} & ANpq, \\ r \wedge \neg p & \text{as} & KrNp, \\ \neg(r \wedge \neg p) & \text{as} & NKrNp, \\ q \to \neg(r \wedge \neg p) & \text{as} & CqNKrNp \end{array}$$

and finally we obtain

$$CANpqCqNKrNp$$

as the translation of the whole formula.

It is clear that an expression in the Łukasiewicz's notation should begin by a capital letter denoting the main connective of it. Its argument(s) is (are) a variable or an expression that begins with a capital, etc. One can easily formulate a procedure deciding whether a given expression written in Łukasiewicz's notation is a well-founded formula or not.

The translation of expressions written in Łukasiewicz's notation into the usual notation using parentheses is also easy. Let us explain this again on an example. Consider the expression $CKCpqCqrCpr$. One starts from the right hand side and looks for the first connective. In our case it is $C$ whose arguments are $p$ and $r$. Hence we get $p \to r$ for $Cpr$. Moving to the left of the considered expression we have $C$ as the next connective – its arguments are $q$ and $r$. Hence we get $q \to r$. Similarly we get $p \to q$ for $Cpq$. Next connective to the left is $K$. Its arguments are two formulas that stand to the right of it, i.e., $Cpq$ and $Cqr$. Hence $KCpqCqr$ can be translated as: $(p \to q) \wedge (q \to r)$. Finally we obtain

$$[(p \to q) \wedge (q \to r)] \to (p \to r)$$

as the translation of the formula $CKCpqCqrCpr$.

What are the advantages of Łukasiewicz's notation? First of all the small number of signs needed to write formulas – one needs no parentheses, commas, dots nor special symbols for connectives as well as no rules deciding how is the structure of a formula. In fact in this notation the structure of a formula is simply determined by positions of letters denoting connectives. Łukasiewicz's notation have found also applications in the programming languages.

On the other hand this notation is rather clumsy and unhandy from the didactic point of view. Is this a consequence of our habits and of the fact that we use notation with parentheses in mathematics and treat it as a natural notation? Well, in the case of expressions of a short length both notation systems seem to be good, but when we consider expression of a "middle" length then usual notation using parentheses is better and in the case of very long expressions – Łukasiewicz's notation is better.

Notice that using Łukasiewicz's notation one can easily formulate problems concerning the propositional calculus – in fact this was just the reason why this notation was constructed. Logicians from the Warsaw Logic School (Łukasiewicz was one of its founder and main figure) studied various systems of the propositional calculus and formulated several conditions that should be satisfied by a good system. In particular a system was considered to be better when its axioms were independent from each other. Also other criteria were formulated, for example criteria concerning the number of axioms, their length (as formulas), they should be also "organic", i.e., no part (subformula) of an axiom could be a theorem of the considered system. All those conditions and demands can be formulated and studied easily and in an elegant way just using Łukasiewicz's notation.

### 12. Notations of Schönfinkel and Leśniewski

In this section we shall present two systems of notation that did not achieve acceptance but that are very interesting. We mean the notation proposed by Moses Iljitsch Schönfinkel (Scheinfinkel) (1884–1941?) and the notation of Stanisław Leśniewski (1886–1939).

Schönfinkel's proposal was formulated by him in a lecture held in Göttingen in 1920 – it has been prepared for publication by H. Behmann (cf. Schönfinkel

1924) who completed it. The aim of Schönfinkel was to reduce the number of logical constants. H.M. Sheffer has shown in (1913) that all propositional connectives can be defined by a single connective called today Sheffer's stroke and denoted as |, i.e., by the connective "not $p$ or not $q$". Schönfinkel has shown that all connectives and quantifiers of the predicate calculus can be in fact reduced to the formula $\forall x(\neg f(x) \vee \neg g(x))$. He wrote

$$f(x)|^x g(x) \qquad \text{for} \qquad \forall x(\neg f(x) \vee \neg g(x))$$

and introduced the function $\mathbf{U}$ for properties $f$, $g$ by the following definition:

