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Bernays Project: Text No. 1

Hilbert's significance for the philosophy of mathematics (1922)

Die Bedeutung Hilberts für die Philosophie der Mathematik

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If we consider the intellectual relationship of the mathematical sciences to philosophy, as they have developed since the time of the Enlightenment, we notice with satisfaction that mathematical thought is now at the point of regaining the powerful influence on philosophical speculation that it had up to Kant's time, but then suddenly lost completely. That sudden turn from mathematical thought was influenced by the general turn from the spirit of the Enlightenment period that took place at the beginning of the nineteenth century.

This detachment of philosophy from the exact sciences was, however, only a unilateral one: While the dominant philosophy became completely estranged[1] from mathematics, a philosophical orientation evolved more and more among mathematicians.

The most important reason for this was that mathematics had grown far beyond the framework within which it had moved up to the time of Kant. Not only had the domain of investigated facts grown considerably, but the whole form of the investigations became grander and the entire method more encompassing. The concept-formations rose to a higher level of generality; the meaning of the formula became less important than conceptual abstractions and leading systematic ideas. Furthermore, the attitude toward the foundations and toward the object of the mathematical sciences also changed.

The task of geometry was understood in broader terms. Formation of geometrical concepts became more general and freed themselves more and more from the connection to spatial representation. In the recently developed geometrical theories, moreover, intuition of space no longer had the significance of an epistemological foundation, but was rather employed merely in the sense of an intuitive analogy.

In arithmetic, research experienced an essential extension of the formulation of problems as well. On the one hand, the concepts of number and of order were generalized, through the invention of set theory, in a completely new way and applied to infinite totalities. On the other hand, the development of algebra led to numbers and quantities no longer being exclusively viewed as the objects of investigation; rather, the formalism of calculation itself was made an object of study, and one made, very generally, the consideration of formalisms one's task. Numbers as well as quantities now appeared only as something special, and the more one examined their lawfulness from more general points of view, the more one was weaned away from taking this lawfulness for granted.

In this way, the whole development of mathematics moved on: to rob of its appearance of exclusiveness and finality all that which had previously been considered to be the only object of research, and whose basic properties were considered as something to be accepted by mathematics and neither capable nor in need of mathematical investigation. The framework which earlier philosophical views, and even Kantian philosophy, had marked out for mathematics was spring. Mathematics no longer allowed philosophy to

[1]Among those philosophers who represented in this respect a laudable exception, Bolzano must be mentioned in particular; he gave the first rigorous foundation of the theory of real numbers.

prescribe the method and the bounds of its research; rather it took the discussion of its methodological problems into its own hands. In this way the axioms of the mathematical theories were investigated in regard to their logical relationships, and the forms of inference were subjected to more precise critique as well. And the more these problems have been pursued, the more mathematical thought has shown its fruitfulness with respect to them, and it has proved itself as an indispensable tool for theoretical philosophy.

To this development, which extends to the present, David Hilbert has contributed isignificantly. What he has accomplished in this field will be described in what follows.

When Hilbert applied himself to the problems that were to be solved concerning the foundations of mathematical thought, he not only had at his disposal his comprehensive command of the mathematical methods, but he was also above all, as it were, predestined for the task by his human disposition. For mathematics had for him the significance of a world view, and he approached those fundamental problems with the attitude of a conqueror who endeavors to secure for mathematical thought a sphere of influence which is as comprehensive as possible.

The main point, while pursuing this goal, was to avoid the mistake of those extreme rationalist thinkers who thought that complete knowledge of everything real could be attained by pure thought. There could be no question of, say, incorporating into mathematics all knowledge of the factual; rather it was necessary, for extending the realm of mathematics in the widest possible way, to delineate sharply the boundaries between the mathematical and the nonmathematical. That would actually allow one to claim for mathematics all mathematical components of knowledge.

In fact, Hilbert also understood the problem in this way. His first and largest work in the field of methodological questions is the *Foundations of Geometry*, which appeared in 1899. In this work, Hilbert laid out a new system of axioms for geometry that he chose according to the criteria of simplicity and logical completeness, following Euclid's concept-formations as closely as possible. He divided the whole system of axioms into five groups of axioms and then investigated more precisely the share the different groups of axioms (as well as single axioms) have in the logical development of geometry.

Through its wealth of new, fruitful methods and viewpoints, this investigation has exercised a powerful influence on the development of mathematical research. However, the significance of Hilbert's foundations of geometry by no means lies only in its purely mathematical content. Rather, what made this book popular and Hilbert's name renowned, far beyond the circle of his colleagues, was the new methodological turn that was given to the idea of axiomatics.

The essence of the axiomatic method, i. e., the method of logically developing a science from axioms and definitions, consists according to the familiar conception in the following: One starts with a few basic propositions, of whose truth one is convinced, puts them as axioms at the top, and derives from them by means of logical inference theorems; their truth is then as certain as that of the axioms, precisely because they follow logically from the axioms. In this view, attention is focused above all on the epistemic character of the axioms. Indeed, originally one considered as axioms only propositions whose truth was evident *a priori*. And Kant still held the view that the success and the fruitfulness of the axiomatic method in geometry and mechanics essentially rested on the fact that in these sciences one could proceed from *a priori* knowledge (the axioms of pure intuition and the principles of pure understanding).

Of course, the demand that each axiom expresses an *a priori* knowable truth was soon abandoned. For on the manifold occasions that presented themselves for the axiomatic method, especially in the further development of physics, it followed, so to speak, automatically that one chose both empirical statements and also mere hypotheses as axioms of physical theories. The axiomatic procedure turned out to be especially fruitful in cases where one succeeded in encompassing the results of multifarious experiences in a statement of general character through positing an axiom. A famous example of this is that of the two propositions about the impossibility of a *perpetuum mobile* of the first and second type ; Clausius put them as axioms at the top of the theory of heat.

In addition, the belief in the *a priori* knowledge of the geometrical axioms was increasingly lost among the researchers in the exact sciences, mainly as a consequence of non-Euclidian geometry and under the influence of Helmholtz' arguments. Thus the empirical viewpoint, according to which geometry is nothing but an empirical science, found more and more supporters. However, this abandonment of *a prioricity* did not alter essentially the perspective on the axiomatic method.

A more powerful change, however, was brought about by the systematic development of geometry. Starting out from elementary geometry, mathematical abstraction had raised itself far above the domain of spatial intuition; it had led to the construction of comprehensive systems, in which ordinary

Euclidian geometry could be incorporated and within which its lawlikeness appeared only as a special case among others with equal mathematical justification. In this way, a new sort of mathematical speculation opened up, by means of which one could consider the geometrical axioms from a higher standpoint. It immediately became apparent, however, that this type of consideration had nothing to do with the question of the epistemic character of the axioms, which of course had formerly been considered as the only significant feature of the axiomatic method. Accordingly, the necessity of a clear separation between the mathematical and the epistemological problems of axiomatics ensued. The demand for such a separation of the problems had already been stated with full clarity by Klein in his Erlangen Program.[2]

What was essential, then, about Hilbert's foundation of geometry was that here, from the beginning and for the first time, in the laying down of the axiom system, the separation of the mathematical and logical realm from the spatial-intuitive realm, and thereby from the epistemological foundation of geometry, was carried out completely and brought to rigorous expression.

To be sure, in the introduction to his book Hilbert does express the thought that laying down the axioms for geometry and the investigation of their relationships is a task that amounts "to the logical analysis of our spatial intuition," and likewise he remarks in the first section that each single of these groups of axioms expresses "certain basic facts of our intuition which belong together."[a] But these statements are located completely outside the axiomatic development, which is carried out without any reference to spatial intuition.

A strict axiomatic grounding of geometry has of course always to satisfy the demand that the proofs should exclusively appeal to what is formulated in the axioms, and that they not draw, in any way, on spatial intuition. In recent times, it was especially Pasch who, in his foundation of geometry,[3] emphazised the importance of this requirement and has done so in a consistent way.

However, Hilbertian axiomatics goes even one step further in the elimination of spatial intuition. Drawing on spatial representation is completely

[2] *Vide* [**?**].
[3] *Vide* [**?**].

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[a] *Vide* [**?**], pp. ■ , ■ .

avoided here, not only in the proofs but also in the axioms and the concept-formations. The words "point," "line," "plane" serve only as names for three different sorts of objects, about which nothing else is directly assumed except that the objects of each sort constitute a fixed determinate system. Any further characterization, then, follows from the axioms. In the same way, expressions like "the point A lies on the line a" or "the point A lies between B and C" are not associated with the usual intuitive meanings; rather these expressions will designate only certain, at first indeterminate, relations, which *are implicitly characterized*[4] only through the axioms in which these expressions occur.

In consequence of this conception, the axioms are in no way judgments, that can be said to be true or false; after all, they have a sense only in the context of the whole axiom system. And even the axiom system as a whole does not state a truth; rather, the logical structure of axiomatic geometry in Hilbert's sense—completely analogous to that of abstract group theory—is a purely hypothetical one. If there are anywhere in reality three systems of objects, as well as determinate relations between these objects, such that the axioms of geometry hold of them (this means that by an appropriate assignment of names to the objects and relations, the axioms turn into true assertions), then all theorems of geometry hold of these objects and relations as well. Thus the axiom system itself does not express something factual; rather, it presents only a possible form of a system of connections that must be investigated mathematically according to its *internal* properties.

Accordingly, the axiomatic treatment of geometry amounts to separating the purely mathematical part of knowledge from geometry, considered considered as a science of spatial figures, and investigating it in isolation on its own. The spatial relationships are, as it were, mapped into the sphere of the mathematical-abstract in which the structure of their interconnections appears as an object of pure mathematical thought. This structure is subjected to a mode of investigation that concentrates only on the logical relations and is indifferent to the question of the *factual* truth, that is, the question whether the geometrical connections determined by the axioms are found in reality (or even in our spatial intuition).

This sort of interpretation, which the axiomatic method in Hilbert's *Foundations of Geometry* presented, offered the particular advantage of not being

[4]One speaks in this sense of "implicit definition."

restricted to geometry but indeed of being transferable to other disciplines without further ado. From the beginning, Hilbert envisaged the point of view of the uniformity of the axiomatic method in its application to the most diverse domains, and guided by this viewpoint, he tried to bring this method to bear as widely as possible. In particular, he succeeded in grounding axiomatically the kinetic theory of gases as well as the elementary theory of radiation in a rigorous way.

In addition, many mathematicians subscribed to Hilbert's axiomatic mode of investigation and worked in the spirit of his endeavors. In particular, it was a success of axiomatics when Zermelo, in the field of set theory, overcame the existing uncertainty of inference by a suitable axiomatic delimitation of the modes of inference and, at the same time, created with his system a common foundation for number theory, analysis, and set theory.[5]

In his Zurich lecture on "■ Axiomatic Thought" (1917),[6] Hilbert gave a summary of the leading methodological thoughts and an overview of the results of research in axiomatics. Here he characterizes the axiomatic method as a general procedure for scientific thinking. In all areas of knowledge, in which one has already come to the point of setting up a theory, or, as Hilbert says, to an arrangement of the facts by means of a framework of concepts, this procedure sets in. Then, it always becomes obvious that a few propositions suffice for the logical construction of the theory, and through this the axiomatic foundation of the theory is made possible. This will at first take place in the sense of the old axiomatics. However, one can always—as within geometry—move on to Hilbert's axiomatic standpoint by disregarding the epistemic character of the axioms and by considering the whole framework of concepts only (as a *possible* form of a system of interconnected relationships) in regard to its internal structure.