$$\mathbf{U}(f, g) = \mathbf{U}fg = f(x)|^x g(x).$$

So for example $\neg a = a|^x a$ and

$$\forall x f(x) = \neg f(x)|^x \neg f(x) = (f(x)|^y f(x))|^x (f(x)|^y f(x)).$$

For $\exists x p(x)$ we get

$$(p(x)|^x p(x))|^y (p(x)|^x p(x)).$$

One can see that even "logically simple" expressions will appear in this notation rather difficult and unhandy. But the aim of Schönfinkel was not to propose a simple and handy notation system for mathematical usage but to construct a notation system which would make possible the development of logical expressions on the basis of few components. His considerations – and consequently also his symbolism – are going in this direction still further being led by (Schönfinkel 1924, pp. 306–307):

> the idea, which at first glance certainly appears extremely bold, of attempting to eliminate by suitable reduction the remaining fundamental notions those of proposition, propositional function, and variable, from those contexts in which we are dealing with completely arbitrary, logically general propositions (for others the attempt would obviously be pointless). [...] It seems to be remarkable in the extreme that [...] it can be done by a reduction to three fundamental signs.

This is really worth of noting and has a fundamental meaning for the construction of logical symbolism.

Schönfinkel stars by constructing a functional calculus with the aim to apply it later to expressions of the predicate calculus. He treats predicates – as Frege did – as functions. His calculus describes the basic relations and operations between functions (including constants) through "*individual functions* of a very general nature" – as he says in (1924, p. 309).

$\mathbf{I}$ is the "identity function" given by $\mathbf{I}x = x$. Its aim is to enable us to treat any constant $x$ as a value of a function $\mathbf{I}x$. $\mathbf{C}$ is the "constancy function" which associates with any constant $a$ the constant function $\mathbf{C}a$ having constant value $a$:

$$\mathbf{C}a(y) = a.$$

For any $x$ one has $Cx(y) = x$. Schönfinkel adopts the rule that if there are no parentheses then functions are applied from left to right. Hence

$$\mathbf{C}xy = x.$$

Its aim is to introduce $y$ "as a 'blind' variable" in expression in which $y$ does not occur. The "interchange function" **T** is introduced by the following definition:

$$(\mathbf{T}\varphi)xy = \varphi yx.$$

The "composing function" **Z** is defined in the following way:

$$\mathbf{Z}\varphi\chi x = \varphi(\chi x).$$

It makes possible to move parentheses in expressions in which functions occur. Finally **S** is the "merging function":

$$\mathbf{S}\varphi\chi x = \varphi x(\chi x)$$

where $\varphi x$ is again a function. As an example Schönfinkel gives here a binary function $\varphi xy$ in which for the second argument one puts the value $\chi x$ of another function on the same argument $x$. The function **S** makes possible to reduce the number of occurrences of a variable in an expression.

We shall apply those "individual functions" on a simple example and show the interesting result. Before this we notice – following Schönfinkel – that all indicated individual functions can be developed using only two of them. For example

$$\mathbf{I}x = x = \mathbf{C}x(\mathbf{C}x) = \mathbf{SCC}x,$$

$$\mathbf{I} = \mathbf{SCC}.$$

Or

$$\mathbf{Z}fgx = f(gx) = \mathbf{C}fx(gx) = \mathbf{S}(\mathbf{C}f)gx = \mathbf{CS}f(\mathbf{C}f)gx = \mathbf{S}(\mathbf{CS})\mathbf{C}fgx,$$

i.e.,

$$\mathbf{Z} = \mathbf{S}(\mathbf{CS})\mathbf{C}.$$

In a similar way one gets

$$\mathbf{T} = \mathbf{S}(\mathbf{ZZS})(\mathbf{CC}).$$

Consider now the following example:

$$\forall x(\neg p(x) \vee \exists x p(x)) =$$

$$= \forall x(\neg p(x) \vee \neg\forall x(\neg p(x) \vee \neg p(x)) = \forall x(\neg p(x) \vee \neg(p(x)|^x p(x))) =$$