Thus, the theory turns into the object of a purely mathematical investigation, just what is called an *axiomatic* investigation. And, to be sure, the same principle questions must always be considered for any theory: First of all, in order to represent a possible system of interconnected relations, the axiom system must satisfy the condition of *consistency*; i. e., the relations expressed in the axioms must be logically compatible with one another. Consequently, the task of proving the consistency of the axiom system arises—a problem with which the old conception of axiomatics was not acquainted, since here

[5] *Vide* [**?**].
[6] *Vide* [**?**].

indeed every axiom counts as stating a truth. Then comes the question of gaining an overview of the logical *dependencies* among the different theorems of the theory. A particular focus of investigation must be on whether the axioms are logically independent of each other or whether, say, one or more axioms can be proved from the remaining ones and are thus superfluous in their role as axioms. In addition, there remains the task of investigating the possibilities of a "deepening of the foundations" of the theory, i. e., examining whether the given axioms of the theory might not be reduced to propositions of a more fundamental character that would then constitute "a deeper layer of axioms" for the framework of concepts under consideration.[b]

This sort of investigation, which is of a mathematical character throughout, can now be applied to any domain of knowledge that is at all suitable for theoretical treatment, and its execution is of the highest value for the clarity of knowledge and for a systematic overview. Thus, through the idea of axiomatics, mathematical thought gains a universal significance for scientific knowledge. Hilbert can indeed claim: "Everything whatsoever, that can be the object of scientific thought is subject, as soon as it is ripe for the formation of a theory, to the axiomatic method and thereby to mathematics."[c]

Now, by means of this comprehensive development of the axiomatic idea, a sufficiently wide framework for the mathematical formulation of problems was indeed obtained, and the epistemological fruitfulness of mathematics was made clear. But with regard to the *certainty* of the mathematical procedure, a fundamental question still remained open.

Namely, the task of proving the consistency of the axiom system was indeed recognized as first and foremost in the axiomatic investigation of a theory. In fact, the consistency of the axioms is the vital question for any axiomatic theory; for whether the framework of concepts represents a system of interconnected relations at all or only the appearance of such a system depends on this question.

If we now examine how things stand with the proof of consistency for the several geometrical and physical theories that have been axiomatically grounded, then we find that this proof is produced in every case only in a relative sense: The consistency of the axiom system to be investigated is proved by exhibiting a system of objects and of relations *within mathematical analysis* that satisfies the axioms. This "method of reduction" to analysis

[b] *Vide* [**?**], p. ■ .
[c] *Vide* [**?**], p. ■ .

(i. e., to arithmetic in the wider sense) presupposes that analysis itself—independently of whether it is considered as a body of knowledge or only as an axiomatic structure (i. e., as a merely possible system of relations)—constitutes a consistent system.

However, the consistency of analysis is not as immediately evident as one would like to think at first. The modes of inference applied in the theory of real numbers and real functions do not have that character of tangible evidence which is characteristic, for instance, of the inferences of elementary number theory. And if one wants to free the methods of proof from everything that is somehow problematic, then one is compelled to axiomatically set up analysis. Thus it turns out to be necessary to provide also a consistency proof for analysis.

From the beginning, Hilbert recognized and emphasized the need for such a proof to guarantee the certainty of the axiomatic method and of mathematics in general. And although his efforts concerning this problem have not yet reached the ultimate goal, he has nonetheless succeeded in finding the methodological approach by which the task can be mathematically undertaken.

Hilbert presented the main ideas of this approach already in 1904 in his Heidelberg lecture "On the Foundations of Logic and Arithmetic."[7] However, this exposition was difficult to understand and was subject to some objections. Since then, Hilbert has pursued his plan further and has given a comprehensible form to his ideas, which he recently presented in a series of lectures in Hamburg.

The line of thought on which Hilbert's approach to the foundations of arithmetic and analysis is based is the following: The methodological difficulties of analysis, on the basis of which in this science one is compelled to go beyond the framework of what is concretely representable, result from the fact that here continuity and infinity play an essential role. This circumstance would also constitute an insuperable obstacle to the consistency proof for analysis, if this proof had to be carried out by showing that a system of things as assumed by analysis, say the system of all finite or infinite sets of whole numbers, is logically possible.

However, the claim of consistency needs not at all be proved in this way. Rather, one can give the following entirely different twist to the claim: The

[7]Appendix VII to [**?**].

modes of inference of analysis can never lead to a contradiction or, what amounts to the same thing: It is impossible to derive the relation $1 \neq 1$ ("1 is not equal to 1") from the axioms of analysis and by means of its methods of inference. Here it is not a question concerning the possibility of a continuous, infinite manifold of certain properties, but concerning the impossibility of a mathematical proof with determinate properties. A mathematical proof is, however, unlike a continuous infinite manifold, a concrete object surveyable in all its parts. A mathematical proof must, at least in principle, be completely communicable from beginning to end. Moreover, the required property of the proof (i. e., that it proceeds according to the principles of analysis and leads to the final result $1 \neq 1$) is also a concretely determinable property. This is why there is also, in principle, the possibility of furnishing a proof of consistency for analysis by means of elementary, and evidently certain, considerations. We only have to take the standpoint that the object of investigation is not constituted by the objects to which the proofs of analysis refer, but rather these proofs themselves.

On the basis of this consideration, the task arises then for Hilbert to examine more precisely the forms of mathematical proofs. We must, so he says in his lecture on axiomatic thought, "make the concept of specific mathematical proof itself the object of an investigation, just as the astronomer takes into account the motion of his location, the physicist concerns himself with the theory of his apparatus, and the philosopher criticizes reason itself."[d] The general forms of logical inference are decisive for the structure of mathematical proofs. That is why the required investigation of mathematical proofs must, in any case, also concern the logical forms of inference. Accordingly, already in the Heidelberg lecture, Hilbert explained that "a partially simultaneous development of the laws of logic and arithmetic [is] necessary."

With this thought Hilbert took up *mathematical logic*. This science, whose idea goes back to Leibniz, and which, in the second half of the nineteenth century developed from primitive beginnings into a fruitful field of mathematical thought, has developed the methods for achieving a mathematical mastery of the forms of logical inference through a symbolic notation for the simplest logical relations (as "and," "or," "not," and "all"). It turned out that by this "logical calculus" only one gains the complete overview of the system of logical forms of inference. The inferential figures, which are

[d] *Vide* [**?**], p. ■ .

dealt with in traditional logic, constitute only a relatively small subfield of this system. Peano, Frege, and Russell in particular, succeeded in developing the logical calculus in such a way that the mental inferences of mathematical proofs can be perfectly imitated by means of symbolic operations.

This procedure of the logical calculus forms a natural supplement to the method of the axiomatic grounding of a science to the following extent: It makes possible, along with the exact determination of the *presuppositions*—as it is brought about by the axiomatic method—also an exact pursuit of the *modes of inference* by which one proceeds from the principles of a science to its conclusions.

In adopting the procedure of mathematical logic, Hilbert reinterpreted it as he had done with the axiomatic method. Just as he had formerly stripped the basic relations and axioms of geometry of their intuitive content, he now eliminates the intellectual content of the inferences from the proofs of arithmetic and analysis which he takes as the object of his investigation. He achieves this by taking the systems of formulas, by which those proofs are represented in the logical calculus—detached from their contentual-logical interpretations—, as the immediate object of consideration, and thereby replacing the proofs of analysis with a purely formal manipulation of definite signs according to fixed rules.

Through this mode of consideration, in which the separation of the specifically mathematical from everything contentual reaches its high point, Hilbert's view on the nature of mathematics and on the axiomatic method finally finds its real completion. For we recognize at this point that that sphere of the abstract mathematical, into which the method of thought of mathematics translates all that is theoretically comprehensible, is not the sphere of logical contentual but rather the domain of pure formalism. Mathematics turns out to be the general doctrine of formalisms, and by understanding it as such, its universal significance also becomes immediately.

This meaning of mathematics as a general doctrine of forms has come to light in recent physics in a most splendid way, especially in Einstein's theory of gravitation, in which the mathematical formalism gave Einstein the leading idea for setting up his law of gravitation, whose more exact form could never have been found without enlisting mathematical tools. And here it was once again Hilbert who first brought this law of gravitation to its simplest mathematical form. And by showing the possibility of a harmonious combination of the theory of gravitation with electrodynamics, he initiated the further mathematical speculations connected to Einstein's theory which were brought to systematic completion by Weyl with the help of his geometrical ideas. If these speculations should stand the test in physics, then the triumph of mathematics in modern science would thereby be a perfect one.

If we now look at the ideas yielded by Hilbert's philosophical investigations as a whole, as well as the effect brought about by these investigations, and if we, on the other hand, bear in mind the unfolding of mathematics in the recent times as outlined above, thenthe essence of Hilbert's philosophical accomplishment reveals itself. By developing a sweeping philosophical conception of mathematics, which does justice to the significance and scope of its method, Hilbert has succeeded in forcefully establishing the claim that mathematics, through its depth and profundity, has a universal intellectual influence throughout the sciences. For this, the friends of mathematics will be always be indebted to him.

Chapter 2

Bernays Project: Text No. 2

On Hilbert's thoughts concerning the founding of arithmetic (1922)

Über Hilberts Gedanken zur Grundlegung der Arithmetik

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Hilbert's new methodological approach for the founding of arithmetic, which I will discuss, is a modified and more definite version of the plan that Hilbert already had in mind for a long time and that he first expressed in his Heidelberg lecture. The previous, quite obscure hints have been replaced by a sharply outlined and comprehensible program, the beginnings of which have already been carried out.

The problem whose solution we are seeking here is that of proving the consistency of arithmetic. First we have to recall how one arrives at the formulation of this problem.

The development of arithmetic (in the wider sense, thus encompassing analysis and set theory), as it has been done since the introduction of rigorous methods, is an *axiomatic* one. This means that, as in the axiomatic grounding of geometry, one begins by assuming a *system of things* with certain relational properties. In Dedekind's foundations of analysis the basis is taken to be the system of elements of the continuum, and in Zermelo's construction of set theory, it is the domain $\mathfrak{B}$. Also in the grounding of analysis that starts by considering number sequences, the number series is conceived of as a closed, surveyable system, akin to an infinite piano keyboard.

In the assumption of such a system with certain relational properties lies something, as it were, *transcendent* for mathematics, and the question arises as to which fundamental position one should take regrading it.

An appeal to an intuitive grasp of the number series as well as to the manifold of magnitudes is certainly to be considered.

But, in any case, this should not be a question of intuition in the primitive sense; for, in any case, no infinite manifolds are given to us in the primitive mode of intuitive representation. And even though it might be quite rash to contest any farther-reaching kind of intuitive evidence from the outset, we will nevertheless respect that tendency of exact science that aims to eliminate the finer organs of cognition as far as possible and to use only the most primitive means of cognition.

According to this viewpoint we will consider whether it is possible to ground those transcendent assumptions in such a way that only *primitive intuitive cognitions are applied*. Because of this restricted use of cognitive means, on the other hand, we cannot demand of this grounding that it allows us to recognize as truths (in the philosophical sense) the assumptions that are to be grounded. Rather, we will be content if one succeeds in showing the arithmetic built on those assumptions to be a possible (i. e., consistent) system of thought.

Hereby we have already arrived at the Hilbertian formulation of the problem. But before we look at how the problem must be tackled, we first have to ask ourselves whether there is not a different and perhaps more natural position to take on the transcendent assumptions.

In fact two different attempts suggest themselves and have also been undertaken. The one attempt aims likewise at a demonstration of consistency, not by the means of primitive intuition, but rather with the help of *logic*.

One will recall that the consistency of Euclidian geometry was already proved by Hilbert using the method of reduction to arithmetic. That is why it now seems appropriate to prove also the consistency of arithmetic by reduction to logic.

Frege and Russell, in particular, vigorously tackled the problem of the logical grounding of arithmetic.

In regard to the original goal, the result was negative.

First of all it became obvious with the famous *paradoxes of set theory* that no greater certainty with respect to the usual mathematical methods is achieved by a reduction to logic. The contradictions of naive set theory could be seen logically as well as set theoretically. Also the control of inferences through the logical calculus, which had been developed further precisely for securing mathematical reasoning, did not help in avoiding the contradictions.

When Russell then introduced the very cautious procedure of the [ramified] higher order predicate calculus, it turned out that analysis and set theory in their usual form could not be obtained in this way. Thus, Russell and Whitehead, in *Principia Mathematica*, were forced to introduce an assumption about the system of "first order" predicates, the so-called *axiom of reducibility*.

But one thereby returned completely to the axiomatic standpoint and gave up the goal of the logical grounding.

By the way, the difficulty appears already within the theory of whole numbers. Here, to be sure, one succeeds—by defining the numbers logically according to Frege's fundamental idea—in proving the laws of addition and multiplication and also the individual numerical equations as logical theorems. However, by this procedure one does not obtain the usual number theory, since one cannot prove that for every number there exists a greater one—unless one expressly introduces some kind of axiom of infinity.

Even though the development of mathematical logic did not in principle lead beyond the axiomatic standpoint, an impressive systematic construction of all of arithmetic, equal in rank to the system of Zermelo, has nonetheless emerged in this way.

Moreover, symbolic logic has taken us further in methodical knowledge: Whereas one previously only took account of the *assumptions* of the mathematical theories, now also the *inferences* are made precise. And it turns out that one can replace mathematical reasoning—in so far as only its outcomes matter—by a purely formal manipulation (according to determinate rules) in which actual thinking is completely eliminated.

However, as already said, mathematical logic does not achieve the goal of a logical grounding of arithmetic. And it is not to be assumed that the reason for this failure lies in the particular form of the Fregean approach. It seems rather to be the case that the problem of reducing mathematics to logic is completely ill-posed, namely, because mathematics and logic do not stand at all to each other in the relationship of particular and general.

Mathematics and logic are based on two different directions of abstraction. While logic deals with the *contentually* most general, (pure) mathematics is the general theory of the *formal* relations and properties. Such that, on the one hand each mathematical consideration is subject to the logical laws, while, on the other hand each logical figure-of-thought falls into the domain of mathematical consideration because of the external structure that necessarily comes with it.

In view of this situation one is impelled to an attempt that is, in a certain way, opposed to that of the logical grounding of arithmetic. Because one fails to establish as logically necessary the mathematically transcendent basic assumptions, the question arises whether these assumptions cannot be dispensed with at all.

In fact, one possibility for eliminating the axiomatic basic assumptions seems to consist of elimination entirely the existential form of the axioms and replacing the existential assumptions by *construction postulates*.

Such a replacement procedure is not new to the mathematician; especially in elementary geometry the constructive version of the axioms is often applied. For example, instead of laying down the axiom that any two points determine a line, one postulates the connection of two points by a line as a possible construction.

Likewise, one can now proceed with the arithmetical axioms. For example, instead of saying "each number has a successor," one introduces progression by one or the attachment of $+1$ as a basic operation.

One thus arrives at the attempt of a *purely constructive development of arithmetic*. And indeed this goal for mathematical thought is a very tempting one: Pure mathematics should be the carpenter of its own house and not be dependent on the assumption of a certain system of things.

This constructive tendency, which was first brought forcefully into prominence by Kronecker, and later, in a less radical form, by Poincaré, is currently pursued by Brouwer and Weyl in their new founding of arithmetic.

Weyl first checks the higher modes of inference in regard to the possibility of a constructive reinterpretation; that is, he investigates whether or not the methods of analysis as well as those of Zermelo's set theory can be interpreted constructively. He finds this impossible, for in the attempt to thoroughly carry out a replacement of the existential axioms by constructive methods, one constantly falls into logical circles.

From this Weyl draws the conclusion that the modes of inference of analysis and set theory have to be restricted to such an extent that in their constructive interpretation no logical circles arise. In particular, he feels compelled to give up the theorem of the existence of the upper bound.

Brouwer goes even further in this direction by also applying the constructive principle to large numbers. If one wants, as Brouwer does, to avoid the assumption of a closed given totality of all numbers and to take as a foundation only the act of progressing by one, performable without bound, then statements of the form "There are numbers of such and such a type ..." do not have a *prima facie* meaning. Thus, one is also not justified in generally putting forward, for each number theoretical statement, the alternative that either the statement holds for all numbers or that there is a number (respectively, a pair of numbers, a triple of numbers, ...) by which it is refuted. This way of applying the *tertium non datur* is then at least questionable.

Thereby we find ourselves in a great predicament: The most successful, elegant, and time-tested modes of inference ought to be abandoned just because, from a specific standpoint, one has no justification for them.

The unsatisfactoriness of such a procedure can not be overcome by the considerations, by which Weyl tries to show that the concept formation of the mathematical continuum, as it is fundamental for ordinary analysis, does not correspond to the visual representation of continuity. For, an exact analogy to the content of perception is not at all necessary for the applicability and the fruitfulness of analysis; rather, it is perfectly sufficient that the method of idealization and conceptual interpolation used in analysis be consistently applicable. As far as pure mathematics is concerned, it only matters whether the usual, axiomatically characterized mathematical continuum is in itself a possible, that is, a consistent, structure.

At best, this question could be rejected if, instead of the hitherto prevailing mathematical continuum, we had at our disposal a simpler and more perspicuous conception that would supersede it. But if one examines more closely the new approaches by Weyl and Brouwer, one notices that a gain in simplicity can not be hoped for here; rather, the complications required in the concept formations and modes of inference are only increased instead of decreased.

Thus, it is not justified to dismiss the question concerning the consistency of the usual axiom system for arithmetic. And what we are to draw from Weyl's and Brouwer's investigations is the result that a demonstration of consistency is not possible by means of replacing existential axioms by construction postulates.

So we come back to Hilbert's idea of a theory of consistency based on a primitive-intuitive foundation. And now I would like to describe the plan, according to which Hilbert intends to develop such a theory, and the leading principles to which he adheres in doing so.

Hilbert takes over what is positively fruitful from each of the two attempts at grounding [mathematics] discussed above. From the logical theory he takes the method of the rigorous formalization of inference. That this formalization is necessary follows directly from the way the task is formulated. For the mathematical proofs are to be made the object of a concrete-intuitive form of view. To this end, however, it is necessary that they are projected, as it were, into the domain of the formal. Accordingly, in Hilbert's theory we have to distinguish sharply between the formal image of the arithmetical statements and proofs as the *object* of the theory, on the one hand, and the contentual thought about this formalism, as the *content* of the theory, on the other hand. The formalization is done in such a way that formulas take the place of contentual mathematical statements, and that a sequence of formulas, following each other according to certain rules, takes the place of an inference. But one does not attach any meaning to the formulas; the formula does not count as the expression of a thought, but it corresponds to a contentual judgment only insofar as it plays, within the formalism, a role analogous to that which the judgment plays within the contentual consideration.

More basic than this connection to symbolic logic is the contiguity of Hilbert's approach with the constructive theories of Weyl and Brouwer. For Hilbert in no way wants to abandon the constructive tendency that aims at the autonomy of mathematics; rather, he is especially eager to bring it to bear in the strongest way. In light of what we stated with respect to the constructive method, this appears at first to be incompatible with the goal to demonstrate the consistency proof of arithmetic. In fact, however, the obstacle to the unification of both goals lies only in a preconceived opinion from which the advocates of the constructive tendency have always proceeded until now, namely, that within the domain of arithmetic every construction must indeed be a *number construction* (respectively set construction). Hilbert considers this view to be a prejudice. A constructive reinterpretation of the existential axioms is possible not only in such a way that one transforms them into generating principles for the construction of numbers; rather, the mode of inference made possible by such an axiom can, as a whole, be replaced by a formal procedure in a such a way that certain signs replace general concepts like number, function, etc.

Whenever concepts are missing, a sign will be readily available. This is the methodical principle of Hilbert's theory. An example should explain what is meant. The existential axiom "Each number has a successor" holds in number theory. In keeping with the restriction to what is concretely intuitive, the general concept of number as well as the existential form of the statement must now be avoided.

As mentioned above, the usual constructive reinterpretation consists in this case in replacing the existential axiom by the procedure of progression by one. This is a procedure of *number* construction. Hilbert, on the contrary, replaces the concept of number by a symbol Z and lays down the formula:

$$Z(a) \rightarrow Z(a+1).$$

Here a is a variable for which any mathematical expression can be substituted, and the sign $\rightarrow$ represents the hypothetical propositional connective "if—then," that is, the following rule holds: if two formulas $\mathfrak{A}$ and $\mathfrak{A} \rightarrow \mathfrak{B}$ are written down, then $\mathfrak{B}$ can also be written down.

On the basis of these stipulations, the mentioned formula accomplishes, within the framework of the formalism, exactly what is otherwise accomplished by the existential axiom for contentual argumentation.

Here we see how Hilbert utilizes the method of formalizing inferences according to the constructive tendency; for him it is in no way merely a tool for the demonstration of consistency. Rather, it is, at the same time, also the way to a *rigorous constructive development* of arithmetic. Moreover, the methodical idea of construction is here conceived of so broadly, that also all higher mathematical modes of inference can be incorporated in the constructive development.

After having characterized the goal of Hilbert's theory, I would now like to outline the basic features of the theory. The following three questions are to be answered:

1. The constructive development should represent the formal image of the system of arithmetic and at the same time it should the object for the intuitive theory of consistency. How does such a development take shape?

2. How is the consistency statement to be formulated?

3. What are the means of the contentual consideration by which the demonstration of consistency is to be carried out?

First, as far as the constructive development is concerned, it is accomplished in the following way. Above all, the different kinds of signs are introduced, and at the same time the substitution rules are determined. Furthermore, certain formulas are layed down as basic formulas. And now "proofs" are to be formed.

What counts here as a proof is a concretely written-down sequence of formulas where for each formula the following alternative holds: Either the formula is identical to a basic formula or to a preceding formula, or it results from such a formula by a valid substitution; or, it constitutes the end formula in an "inference," that is, in a sequence of formulas of the type

$$\frac{\begin{array}{c}\mathfrak{A}\\ \mathfrak{A} \rightarrow \mathfrak{B}\end{array}}{\mathfrak{B}}$$

Hence a "proof" is nothing else than a figure with certain concrete properties and such figures constitute the formal image of arithmetic.

This answer to the first question makes the urgency of the second especially evident. For what should the statement of consistency mean in regard to the pure formalism? Isn't it impossible that mere formulas can contradict themselves?

The simple reply to this is: The contradiction is formalized just as well. Faithful to his principle Hilbert introduces the letter Ω for the contradiction; and the role of this letter within the formalism is determined by laying down basic formulas in such a way that from any two formulas—to which contrary statements correspond—Ω can be deduced. More precisely, by adding two such formulas to the basic formulas, a proof with Ω as end formula can be constructed.

In particular the basic formula

$$a = b \rightarrow (a \neq b \rightarrow \Omega)$$

serves us here, where $\neq$ is the usual sign of inequality. (The relation of inequality is taken by Hilbert as a genuine arithmetical relation, just as equality is, but not as the logical negation of equality. Hilbert does not introduce a sign for negation at all.)

Now, the statement of consistency is simply formulated as follows: Ω can not be obtained as the end formula of a proof.

Hence, this claim is in need of a demonstration.

Now the only remaining question concerns the means by which this demonstration should be carried out. In principle this question is already settled. For our whole problem originates from the demand of taking only what is concretely intuitive as a basis for mathematical considerations. Thus the matter is simply to realize which tools are available to us from the concrete-intuitive point of view.

This much is certain: We are justified in using, to the full extent, the elementary ideas of succession and order as well as the usual counting. (For example, we can see whether there are three, or fewer, occurrences of the sign $\rightarrow$ in a formula.)

However, we cannot get by in this way alone; rather, it is absolutely necessary to apply certain forms of complete induction. Yet, in doing so we do not go beyond the domain of what is concretely intuitive.

To wit, two types of complete induction are to be distinguished: the narrower form of induction, which applies only to something completely and concretely given, and the wider form of induction, which uses in an essential manner either the general concept of whole number or the operating with variables.

Whereas this wider form of complete induction is a higher mode of inference which is to be grounded by Hilbert's theory, the narrower form of inference is part of primitive intuitive knowledge and can therefore be used as a tool of contentual argumentation.