$$= p(x)|^x(p(x)|^x p(x)) = \mathbf{U}p(\mathbf{U}pp) = \mathbf{U}p(\mathbf{U}p(\mathbf{I}p)) = \mathbf{U}p(\mathbf{SUI}p) =$$

$$= \mathbf{SU}(\mathbf{SUI})p.$$

If one expresses **I** by **C** and **S** then in the considered expression only **U**,**C** and **S** will occur. The variable $p$ can be disregarded. Schönfinkel says that "a variable in a proposition of logic is, after all, nothing but a token (*Abzeichen*) that characterizes certain argument places and operators as belonging together" (1924, p. 307). At the end of an expression one does not need this "token". Moreover, one can further transform logical expression in such a way that **U** will occur at the very end. This was observed by H. Behman at the end of (1924) – we show this on our example. One gets:

$$\mathbf{SU}(\mathbf{SUI})p = \mathbf{SU}(\mathbf{SU}(\mathbf{CIU}))p = \mathbf{SU}(\mathbf{SS}(\mathbf{CI})\mathbf{U})p =$$
$$= \mathbf{SS}(\mathbf{SS}(\mathbf{CI}))\mathbf{U}p.$$

By an appropriate transformation one can eliminate also **U**. In this way one sees that any formula can be written using only two signs, namely **S** and **C**. In our example one gets:

$$\mathbf{SS}(\mathbf{SS}(\mathbf{CI})) = \mathbf{SS}(\mathbf{SS}(\mathbf{C}(\mathbf{SCC}))).$$

This reduction is – according to Schönfinkel – really "noteworthy" and characteristic for logical expressions and the operating in logical systems. One obtains a reduction solely by manipulations and combinations of signs and sequences of signs which do not occur themselves and in this way discover the purely formal character of objects of logical operations.

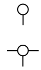
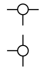
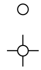
Schönfinkel's work was the beginning of the so called combinatorial logic developed by H.B. Curry (cf. Curry 1930). This logic describes and explains in an axiomatic way how one should operate on combinators – as Curry calls the "individual functions".

Finally let us consider the notation system proposed by Polish logician and philosopher Stanisław Leśniewski – it is of quite different nature and was developed to fulfil quite different aims. It should provide us with an ideographical notation for propositional connectives.

Leśniewski proposed as a symbol for a unary connective a horizontal line with an appropriate vertical one:

for affirmation $\relbar\joinrel\relbar\joinrel\dashv$ ,
for negation $\vdash\joinrel\relbar\joinrel\relbar$ ,
for tautology $\vdash\joinrel\relbar\joinrel\dashv$ ,
for contradiction $\relbar\joinrel\relbar\joinrel\relbar$ .

Symbols for binary connectives are constructed by using a circle and radius arms. In particular one has:

 denotes conjunction (and),
 denotes disjunction (or),
 denotes implication (if ... then ... ),
 denotes equivalence (if and only if),
$\circ$ denotes the contradictory (always false),
 denotes tautology (always true).

The main principle of this system of notation can be described as follows: suppose that the combinations of truth-values $t$ (true) and $f$ (false) or, resp. 1

and 0 of the arguments of a binary propositional connective are written in the following matrix:

$$\begin{array}{ccc} & 00 & \\ 01 & \circ & 10 \\ & 11 & \end{array}$$

One adds now a radius arm at the places where the given connective has value 1. So for example the connective

indicates that its value is 0 only in the case when both its arguments are 1. Hence it is Sheffer's stroke:

$a$  $b = \neg a \vee \neg b = a|b.$

The notation of Leśniewski is called "hun-and-spoke-notation", "wheel-notation" or "o clock notation". It could be also called "slice-notation", "matrix-notation" or "signal-notation". One can easily see that every symbol of a propositional connective represents here its own matrix just by a semaphore-code or signal-code. The basic truth-value properties of a connective are directly indicated by the symbol. Hence this notation system makes possible to solve immediately any problem which can be solved using truth-value schemes. This system is didactically skillful as any other system which connects the symbolic level and the level of meaning.