As typical examples of the narrower form of complete induction, as it is used in the argumentations of Hilbert's theory, the following two inferences can be adduced:

1. If the sign $+$ occurs at all in a concretely given proof, then, in reading through the proof, one finds a place where it occurs for the first time.

2. If one has a general procedure for eliminating, from a proof with a certain concretely describable property $\mathfrak{E}$, the first occurrence of the sign Z, without the proof losing the property $\mathfrak{E}$ in the process, then one can, by repeated application of the procedure, completely remove the sign Z from such a proof, without its losing the property $\mathfrak{E}$.

(Notice, that here it is exclusively a question of formalized proofs, i. e., proofs in the sense of the definition given above.)

The method which the theory of consistency must follow is hereby set forth in its essentials. Currently the development of this theory is still in its early infancy; most of it has yet to be accomplished. In any case though, the possibility in principle and the feasibility in practice of the required point of view can already be recognized from what has been achieved so far; and one also sees that the considerations to be employed here are *mathematical* in the very genuine sense.

The great advantage of Hilbert's procedure is just this: The problems and difficulties that present themselves in the founding of mathematics are transferred from the epistemologico-philosophical domain into the realm of what is properly mathematical.

Mathematics creates here a court of arbitration for itself, before which all fundamental questions can be settled in a specifically mathematical way, without having to rack one's brain about subtle questions of logical scruples such as whether judgments of a certain form make sense or not.

Hence we can also expect that the enterprise of Hilbert's new theory will soon meet with approval and support within mathematical circles.

Chapter 3

Bernays Project: Text No. 5

Problems of Theoretical Logic (1927)

Probleme der theoretischen Logik

(*Unterrichtsblätter für Mathematik und Naturwissenschaft* XXXIII,
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The topic of the lecture and its title have been chosen in the spirit of Hilbert. What is here called theoretical logic is usually referred to as symbolic logic, mathematical logic, algebra of logic, or logical calculus. The purpose of the following remarks is to present this research area in a way that justifies calling it theoretical logic.

Mathematical logic is in general not very popular. It is most often regarded as idle play that neither supports effectively practical inference nor contributes significantly to our logical insights.

To begin with, the charge of playfulness is only justified with regard to the initial treatment of mathematical logic. The main emphasis was initially put on the formal analogy to algebra, and the pursuit of the latter was often considered as an end in itself. But this was the state of affairs decades ago, and today the problems of mathematical logic are inseparably intertwined with the questions concerning the foundations of the exact sciences, so that one can no longer speak of a merely playful character.

Secondly, concerning the application to practical inference, it has to be mentioned first that a symbolic calculus promises advantages only to someone who has sufficient practice in using it. But, in addition, one has to consider that—in contrast to most kinds of symbolisms which serve, after all, the purpose of abbreviating and contracting operations—it is the primary task of the logical calculus to decompose the inferences into their ultimate constituents and to make outwardly evident each individual step and bring it thereby into focus. The main interest connected with the application of the logical calculus is consequently not one of technique, but of theory and principle. This leads me to the third charge; namely that mathematical logic does not significantly further our logical insights. This opinion is connected with the view on logic expressed by Kant in the second preface to the *Critique of Pure Reason*, where he says: "It is remarkable also that to the present day this logic has not been able to advance a single step, and is thus to all appearance a closed and completed body of doctrine."[a]

It is my intention to show that this standpoint is erroneous. To be sure, Aristotle's formulation of the ultimate principles of inference and their immediate consequences constitutes one of the most significant intellectual accomplishments; it is also one of the very few [accomplishments] which belong to the permanently secured part of the realm of philosophical knowledge. This fact will continue to receive its full due. But this does not prevent us from ascertaining that traditional logic, in posing its problems, is essentially open-ended, and in arranging its facts it is insufficiently adapted to the needs of either a systematic overview and of methodical and epistemologico-critical insights. Only the newer logic, as it has developed under the name of algebra of logic or mathematical logic, introduced such concept formations and such an approach to formal logic as makes it possible to satisfy these needs of systematics and of philosophy.

The realm of logical laws, the world of abstract relations, has only thereby been revealed to us in its formal structure, and the relationship of mathe-

[a][?], p. 17.

matics and logic has been illuminated in a new way. I will try briefly to give an idea of this transformation and of the results it has brought to light.

In doing so I will not be concerned with presenting the historical development or the various forms in which mathematical logic has been pursued. Instead, I want to choose a presentation of the new logic that best facilitates relating and comparing it to traditional logic. As for logical symbols, I shall use the symbolism Hilbert employs now in his lectures and publications.

Traditional logic subdivides its problems into the investigation of concept formation, of judgment, and of inference. It is not advantageous to begin with concept formation, because its essential forms are not elementary but are already based on judgments. Let us begin, therefore, with judgment.

Here, the newer logic immediately introduces an essentially new vantage point, replacing classifications by the search for elementary logical operations. One does not speak of the categorical or the hypothetical or the negative judgment, but of the categorical or hypothetical connexion, of negation as a logical operation. In the same way, one does not classify judgments into universal and particular ones but introduces logical operators for universality and particularity.

This approach is more appropriate than that of classification for the following reason. In judgments different logical processes generally occur in combination, so that a unique corresponding classification is not possible at all.

First let us consider the *categorical* relationship, i. e. that of subject and predicate. We have here an object and a proposition about it. The symbolic representation for this is

$$P(x),$$

to be read as:

"x has the property P."

The relation of the predicate to an object is here explicitly brought out by the variable. This is merely a clearer kind of notation; however, the remark that *several objects* can be subjects of a proposition is crucial. In that case one speaks of a *relation* between several objects. The notation for this is

$$R(x, y), \text{ or } R(x, y, z), \text{ etc.}$$

Cases and prepositions are used in ordinary language to indicate the different members of relations.

By taking into account relations, logic is extended in an essential way when compared with its traditional form. I shall speak about the significance of this extension when discussing the theory of inference.

The forms of universality and particularity are based on the categorical relationship. Universality is represented symbolically by

$$(x)P(x)$$

"all x have the property P."

The variable x appears here as a "bound variable;" the proposition does not depend on x—in the same way as the value of an integral does not depend on the variable of integration.

We sharpen the particular judgment first by replacing the somewhat indefinite proposition, "some x have the property P," with the existential judgment:

"there is an x with the property P,"

written symbolically:

$$(Ex)P(x).$$

By adding *negation*, the four types of judgment are obtained which are denoted in Aristotelian logic by the letters "a, e, i, o."

If we represent negation by putting a bar over the expression to be negated, then we obtain the following representations for the four types of judgment:

$$\begin{array}{rll} \text{a:} & (x) & P(x) \\ \text{e:} & (x) & \overline{P(x)} \\ \text{i:} & (Ex) & P(x) \\ \text{o:} & (Ex) & \overline{P(x)}. \end{array}$$

Already here, in the doctrine of "oppositions," it proves useful for the comprehension of matters to separate the operations; thus we recognize, for example, that the difference between contradictory and contrary opposition lies in the fact that in the former case the whole proposition, e. g., $(x)P(x)$, is negated, whereas in the latter case only the predicate $P(x)$ is negated.

Let us now turn to the *hypothetical relationship*.

$$A \rightarrow B \qquad \text{"if } A \text{, then } B\text{."}$$

This includes a connexion of *two* propositions (predications). So the members of this connexion already have the form of propositions, and the hypothetical relationship applies to these propositions as *undivided units*. The latter already holds also for the negation $\overline{A}$.

There are still other such propositional connexions, in particular:

the fact that A *exists together* with B: $A \mathbin{\&} B$,

and further, the *disjunctive connexion*; there we have to distinguish between the exclusive "or," in the sense of the Latin "aut-aut," and the "or" in the sense of "vel." In accordance with Russell's notation this latter connexion is represented by $A \vee B$.

In ordinary language, such connexions are expressed with the help of conjunctions.

A consideration, analogous to that used in the doctrine of opposition, suggests itself here, namely to combine the binary propositional connexions with negation in one of two ways, either by negating the individual members of the connexion or by negating the latter as a whole. And now, let us see what dependency relations result.

To indicate that two connexions have materially the same meaning (or are "equivalent"), I will write "eq" between them (though, "eq" is not a sign of our logical symbolism).

In particular the following connexions and equivalences result:

$$\begin{array}{rl}
\overline{A} \mathbin{\&} \overline{B}: & \text{“neither } A \text{ nor } B\text{”} \\
\overline{A \mathbin{\&} B}: & \text{“}A \text{ and } B \text{ exclude each other”} \\
\overline{A \mathbin{\&} B} & \text{eq } \overline{A} \vee \overline{B} \\
 & \text{eq } A \rightarrow \overline{B} \\
 & \text{eq } B \rightarrow \overline{A} \\
\overline{A} \rightarrow B & \text{eq } A \vee B \\
\overline{\overline{B}} & \text{eq } B
\end{array}$$

(double negation is equivalent to affirmation).

From this it furthermore follows:

$$\begin{array}{rl}
A \rightarrow B & \text{eq } \overline{A \mathbin{\&} \overline{B}} \\
 & \text{eq } \overline{A} \vee B \\
\overline{A \vee B} & \text{eq } \overline{\overline{A} \rightarrow B} \\
 & \text{eq } \overline{A} \mathbin{\&} \overline{B}.
\end{array}$$

On the basis of these equivalences it is possible to express some of the logical connexions

$$\overline{},\ \rightarrow,\ \&,\ \vee$$

by means of others. In fact, according to the above equivalences one can express

$$\begin{array}{lll} \rightarrow & \text{by} & \vee \text{ and } \overline{} \\ \vee & \text{by} & \& \text{ and } \overline{} \\ \& & \text{by} & \rightarrow \text{ and } \overline{} \end{array}$$

so that each of

$$\begin{array}{ll} & \& \text{ and } \overline{} \\ \text{or} & \vee \text{ and } \overline{} \\ \text{or} & \rightarrow \text{ and } \overline{} \end{array}$$

alone suffice as basic connexions. One can get along even with a single basic connexion, but, to be sure, not with one of those for which we already have a sign. If we introduce for the connexion of mutual exclusion $\overline{A \,\&\, B}$ the sign $A|B$ then the following equivalences obtain:

$$\begin{array}{lll} A|A & \text{eq} & \overline{A} \\ A|\overline{B} & \text{eq} & \overline{A \,\&\, \overline{B}} \\ & \text{eq} & A \rightarrow B. \end{array}$$

This shows that with the aid of this connexion one can represent negation as well as $\rightarrow$ and, consequently, the remaining connexions. Just like the relation of mutual exclusion also the connexion

$$\text{“neither — nor”} \quad \overline{A} \,\&\, \overline{B}$$

can be taken as the only basic connexion. If for this connexion we write

$$A \parallel B,$$

then we have

$$\begin{array}{lll} A \parallel A & \text{eq} & \overline{A} \\ \overline{A} \parallel \overline{B} & \text{eq} & A \,\&\, B; \end{array}$$

thus, negation as well as & is expressible by means of this connexion.

These reflections already border somewhat on the playful. Nevertheless, it is remarkable that the discovery of such a simple fact as that of reducing all propositional connexions to a single one was reserved for the 20th century. The equivalences between propositional connexions were not at all systematically investigated in the old logic.[1] There one finds only occasional remarks like, for example, that of the equivalence of

$$A \to \overline{B} \text{ with } B \to \overline{A}$$

on which the inference by "contraposition" is based. The systematic search for equivalences is, however, all the more rewarding as one reaches here a self-contained and entirely surveyable part of logic, the so-called *propositional calculus.* I will explain in some detail the value of this calculus for reasoning.

Let us reflect on what the sense of equivalence is. When I say

$$\overline{A \,\&\, B} \text{ eq } \overline{A} \vee \overline{B},$$

I do not claim that the two complex propositions have the same sense but only that they *have the same truth value*. That is, no matter how the individual propositions A, B are chosen, $\overline{A} \vee \overline{B}$ and $\overline{A \,\&\, B}$ are always simultaneously true or false, and consequently these two expressions can represent each other with respect to truth.

Indeed, any complex proposition A and B can be viewed as a mathematical function assigning to each pair of propositions A, B one of the values "true" or "false." The actual content of the propositions A, B does not matter at all. Rather, what matters is only whether A is true or false and whether B is true or false. So we are dealing with *truth functions*: To a pair of truth values another truth value is assigned.

Each such function can be given by a schema in such a way, that the four possible connections of two truth values (corresponding to the propositions

[1]Today these historical remarks stand in need of correction. In the first place, the reducibility of all propositional connexions to a single one was already discovered in the 19th century by Charles S. Peirce—to be sure, a fact which became more generally known only with the publication of his collected works in 1933. Further, it is not correct that the equivalences between propositional connectives were not considered systematically in the old logic—to be sure, not in Aristotelian logic, but in other Greek schools of philosophy. (On this topic see $_{c_1}$**the**$_{c_1}$ book *Formal Logic* (*vide* [**?**]).)

Remark: This footnote, as well as the next three, are subsequent additions occasioned by the republication of this lecture.

A, B) are represented by four cells, and in each of these the corresponding truth value of the function ("true" or "false") is written down.

The schemata for $A \mathbin{\&} B, A \vee B, A \rightarrow B$ are specified here.

$A \mathbin{\&} B$:

B \ A	true	false
true	true	false
false	false	false

$A \vee B$:

B \ A	true	false
true	true	true
false	true	false

$A \rightarrow B$:

B \ A	true	false
true	true	true
false	false	true

One can easily calculate that there are exactly 16 different such functions. The number of different functions of n truth values

$$A_1, A_2, \ldots, A_n$$

is, correspondingly, $2^{(2^n)}$.

To each function of two or more truth values corresponds a class of substitutable[b] propositions of connexions. Among these one class is distinguished, namely the class formed by those connexions that are always true.

These connexions represent all logical sentences that hold generally and in which individual propositions occur only as undivided units. We will call the expressions representing sentences that hold generally *valid formulas.*[c]

We master propositional logic, if we know the valid formulas (among the propositions of connexions), or if we can decide for a given propositional

[b] *Vide* [**?**], pp. 47–48: "Um uns kurz ausdrücken zu können wollen wir zwei Aussagenverknüpfungen durch einander 'ersetzbar' nennen, wenn sie dieselbe Wahrheitsfunktion darstellen."

[c] *Vide* ■ for the distinction between "to hold generally" and "to be valid."

connexion whether or not it is valid. After all, the task for reasoning in propositional logic is formulated as follows:

Certain connexions

$$V_1, V_2, \ldots, V_k,$$

are given; they are built up from elementary propositions $A, B, \ldots$, and represent true sentences for a certain interpretation of the elementary propositions. The question is whether another given connexion D of these elementary propositions follows logically whenever $V_1, V_2, \ldots, V_k$ are valid, indeed without considering the more precise content of the propositions $A, B, \ldots$.

The answer to this question is "yes," if and only if

$$(V_1 \,\&\, V_2 \,\&\, \ldots \,\&\, V_k) \rightarrow D,$$

composed from $A, B, \ldots$, represents a valid formula.

The decision concerning the validity of a propositional connexion can in principle always be reached by trying out all relevant truth values. The method of considering equivalences, however, provides a more convenient procedure. That is to say, by means of equivalent transformations each formula can be put into a certain *normal form* in which only the logical symbols $\&, \vee, {}^{-}$ occur, and from this normal form one can read off directly whether or not the formula is valid.

The rules of transformation are also very simple. One can in particular calculate with & and $\vee$ in complete analogy to $+$ and $\cdot$ in algebra. Indeed, matters are here even simpler, as & and $\vee$ can be treated in a completely symmetrical way.

By considering the equivalences, we entered, as already mentioned, the domain of inferences. But here we carried out the inferences, as it were, in a naive way, on the basis of the meaning of the logical connexions, and we turned the task of making inferences into a decision problem.

But for logic there remains the task of *systematically* presenting the rules of inference.

Aristotelian logic lays down the following principles of inference:

1. Rule of categorical inference: the *dictum de omni et nullo*: what holds universally, holds in each particular instance.

2. Rule of hypothetical inference: if the antecedent is given, then the consequent is given, i. e. if A and if $A \rightarrow B$, then B.

3. Laws of negation: law of contradiction and law of excluded middle: A and $\overline{A}$ can not both hold, and, at least one of the two propositions must hold.

4. Rule of disjunctive inference: if at least one of A or B holds and if $A \to C$ as well as $B \to C$, then C holds.

One can say that each of these laws represents the implicit definition for a logical process: 1. for universality, 2. for the hypothetical connexion, 3. for negation, 4. for disjunction ($\vee$).

These laws contain indeed the essence of what is expressed when inferences are being made. But for a complete analysis of inferences this does not suffice. For this we demand that nothing needs to be reflected upon, once the principles of inference have been spelled out. The rules of inference must be constituted in such a way that they eliminate logical thinking. Otherwise we would have to have again logical rules which specify how to apply those rules.

This demand to exorcise the mind can indeed be met. The development of the doctrine of inferences obtained in this way is analogous to the axiomatic development of a theory. Certain logical laws written down as formulas correspond here to the axioms, and operating [on formulas] externally according to fixed rules, that lead from the initial formulas to further ones, corresponds to contentual reasoning that usually leads from axioms to theorems.

Each formula, that can be derived in such a way, represents a valid logical proposition.

Here it is once again advisable to separate out *propositional logic*, which rests on the principles 2., 3., and 4. We need only the following rules; we represent the elementary propositions by variables

$$X, Y, \ldots$$

The first rule now states: any propositional connexion can be substituted for such variables (substitution rule).

The second rule is the inference schema

$$\begin{array}{c} \mathfrak{S} \\ \mathfrak{S} \to \mathfrak{T} \\ \hline \mathfrak{T} \end{array}$$

according to which the formula $\mathfrak{T}$ is obtained from two formulas $\mathfrak{S}$, $\mathfrak{S} \to \mathfrak{T}$.

The choice of the initial formulas can be made in quite different ways. One has taken great pains, in particular, to get by with the smallest possible number of axioms, and in this respect the limit of what is possible has indeed been reached. The purpose of logical investigations is better served, however, when we separate, as in the axiomatics for geometry, various *groups of axioms* from one another, such that each group gives expression to the role of one logical operation. The following list then emerges:

I	Axioms of implication
IIa)	Axioms for &
IIb)	Axioms for $\vee$
III	Axioms of negation.

This system of axioms[d] generates through application of the rules *all* valid formulas of propositional logic.[2] This *completeness* of the axiom system can be characterized even more sharply by the following facts: if we add any underivable formula to the axioms, then we can deduce with the help of the rules arbitrary propositional formulas.

The division of the axioms into groups has a particular advantage, as it allows one to separate out *positive logic*. We understand this to be the system of those propositional connexions that are valid without assuming that an opposite exists.[e] For example:

$$(A \mathbin{\&} B) \rightarrow A$$
$$(A \mathbin{\&} (A \rightarrow B)) \rightarrow B.$$

The system of these formulas presents itself in our axiomatics as the totality of those formulas that are derivable without using axiom group III. This system is far less perspicuous than the full system of valid formulas. Also,

[2] We refer here only to those formulas that can be built up with the operations $\rightarrow, \&, \vee$ and with negation. If further operation symbols are added, then they can be introduced by replacement rules. To be sure, one is not bound to distinguish the four mentioned operations in this particular way.

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[d] Fixing the axioms as in [**?**], p. 65; already formulated in the early twenties As to completeness, cf. the Habilitationsschrift of Bernays written in 1918.

[e] *Vide* [**?**], p. 67: "Die '*positive Logik*' ..., d. h. die Formalisierung derjenigen logischen Schlüsse, welche unabhängig sind von der Voraussetzung, daß zu jeder Aussage ein Gegenteil existiert."

no decision procedure is known by which one can determine, in accordance with a definite rule, whether a formula belongs to this system.[3] It is not the case that, for instance, every formula expressible in terms of $\to, \&, \vee$, which is valid and therefore derivable on the basis of I–III, is already derivable from I–II. One can rigorously prove that this is not the case.

An example is provided by the formula

$$A \vee (A \to B).$$

Representing $\to$ by $\vee$ and $^-$ this formula turns into

$$A \vee (\overline{A} \vee B),$$

and this representation allows one immediately to recognize the formula as valid. However, it can be shown that the formula is not derivable within positive logic, i. e., on the basis of axioms I–II. Hence, it does not represent a law of positive logic.

We recognize here quite clearly that negation plays the role of an *ideal element* whose introduction aims at rounding off the logical system to a totality with a simpler structure, just as the system of real numbers is extended to a more perspicuous totality by the introduction of imaginary numbers, and just as the ordinary plane is completed to a manifold with a simpler projective structure by the addition of points at infinity. Thus this method of ideal elements, fundamental to science, is already encountered here in logic, even if we are usually not aware of its significance.

A special part of positive logic is constituted by the doctrine of *chain inferences* that was discussed already in Aristotelian logic. In this area there are also natural problems and simple results, not known to traditional logic and again requiring that specifically mathematical considerations be brought to bear. I have in mind Paul Hertz's investigations of sentence-systems.[f] —

The axiomatics we have considered up to now refers to those inferences which depend solely on the rules of the hypothetical and disjunctive inference, and of negation. Now we still have the task of incorporating *categorical*

[3]Since then decision procedures for positive logic have been given by Gerhard Gentzen and Mordechaj Wajsberg.

[f] *Vide* [**?**], p. 84, but also [**?**]■ , p. 300.

reasoning into our axiomatics. How this is done I will only describe briefly here.

Of the *dictum de omni et nullo* we need also the converse: "what holds in each particular instance, also holds generally." Furthermore, we have to take into account the particular judgment. It holds analogously:

> "If a proposition $A(x)$ is true of some object x, then there is an object of which it is true, and vice versa."

Thus we obtain four principles of reasoning that are represented in the axiomatics by two new initial formulas and two rules. A substitution rule for the individual variables $x, y, \ldots$ is also added.

Moreover, the substitution rule concerning propositional variables $X, Y, \ldots$ has to be extended in such a way now that the formulas of propositional logic can be applied also to expressions containing individual variables.

Let us now see, how the typical Aristotelian inferences are worked out from this standpoint. For that it is necessary to say first something about the interpretation of the universal judgment "all S are P."

According to the Aristotelian view, such a judgment presupposes that there are certain objects with property S, and it is then claimed that all these objects have property P. This interpretation of the universal judgment, to which Franz Brentano in particular objected from the side of philosophy, is admittedly quite correct. But it is suited neither for the purposes of theoretical science nor for the formalization of logic, since the implicit presupposition brings with it unnecessary complications. Therefore we shall restrict the content of the judgment, "all S are P," to the assertion, "an object having property S has also property P."

Accordingly, such a judgment is simultaneously universal and hypothetical. It is represented in the form

$$(x)(S(x) \rightarrow P(x)).$$

The so-called categorical inferences contain consequently a combination of categorical and hypothetical inferences. I want to illustrate this by a classical example:

> "All men are mortal, Cajus is a man, therefore Cajus is mortal."

If we represent "x is human" and "x is mortal" in our notation by $H(x)$ and $Mrt(x)$ respectively, then the premises are

$$(x)(H(x) \rightarrow Mrt(x)),$$
$$H(Cajus),$$

and the conclusion is: $Mrt(Cajus)$.

The derivation proceeds, first, according to the inference from the general to the particular, by deducing from

$$(x)(H(x) \rightarrow Mrt(x))$$

the formula

$$H(Cajus) \rightarrow Mrt(Cajus).$$

And this proposition together with

$$H(Cajus)$$

yields according to the schema of the hypothetical inference:

$$Mrt(Cajus).$$

It is characteristic for this representation of the inference that one refrains from giving a quantitative interpretation of the categorical judgment (in the sense of subsumption). Here one recognizes particularly clearly that mathematical logic does not depend in the least upon being a logic of extensions.