## 13. Symbolic Mania and Pragmatic Phobia

Having discussed Łukasiewicz and Leśniewski (two important representatives of Polish logic) and their contribution to the development of logical symbolism we should not forget Kazimierz Twardowski (1866–1938), the founder of the Lvov-Warsaw philosophical school. Though he was not a logician but it is worth presenting here his general remarks concerning the logical and mathematical symbolism.

Twardowski appreciated the rôle and significance of a good symbolism in logic but simultaneously warned against its overestimation. Though he himself was not a logician and did not work in logic he knew the works of his students and followers, Łukasiewicz and Leśniewski, and their attempts to identify clarity with the use of a symbolic language. He was not against the "new logic" but stood in opposition to attitudes which were genetically connected to symbolic logic, which he described as symbolic mania and pragmatic phobia. According to him a symbol represents an object but cannot replace it. On the contrary, a symbol is always only a tool. If we forget these two things we have the attitude Twardowski called symbolic mania. It is characterized by a faith in the infallibility of symbolism, in an autonomy of operations on symbols and by a condemnation of opinions which are independent of any symbolism. This attitude is strongly connected with another, which Twardowski called pragmatic phobia. It consists of bias against objects denoted by symbols. Both tendencies can be found in logic and mathematics. Twardowski quotes in (1927) a vivid picture of a mathematician (taken from Bouasse 1920) suffering under them.

The development of symbolic mania and pragmatic phobia is stimulated by the fact that symbolism is a good tool which affords possibilities for clarity and precision; and by the fact that in mathematics and logic the objects which are studied, hence "things" denoted by logical and mathematical symbols, are usually mental constructions. These attitudes can have negative consequences. Twardowski wrote in (1927):

> The tendency of putting symbols above objects can lead to the situation that one claims about objects only that which follows from assumptions about symbolic operations regardless of what things say about themselves or even in the face of facts. (p. 398)

One should stress that Twardowski was not averse to the use of symbolic language in logic and mathematics. On the contrary – he estimated it at its proper value but warned against overestimation of it. Twardowski's views were fully accepted by the Lvov-Warsaw logical school. Its representatives proclaimed the thesis that a symbol represents an object and the thesis on semantic intension and semantic transparency of a symbol.

## 14. A Survey of Nowadays Used Notation Systems

The symbolic used today is in fact that proposed by Peano with some small changes made by Russell. It is the consequence of its clarity, clearness and simplicity. Important was also the fact that Whitehead and Russell used it in their monumental work *Principia Mathematica* – it was written almost exclusively (with the exception of some necessary explanations in English) in a symbolic language.

Changes in Peano's symbolic proposed by Russell and other logicians consisted in changing of the shape of particular symbols. To provide the reader with some information about symbols used nowadays in logic we give a survey of symbols applied in the propositional calculus and in the predicate logic.

| connective | PM | HA, HB | HS | DIN | other symbols |
|---|---|---|---|---|---|
| negation | $\sim p$ | $\overline{p}$ | $\overline{p}$ | $\neg p$ | $p', -p$ |
| conjunction | $p \cdot q$, $pq$ | $p\&q$ | $p \wedge q$ | $p \wedge q$ | |
| disjunction | $p \vee q$ | $p \vee q$, $pq$ | $p \vee q$ | $p \vee q$ | $p + q$ |
| implication | $p \supset q$ | $p \rightarrow q$ | $p \rightarrow q$ | $p \rightarrow q$ | |
| equivalence | $p \equiv q$ | $p \sim q$ | $p \leftrightarrow q$ | $p \leftrightarrow q$ | |

PM is an abbreviation for *Principia Mathematica*, HA and HB denote, resp., *Grundzüge der theoretischen Logik* by Hilbert and Ackermann (1928) and *Grundlagen der Mathematik* by Hibert and Bernays (1934, 1939), HS denotes the Münster school around Hermes and Scholz and DIN – Deutsche Institut für Normierung (DIN 5473, 1992 – ISO 31 – 11:1978 International Organization for Standardization).