Our rules and initial formulas permit us now to derive all the familiar Aristotelian inferences as long as they agree with our interpretation of the universal judgment—that leaves just 15. In doing so one realizes that there are actually only very few genuinely different kinds of inferences. Furthermore, one gets the impression that the underlying problem is delimited in a quite arbitrary way.

A more general problem, which is also solved in mathematical logic, consists in finding a decision procedure that allows one to determine whether a predicate formula is valid or not. In this way, one masters reasoning in the domain of predicates, just as one masters propositional logic with the decision procedure mentioned earlier.

But our rules of inference extend much farther. The actual wealth of logical connections is revealed only when we consider *relations* (predicates with several subjects). Only then does it become possible to capture *mathematical proofs* in a fully logical way.

However, here one is induced to add various *extensions* which are suggested to us also by ordinary language.

The first extension consists in introducing a formal expression for "x is the same object as y," or "an object different from y." For this purpose the "*identity* of x and y" has to be formally represented as a particular relation, the properties of which are to be formulated as axioms.

Second, we need a symbolic representation of the logical relation we express linguistically with the aid of the genitive or the relative pronoun in such phrases as "the son of Mr. X" or "the object that." This relation forms the basis of the *concept of a function* in mathematics. It matters here that an object, having uniquely a certain property or satisfying a certain relation to particular objects, is characterized by this property or relation.

The most significant extension, however, is brought about by the circumstance that we are led to consider predicates and relations themselves as objects, just as we do in ordinary language when we say, for example, "patience is a virtue." We can state properties of predicates and relations, and furthermore, [second order] relations between predicates and also between relations. Likewise, the forms of universality and particularity can be applied with respect to predicates and relations. In this way we arrive at a logic of *second order*; for its formal implementation the laws of categorical reasoning have to be extended appropriately to the domain of predicates and relations.

The solution of the decision problem—which, incidentally, is here automatically subsumed under a more general problem—presents an enormous task for this enlarged range of logical relation resulting from the inclusion of relations and the other extensions mentioned. Its solution would mean that we have a method that permits us, at least in principle, to decide for any given mathematical proposition whether or not it is provable from a given list of axioms. As a matter of fact, we are far from having a solution of this problem. Nevertheless, several important results of a very general character have been obtained in this area through the investigations of Löwenheim and Behmann; in particular one succeeded in completely solving the decision problem for *predicate logic* also in the case of second order logic.[4]

Here we see that the traditional doctrine of inferences comprises only a

[4]Notice that one speaks here of "predicate logic" in the sense of the distinction between predicates and relations. Thus, what is meant here by "predicate logic" is what currently is mostly called the logic of monadic predicates. The logic of polyadic predicates is already generally undecidable for the first order case, as was shown by Alonzo Church.

minute part of what really belongs to the domain of logical inference.

As yet I have not even mentioned *concept formation*. And, for lack of time, I cannot consider it in detail. I will just say this much: a truly penetrating logical analysis of concept formation becomes possible only on the basis of the theory of relations. Only by means of this theory one realizes what kind of complicated combinations of logical expressions (relations, existential propositions, etc.) are concealed by short expressions of ordinary language. Such an analysis of concept formation has been initiated to a large extent, especially by Bertrand Russell, and it has led to knowledge about general logical processes of concept formation. The methodical understanding of science is being furthered considerably through their clarification.

I now come to the end of my remarks. I have tried to show that logic, that is to say the correct old logic as it was always intended, obtains its genuine rounding off, its proper development and systematic completion, only through its mathematical treatment. The mathematical mode of consideration is introduced here not artificially, but rather arises in an entirely natural way, in the further pursuit of [actual] problems.

The resistance to mathematical logic is widespread, particularly among philosophers; it has—apart from the reasons mentioned at the beginning—also a principled one. Many approve of having mathematics absorbed into logic. But here one realizes the opposite, namely, that the system of logic is absorbed into mathematics. With respect to the mathematical formalism logic appears here as a specific interpretation and application, perfectly resembling the relation between, for example, the theory of electricity and mathematical analysis, when the former is treated according to Maxwell's theory.

That does not contradict the generality of logic, but rather the view that this generality is superordinate to that of mathematics. Logic is about certain contents that find application to any subject matter whatsoever, insofar as it is thought about. Mathematics, on the other hand, is about the most general laws of any combination whatsoever. This is also a kind of highest generality, namely, in the direction towards the *formal*. Just as every thought, including the mathematical ones, is subordinate to the laws of logic, each structure, each manifold however primitive—and thus also the manifold given by the combination of sentences or parts of sentences—must be subject to mathematical laws.

If we wanted a logic free of mathematics, no theory at all would be left, but only pure reflection on the most simple connections of meaning. Such purely contentual considerations—which can be comprised under the name "philosophical logic"—are, in fact, indispensable and decisive as a starting point for the logical theory; just as the purely physical considerations, serving as the starting point for a physical theory, constitute the fundamental intellectual achievement for that theory. But such considerations do not constitute fully the theory itself. Its development requires the mathematical formalism. Exact systematic theory of a subject is, for sure, mathematical treatment, and it is in this sense that Hilbert's dictum holds: ■ "Anything at all that can be the object of scientific thought, as soon as it is ripe for the formation of a theory ... will be part of mathematics."■ [g] Even logic can not escape this fate.

[g] *Vide* [**?**], p. ■ .

Chapter 4

Bernays Project: Text No. 6

Appendix to Hilbert's lecture "The foundations of mathematics" (1927)

Zusatz zu Hilberts Vortrag (über „Die Grundlagen der Mathematik")

(*Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität* 6, pp. 89–92; engl. in [**?**], pp. 485–489)

Translation by: *Stefan Bauer-Mengelberg, Dagfinn Føllesdal*
Revised by: *CMU*
Final revision by:

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1. To supplement the preceding paper [by Hilbert] let me add some more detailed explanations concerning the consistency proof by Ackermann that was sketched there.

First, as for an upper bound on the number of steps of replacement in the *case of embedding*, it is given by 2^n, where n is the number of ε-functionals distinct in form. The method of proof described furnishes yet another, substantially closer bound, which, for example, for the case in which there is no embedding at all yields the upper bound $n + 1$.[a]

[a] *Vide* [**?**], pp. 96–97, for how this bound is obtained.

2. Let the argument by which we recognize that the procedure is finite in the *case of superposition* be carried out under simple specializing assumptions.

The assumptions are the following: Let the ε-functionals occurring in the proof be

$$\varepsilon_a \mathfrak{A}(a, \varepsilon_b \mathfrak{K}(a, b))$$

and

$$\varepsilon_b \mathfrak{K}(\mathfrak{a}_1, b), \varepsilon_b \mathfrak{K}(\mathfrak{a}_2, b), \ldots, \varepsilon_b \mathfrak{K}(\mathfrak{a}_n, b),$$

where $\mathfrak{a}_1, \ldots, \mathfrak{a}_n$ may contain $\varepsilon_a \mathfrak{A}(a, \varepsilon_b \mathfrak{K}(a, b))$ but no other ε-functional.

The procedure now consists in a succession of "total replacements;" each of these consists of a function replacement $\chi(a)$ for $\varepsilon_b \mathfrak{K}(a, b))$, by means of which $\varepsilon_a \mathfrak{A}(a, \varepsilon_b \mathfrak{K}(a, b))$ goes over into $\varepsilon_a \mathfrak{A}(a, \chi(a))$, and a replacement for $\varepsilon_a \mathfrak{A}(a, \chi(a))$, by means of which $\mathfrak{a}_1, \ldots, \mathfrak{a}_n$ go over into numerals $\mathfrak{z}_1, \ldots, \mathfrak{z}_n$ and the values

$$\chi(\mathfrak{z}_1), \ldots, \chi(\mathfrak{z}_n)$$

are obtained for

$$\varepsilon_b \mathfrak{K}(\mathfrak{a}_1, b), \ldots, \varepsilon_b \mathfrak{K}(\mathfrak{a}_n, b).$$

We begin with the function

$$\chi_0(a),$$

which has the value 0 for all a ("zero replacement"), and accordingly also replace the terms

$$\varepsilon_b \mathfrak{K}(\mathfrak{a}_1, b), \ldots, \varepsilon_b \mathfrak{K}(\mathfrak{a}_n, b)$$

by 0.

Holding this replacement fixed, we apply to

$$\varepsilon_a \mathfrak{A}(a, \chi_0(a))$$

the original testing procedure, which after two steps at most leads to the goal; that is, all the critical formulas corresponding to

$$\varepsilon_a \mathfrak{A}(a, \chi_0(a))$$

then become correct.

Thus we obtain one or two total replacements,

$$\mathfrak{E}_0, \text{ or } \mathfrak{E}_0 \text{ and } \mathfrak{E}_0',$$

respectively. Now either $\mathfrak{E}_0$ (or $\mathfrak{E}'_0$) is final or one of the critical formulas corresponding to

$$\varepsilon_b\mathfrak{K}(\mathfrak{a}_1, b),\ \varepsilon_b\mathfrak{K}(\mathfrak{a}_2, b),\ \ldots$$

becomes false. Assume that this formula corresponds to, say $\varepsilon_b\mathfrak{K}(\mathfrak{a}_1, b)$ and that $\mathfrak{a}_1$ goes into $\mathfrak{z}_1$. Then we find a value $\mathfrak{z}$ such that

$$\mathfrak{K}(\mathfrak{z}_1, \mathfrak{z})$$

is correct. Now that we have this value, we take as replacement function for

$$\varepsilon_b\mathfrak{K}(a, b)$$

not $\chi_0(a)$, but the function

$$\chi_1(a)$$

defined by

$$\begin{aligned} \chi_1(\mathfrak{z}_1) &= \mathfrak{z} \\ \chi_1(a) &= 0 \quad \text{for } a \neq \mathfrak{z}_1. \end{aligned}$$

At this point we repeat the above procedure with $\chi_1(a)$, the values of the

$$\varepsilon_b\mathfrak{K}(\mathfrak{a}_\nu, b) \qquad (\nu = 1, \ldots, n)$$

now being determined only after a value has been chosen for

$$\varepsilon_a\mathfrak{A}(a, \chi_1(a)),$$

and thus we obtain one or two total replacements,

$$\mathfrak{E}_1,\ \text{or } \mathfrak{E}_1 \text{ and } \mathfrak{E}'_1.$$

Now either $\mathfrak{E}_1$ (or $\mathfrak{E}'_1$) is final or for one of the ε-functionals that result from

$$\varepsilon_b\mathfrak{K}(\mathfrak{a}_1, b), \ldots, \varepsilon_b\mathfrak{K}(\mathfrak{a}_n, b),$$

by the previous total replacement we again find a value $\mathfrak{z}'$, such that for a certain $\mathfrak{z}_2$

$$\mathfrak{K}(\mathfrak{z}_2, \mathfrak{z}')$$

is correct, while

$$\mathfrak{K}(\mathfrak{z}_2, \chi_1(\mathfrak{z}))$$

is false. From this it directly follows that

$$\mathfrak{z}_2 \neq \mathfrak{z}_1.$$

Now, instead of $\chi_1(a)$ we introduce $\chi(a)_2$ as replacement function by means of the following definition:

$$\begin{aligned}
\chi_2(\mathfrak{z}_1) &= \mathfrak{z} \\
\chi_2(\mathfrak{z}_2) &= \mathfrak{z}' \\
\chi_2(a) &= 0 \quad \text{for } a \neq \mathfrak{z}_1, \mathfrak{z}_2.
\end{aligned}$$

The replacement procedure is now repeated with this function $\chi_2(a)$.

As we continue in this way, we obtain a sequence of replacement functions

$$\chi_0(a), \chi_1(a), \chi_2(a), \ldots,$$

each of which is formed from the preceding one by addition, for a new argument value, of a function value different from 0; and for every function $\chi(a)$ we have one or two replacements,

$$\mathfrak{E}_p, \text{ or } \mathfrak{E}_p \text{ and } \mathfrak{E}'_p.$$

The point is to show that this sequence of replacements terminates. For this purpose we first consider the replacements

$$\mathfrak{E}_0, \mathfrak{E}_1, \mathfrak{E}_2, \ldots .$$

In these,

$$\varepsilon_a \mathfrak{A}(a, \varepsilon_b \mathfrak{K}(a, b))$$

is always replaced by 0; the

$$\varepsilon_b \mathfrak{K}(\mathfrak{a}_\nu, b) \qquad (\nu = 1, \ldots, n)$$

therefore always go over into the same ε-functionals; for each of these we put either 0 or a numeral different from 0, and this is then kept as a final replacement. Accordingly, at most $n + 1$ of the replacements

$$\mathfrak{E}_0, \mathfrak{E}_1, \mathfrak{E}_2, \ldots$$

can be distinct.[b] If, however, $\mathfrak{E}_k$ is identical with $\mathfrak{E}_l$, then neither one has, or else each has, a successor replacement

$$\mathfrak{E}'_k, \text{ or } \mathfrak{E}'_l,$$

and in these

$$\varepsilon_a \mathfrak{A}(a, \varepsilon_b \mathfrak{K}(a, b))$$

is then in both cases replaced by the same number found as a value, so that, for both replacements, the

$$\varepsilon_b \mathfrak{K}(\mathfrak{a}_\nu, b) \qquad (\nu = 1, \dots, n)$$

also go over into the same ε-functionals.

Accordingly, of the replacements $\mathfrak{E}'_l$ for which $\mathfrak{E}_l$ coincides with a fixed replacement $\mathfrak{E}_k$ again at most $n + 1$ can be distinct.

Hence there cannot be more than $(n + 1)^2$ distinct

$$\mathfrak{E}_p, \text{ or } \mathfrak{E}_p \text{ and } \mathfrak{E}'_p$$

altogether. From this it follows, however, that our procedure comes to an end at the latest with the replacement function

$$\chi_{(n+1)^2}(a).$$

For, the replacements associated with two distinct replacement functions $\chi_p(a)$ and $\chi_q(a)$, $q > p$, cannot coincide completely, since otherwise we would by means of $\chi_q(a)$ be led to the same value $\mathfrak{z}^*$ that has already been found by means of $\chi_p(a)$, whereas this value is already used in the definition of the replacement functions following $\chi_p(a)$, hence in particular also in that of $\chi_q(a)$.

3. Let us note, finally, that in order to take into consideration the axiom of complete induction, which for the purpose of demonstrating the consistency may be given in the form

$$(\varepsilon_a A(a) = b') \to \overline{A}(b),$$

we need only, whenever we have found a value $\mathfrak{z}$ for which a proposition $\mathfrak{B}(a)$ holds, go to the least such value by seeking out the first correct proposition in the sequence

$$\mathfrak{B}(0), \mathfrak{B}(0'), \dots, \mathfrak{B}(\mathfrak{z})$$

of propositions that have been reduced to numerical formulas.[c]

[b] See footnote a.

[c] In [?], p. 213, end of footnote 1, Bernays writes that this last paragraph, on mathe-

matical induction, should be deleted. In 1927 Hilbert and his collaborators had not yet gauged the difficulties facing consistency proofs of arithmetic and analysis. Ackermann had set out (in [?]) to prove the consistency of analysis; but, while correcting the printer's proofs of his paper, he had to introduce a footnote, on page 9, that restricts his rule of substitution. After the introduction of such a restriction it was no longer clear for which system Ackermann's proof establishes consistency. Certainly not for analysis. The proof suffered, moreover, from imprecisions in its last part. Ackermann's paper was received for publication on 30 March 1924 and came out on 26 November 1924. In 1927, received for publication on 29 July 1925 and published on 2 January 1927, von Neumann criticized Ackermann's proof and presented a consistency proof that followed lines somewhat different from those of Ackennann's. The proof came to be accepted as establishing the consistency of a first-order arithmetic in which induction is applied only to quantifier-free formulas. When he was already acquainted with von Neumann's proof, Ackermann communicated, in the form of a letter, a new consistency proof to Bernays. This proof developed and deepened the arguments used in Ackermann's 1924 proof, and, like von Neumann's, it applied to an arithmetic in which induction is restricted to quantifier-free formulas. It is with this proof of Ackermann's that Hilbert's remarks above [BB: insert explicit reference] (pp. 477-479) and Bernays's present comments are concerned. It was felt at that point, among the members of the Hilbert school, that the consistency of full first-order arithmetic could be established by relatively straightforward extensions of the arguments used by von Neumann or by Ackermann (*vide* [?], p. 137, lines 20-21; [?], p. 490, line 4u, to p. 491, line 2; [?], p. 211, lines 4-7). These hopes were dashed by Gödel's 1931. Ackermann's unpublished proof was presented in [?], pp. 93-130. In [?] Ackermann gave a consistency proof for full first-order arithmetic, using a principle of transfinite induction (up to ε_0) that is not formalizable in this arithmetic.

Chapter 5

Bernays Project: Text No. 7

On Nelson's Position in the Philosophy of Mathematics (1928)

Über Nelsons Stellungnahme in der Philosophie der Mathematik

(*Die Naturwissenschaften* 16, pp. 142–145)

Translation by: *Volker Peckhaus*
Revised by: *Paolo Mancosu*
Final revision by: *CMU*

In connection with the preceding article by Otto Meyerhof, a few words on Nelson's significance for the philosophy of mathematics might be added.

Nelson was among those philosophers whose style of thinking resulted from a familiarity with the spirit of the exact sciences. Mathematics and physics represented the methodical ideal that he strove to achieve in elaborating his philosophical thoughts.

He considered the demand of systematic rigor to be completely satisfied in mathematical axiomatics, in particular in the form that Hilbert had given it in *Foundations of Geometry.* And he therefore endeavored to extend the reach of this method of axiomatics in the domain of philosophy.

In doing so Nelson avoided the unfruitful imitation of mathematics that was dominant in pre-Kantian metaphysics, which was based on the belief that knowledge could be conjured up from nothing by logical reasoning.

As a follower of Kant, he held the doctrine of the *synthetic character* of mathematical knowledge; he stressed that the cognitive content of mathematics was captured in its axioms, which he considered the expression of knowledge deriving from *pure intuition*.

In various writings, in particular in the essay "Remarks on non-Euclidean geometry" (1906), he turned against the skeptical and the empiricist conceptions, which—in regard to the validity of the geometrical axioms—have found more and more adherents among scientists since the discovery of non-Euclidean geometry.

Here he shows how these views result from clinging to the old Aristotelian doctrine according to which all knowledge has its origin either in the senses as the source of experience or in the understanding as the source of logic.

If this disjunction, which in itself is not compelling, is dropped, one retains the possibility of recognizing extra-logical necessities, especially of an intuitive sort, which are expressed in synthetic propositions. In particular, concerning the parallel axiom, if this "dogmatic disjunction" is abandoned, it is by no means possible to infer from the logical possibility of a non-Euclidean geometry that the parallel axiom has no necessary validity, but only the that this axiom has a synthetic, i. e. non-logical, character.

These ideas were further elaborated by Nelson in a lecture "On the Foundations of Geometry," which he delivered in Paris in April 1914 (on the occasion of the foundation of the *Société internationale de philosophie mathématique*.

Here Nelson supports his claim of the *intuitive but at the same time rational character of geometrical knowledge* by a series of arguments.

In particular, he points out that the difficulties presented by a conceptual description of (the continuity of) the continuum are a clear sign of the fact that this is a task posed to thought from without, i. e. through intuition.

He furthermore emphasizes that intuition cannot be charged with the typical geometrical errors such as, for instance, those which originate from overlooking the possibility of one-sided surfaces; rather, they result from hasty conceptual generalizations of intuitively grasped states of affairs.

In addition, he objects to the claim that non-Euclidean space can be grasped intuitively. In the familiar spatial presentations of non-Euclidean geometry, e. g. by the geometry of the interior of a sphere with a suitable definition of congruence, what is presented is not a non-Euclidean space but only the satisfaction of the non-Euclidean laws by certain objects and relations of the Euclidean space.

If this argument is not accepted by many today, this is due to the fact that today's mathematicians and physicists have mostly lost sight the real meaning of the words "intuition" and "intuitive," so that one talks about intuitiveness in most cases only in a paled and blurred sense, according to which no distinction is drawn between real intuitive representation and mere intuitive analogy.

A weightier objection against Nelson's standpoint originates from the view that our spatial intuition is not perfectly sharp; therefore the geometrical laws are only approximately determined by intuition and are derived from the data of intuition only by a process of idealization.

Nelson argues against this claim as follows. It cannot be denied that the geometrical axioms represent an idealization with respect to the facts of observation. But this circumstance only speaks against the *empirical* character of geometrical laws. Their *intuitive* character is not thereby disputed (unless one relies on the dogmatic disjunction already mentioned).

On the contrary, an idealization presupposes an ideal. Only if such an ideal, in the sense of an epistemological norm, is given to us, does the abstraction that is to be carried out by the idealization have its definite distinctiveness, free of arbitrariness; and only then, as well, is the stability of the idealization vis-a-vis the extensions of our domain of experience guaranteed. Hence, it is the viewpoint of idealization that points to the fact of pure intuition, on the basis of which the process of idealization can simply be understood as the transition from sensory intuition to pure intuition.

From this doctrine of pure intuition as the norm for geometrical idealizations, Nelson draws the consequence that there is a fundamental difference between geometrical and physical idealization. In physical idealizations, the applicability to reality is always problematic, in the first place because the assumption of a limit for the idealizing limit process requires a justification through experience and therefore can only be shown as highly probable at best. By contrast, in geometrical idealizations the limiting entities are given to us in pure intuition, which guides the process of geometrical idealization; here the existence of a limit is for us certain, independently of experience.

The independence from experience is not to be understood in the sense of pure immanence, so that one should, e. g., distinguish the *a priori* validity of geometry for intuition from the validity of "real" (physical) space. Rather, Nelson states explicitly—in this respect too, a true follower of Kant: "We know only *one* space. This is the space of geometry and in which physical bodies are."

Accordingly, the laws of geometry are binding for physics. They form the framework within which all natural science is bound, and only through which does the task of physical research receive its determination. This is because, as Nelson explains, if one makes geometry itself an object of experimental control, then one loses the possibility of drawing definite conclusions from physical observations. For, given a new observation, one can never know whether it expresses a previously unknown feature of space or some other physical fact. Nelson elucidates this by the following example. Let us assume that, when the Earth was thought to be a disk, one had established that the sum of the angles of earthly triangles was larger than two right angles; then one could equally have concluded from this result, according to the empirical conception of geometry, either a non-Euclidean property of space or the spherical shape of the earth.

What is said here in particular about geometrical laws similarly applies to all those laws which, according to the Kantian doctrine, are taken from pure intuition, i. e. also the laws of time and the geometric doctrine of motion (kinematics).

Because of his conviction about the binding *a priori* character of these laws for the physical explanation of nature, Nelson opposed the new physics, whose characteristic feature consists precisely in the increased freedom from the necessity of integrating all physical facts into the framework of the *a priori* fixed, spatio-temporal ordering, which resulted in the distinguished position of the geometric-kinematic laws vis-a-vis the physical laws.

However, this change in the methodological conception of physics forms only a part of the philosophical impact originating from the more recent development of the exact sciences. Another important influence comes from research on the *foundations of arithmetic*. Nelson was actively involved n the development of this research.

Nelson was in close touch with the work resulting from *Cantorian set theory* through several members of the neo-Friesian school founded by him, especially Gerhard Hessenberg, who was one of the leaders in this development.

He dealt specifically with the *paradoxes of set theory*, the emergence of which he witnessed. These paradoxes had a special interest for Nelson because of their relation to certain dialectical modes of inference, which he often used for disproving antagonistic views — especially by showing an "introjected" contradiction, i. e. a contradiction which occurs in such cases where accepting the validity or insightfulness of a posited general claim already gives a counterexample to its validity.