In the predicate logic (of the first and higher orders) the following symbols are used:

| | universal quantifier | existential quantifier |
|---|---|---|
| Whitehead, Russell | $(x)$ | $(\exists x)$ |
| Hilbert, Ackermann | $(x)$ | $(\mathrm{E}x)$ |
| Hilbert, Bernays | $(x)$ | $(\mathrm{E}x)$ |
| Kleene | $\forall x$ | $\exists x$ |
| Quine | $(x)$ | $(\exists x)$ |
| Rosser | $(x)$ | $(\mathrm{E}x)$ |
| others | $\bigwedge_x, \bigwedge\limits_x, \bigwedge x$, | $\bigvee_x, \bigvee\limits_x, \bigvee x$, |
| | $\prod_x, \prod x, \bigcap_x$ | $\sum_x, \sum x, \bigcup_x$ |
| DIN | $\forall x$ | $\exists x$ |
| most often used | $\forall x, \bigwedge\limits_x$ | $\exists x, \bigvee\limits_x$ |

[Source: Whitehead/Russell 1910–1913, Hilbert/Ackermann 1928, Hilbert/Bernays 1934 and 1939, Kleene 1952, Quine 1940, Rosser 1953.]

Note that Russell and Whitehead beside $(x)$ used – in the case when a universal quantifier stood at the very beginning of a formula which was an implication – Peano's notation $\ldots \supset_x \ldots$

In our opinion the most consequent is the following notation system: $\neg, \wedge, \vee, \rightarrow, \leftrightarrow, \bigwedge, \bigvee$. It comes from Alfred Tarski (1901–1983). The symbol $\vee$ for disjunction is similar to the first letter of Latin *vel* (that means "or"), the symbol $\wedge$ is the inverse of $\vee$ (as conjunction is the inverse of disjunction). In this way the duality between disjunction and conjunction is stressed. The symbols for quantifiers $\bigwedge$ and $\bigvee$ indicate that the universal quantifier is a generalization (in a certain sense) of the conjunction and the existential quantifier – a generalization of the disjunction.

As indicated above symbols for quantifiers used most often nowadays are $\forall x$ and $\exists x$ (they are used also in LaTeX, the most popular editor for logicians and mathematicians). Those signs resemble, resp., letters A (all) and E (exists).

## 15. Conclusions

To conclude we can state that the development of a symbolic language for logic was always connected with some philosophical assumptions concerning language (in particular the language of science). We can say that one of the most important aims which was a guiding principle for logicians, mathematicians and philosophers in constructing such a symbolism was the idea of establishing an exact, ideal and precise language which would replace the unclear and imprecise colloquial language. There arose problems about the connections between language and reality. We see here a rich variety of solutions. One can also spot a tendency which – starting from the linguistical inclination to reduce logic to the study of properties of a language – leads at the very end to the identification of logic with the theory of signs. (One observes this clearly comparing ideas of Russell, Wittgenstein and Carnap.)

Notice at the end that a logical symbolism is an example of an ideography (i.e., of a writing in which symbols denote concepts and not sounds). In such a system expressions are understood in the same way by all knowing the basic signs though they can be read in different way by various people. So is the case in logic and mathematics. For example the formula $\neg p \rightarrow q$ is understood in the same way by all who know the elements of the propositional calculus though it will be read in a different way by people of various nations: in particular a Polish logician will read it as "jeżeli $p$, to $q$", an English logician – as "if $p$ then $q$", a German logician – as "wenn $p$ dann $q$", etc.

The symbolic language of logic (and mathematics) is an example of an (almost uniform) ideographical system accepted all over the world. It not only facilitates communication among logicians and mathematicians but is also a sign of the universal character of those disciplines.

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# NAME INDEX