The essay "Remarks on the paradoxes of Russell and Burali-Forti" (*vide* [?]), composed by Nelson together with Grelling, does not claim to solve the paradoxes; it served to state them more precisely and sharpen the given range of problems and reject unsatisfactory solutions. It is here that the very concise paradox related to the word "heterological" was presented for the first time.

Nelson was critical of attempts to found mathematics by pure logic. By contrast, he had a deep and active sympathy for the Hilbertian enterprise of a new foundation of mathematics. In this way of founding mathematics, Nelson welcomed the realization of the methodological principle of a *separation of critique and system*, i. e. the complete dissociation of the foundational procedure from the systematic deductive construction of mathematics, and the associated epistemological distinction between proper mathematical facts and "meta-mathematical" facts which have to be shown by the foundation. This agreement of the Hilbertian approach with the basic ideas of his own methodology, following Fries, was a source of great satisfaction for Nelson. Even shortly before the end of his life he expounded in a paper (56th convention of German philologists and schoolmen, Göttingen, September 1927) the methodological kinship of the Hilbertian foundation with the Friesian critique of reason.

There is, however, still another feature relating the Hilbertian foundation of mathematics to Nelson's philosophy: the "finitist attitude" demanded by Hilbert as methodological foundation must be characterized epistemologically as some sort of *pure intuition*, because, on the one hand, it is intuitive and, on the other hand, it goes beyond what can actually be experienced.

The prerequisite of such a foundation of knowledge is, as such, still independent of the special nature of the Hilbertian conception; it holds for any finitist foundation of mathematics. A characteristic feature of the Hilbertian foundation, however, is that here the *finitist standpoint is related to the axiomatic foundation of the theoretical sciences*. The conditions of the finitist attitude present themselves thereby as the conditions *for the possibility of theoretical knowledge of nature*, quite in the sense of the Kantian formulation of the problem.

Once this connection is generally recognized, it will be possible for the basic ideas of the Kantian critique of pure reason to be revived in a new form, detached from its particular historical conditions, from whose bounds theoretical science has freed itself.

Such a methodological clarification can help contribute to restoring what was correct in the rational tendencies that were always advocated by Nelson, but which are so one-sidedly disregarded today.

Chapter 6

Bernays Project: Text No. 8

The Basic Notions of Pure Geometry in Their Relation to Intuition (1928)

Die Grundbegriffe der reinen Geometrie in ihrem Verhältnis zur Anschauung

(*Die Naturwissenschaften* 16, pp. 197–203)

Translation by: *Volker Peckhaus*
Revised by: *CMU*
Final revision by: –

A discussion of the relation between axiomatic geometry and intuition can be carried out from very different perspectives and on the basis of different epistemological assumptions.

The present book, written by Richard Strohal with the essential collaboration of Franz Hillebrand, sets out to emphasize a certain methodological and epistemological view of geometry. The introduction states that the "psychological prehistory" of geometrical concepts and principles is the subject of the investigation. In fact, however, the more specific elaboration of the program shows already that it does by no means concern questions of genetic psychology, but questions such as: In what way do we have to recur on intuition when introducing geometrical concepts; what role does intuition play for the formation of basic concepts and complex concepts as well as for setting up the principles of geometry; and how do we have to evaluate then the epistemological character of these principles?

In this connection the author does not at all intend to make geometry appear as being determined to the greatest possible extent by intuition.

On the one hand Strohal, as he mentions in the beginning, wants to leave the question of application to "our space" completely aside (he does not, in fact, go to such an extreme); he is concerned with the foundations of *pure* geometry. A foundation of geometry through spatial experience is out of question for him. But he also excludes a rational foundation based on an appeal to an aprioristic evidence of geometrical intuition, because he does not accept any aprioristic evidence other than the analytic one and does not attribute any rational character to intuition. He does not enter into a closer discussion of the concept of "intuition," but begins with the view—which he takes as self-evident, as it were, and which is admittedly also common among exact researchers—that intuition is neither capable of giving us perfectly clear objects nor of presenting us with a relation as necessary, so that all idealizations and all insights of strict generality come about only by way of conceptual abstraction.

Considering his epistemological position one should now think that Strohal would welcome the standpoint of Hilbert's formal axiomatics as being in accord with his views and his intention. But in fact he agrees by no means agrees with this modern axiomatics; he rather explicitly opposes it and, in particular, Hilbert's foundation of geometry.

It is difficult to explain comprehensibly and in a few words how Strohal intends to deal with geometry because in his conception different intentions are at play. In any case, this present attempt to dissent in principle from the current standpoint of axiomatics and to go back to older tendencies may at first sight seem appealing to some, but it is, on closer inspection, only suited to bring our current standpoint into brighter light, and to make clear the justification of the motives from which it arose in a particularly precise way. But especially from this point of view it seems not to be useless to present the main points of Strohal's views and to discuss his presentation critically.

Strohal deals in particular detail with the *formation of concepts*. First of all, the role of intuition, according to Strohal, consists in the following:

1. Elementary concepts are obtained from intuition by processes of abstraction.

2. Intuition serves as a cause (*causa occasionalis*) for the formation of complex concepts (for "synthetical definitions") by suggesting the formation of certain conceptual syntheses. This is done in the following way: sharp definitions are obtained by combining elementary concepts and replace intuitive concepts, i. e., concepts directly taken from intuition (like the intuitive concept of a straight line or of the circle); the extension of a concept formed in this way does not have to coincide completely with the corresponding intuitive concept.

For one thing, we have to take into account here that the intuition under consideration by no means always has to be spatial intuition according to Strohal, e. g., the elementary concept of *congruence*, which he identifies in the style of Bolyai with "indistinguishability except for location," is obtained in the way that "the intuitive givenness of indistinguishable qualities, colors, sounds, odors etc." leads first to a vague concept of indistinguishability (equality); from this we get then the rigorous concept of indistinguishability as a limiting concept by a process of abstraction (pp. 71–72).

It is above all essential, however, that we are not free, according to Strohal, to introduce as an elementary concept just any concept that has been obtained from intuition by abstraction. He rather claims that: a concept may be regarded as an elementary one only "if an entity falling under the extension of the respective concept cannot also be given by conceptual marks," or in a more succinct formulation: "When it is at all *possible* to define a concept explicitly, then one *has to* define it."

This "criterion" is of course completely indeterminate; since the possibility of explicitly defining a concept depends essentially on the choice of geometrical principles, and the selection of principles depends on the choice of elementary concepts.

The motivation for the criterion is also quite unsatisfactory. Strohal asserts that the explanation of a concept has to make it possible "to decide whether an object which is given in some way falls under the extension of the respective concept or not" (p. 18). For instance, we have to be able to decide whether the geometrical location of all points equidistant from two fixed points A, B falls under the extension of the concept of a straight line; such a task would be hopeless, he thinks, if one would regard the concept of a straight line as a basic concept (p. 19). Again Strohal does not take into account that the extensional relations between geometrical concepts are only determined by the principles of geometry and that they, on the other hand, can make it possible also to prove a complex concept to be extensionally equivalent to an elementary concept. Lacking a more immediate justification, he says "obviously."

Despite the indeterminacy of the criterion, the aim pursued with it can be recognized: Geometry should—like a philosophical science—advance in its formation of concepts from the highest generality to the particular by way of conceptual synthesis. It must therefore not take the concepts of particular geometrical entities as elementary concepts, but only those of an entirely general character.

Because of this methodological demand, Strohal is forced to depart completely from the well-known elementary construction of geometry as it can be found in Euclid and, in a similar form, also in Hilbert's *Foundations*. He finds a formation of geometrical concepts in line with his principle in Lobachefsky and Bolyai. He follows these two, especially Lobachefsky, when introducing the elementary concepts. On the basis of an exhaustive discussion he arrives at the following system of elementary concepts:

1. the spatial (spatial formations);

2. the contact (the adjoining);

3. the "having-it-inside" (the part-whole relation);

4. the congruence (indistinguishability except for location).

Obviously we are here dealing with a construction of geometry according to which the *topological* properties of space have precedence and only then their is *metric* introduced. This method of constructing geometry and its systematic advantages are familiar to the mathematician—especially since the investigations of Riemann and Helmholtz[1] on the foundations of geometry. He will not be satisfied, however, with having only this kind of foundation available. In particular, the usual elementary foundational approach has the great methodological advantage that geometry, like elementary number theory, starts here by considering certain simple, easily comprehensible objects, and that one does not need to introduce the concept of continuity and limit

1[1] Helmholtz's group-theoretic conception, which was carried further by Lie and Hilbert, is however not in line with Strohal's intention (as will be seen from the following). The "derivation of the elementary spatial concepts from that of equality" sketched by Weyl (in the first paragraph of his book *Space, time, matter* (*vide* [**?**]) is more in accord with it.

processes from the outset. In any event one will insist on the freedom to choose the basic concepts relative to the viewpoint according to which geometry is carried out.

Strohal concedes, however, that it is in principle possible that systems other than the one he gives "connect with intuition immediately in a different manner, i. e., are based on other elementary concepts" (p. 63). But, he rejects almost all other foundational approaches.

In his opinion, e. g., the concept of a straight line should not be taken as a basic concept.[2] He also deliberately avoids introducing the point as a basic element. In his system the point is defined as the common boundary of two lines which touch each other; the line results accordingly from two touching surfaces and the surface from two touching solids.

He completely rejects the idea of taking the *concept of direction* as an elementary concept. He declares that if one intends to use the concept of direction for defining the straight line, this would "only be possible by considering the concept 'equidirected' as an elementary concept, which is not further reducible, and thus connects to the intuition of 'straightness' itself. That is to say, since no intuition can yield this elementary concept other than that of an intuitive straight line, this amounts to regarding the straight line itself as an elementary concept." (p. 56). By contrast, one should remark that one can obtain different directions starting from a point intuitively independent of the idea of straightness by considering different parts of the visual field and by the imaginations of directions connected to our impulses of motion. And moreover, as far as comparison of directions starting from *different* points is concerned, Strohal, according to his methodological principles would have to accept their synthetic introduction by linking the concept of direction with the concept of "indistinguishability," since he arrives at the comparison of lengths of segments in different locations in a very similar way. In particular the pure closeness geometry discovered by Weyl has recently clarified that, indeed, the *a priori* comparability of separate segments is by no means more easily comprehensible than the comparability of directions starting from distinct points. Here Strohal only repeats an old prejudice.

Strohal also rejects the characterization of the relation of congruence by the concept of *rigid motion* as a circular procedure. "The concept of a rigid

2[1] Incidentally, Strohal considers a straight line only as a spatial object, or straightness as a property of a line. He does not consider at all the possibility of introducing collinearity as a relation between *three* points.

solid which occurs in this connection can again be explained in no other way than by presupposing the congruence of the different positions of this solid. If one wants to understand the rigid solid as an elementary concept, however, one will find that to obtain it no other intuitions will help than those which give us the concept of congruence itself, so that the detour through the concept of a rigid solid becomes pointless" (pp. 17–18). This argumentation would be justified only if the concept of a rigid solid would have to be formed as an ordinary generic concept, e. g., in such a way that starting from an empirical representation of the rigid solid one arrives by abstraction at the concept of the perfectly rigid solid. It is in fact possible to carry out instead a completely different abstraction process, which consists in sharpening by abstraction the intuitive facts about rigid bodies concerning freedom of motion and coincidence into a strict lawfulness, and then forming the geometrical concept of a rigid solid with respect to this *lawfulness*. In its mathematical formulation, this kind of concept formation emerges by considering rigid motions from the outset not individually, but by considering *the group of rigid motions*.

This thought, which goes back to Helmholtz, was groundbreaking for an entire line of geometrical research, and is increasingly topical because of the theory of relativity. It is not mentioned by Strohal at all.

Now, if so many approaches adopted by mathematics in order to erect geometry are rejected, one would expect that the way of justification so decisively preferred by Strohal would be presented as a paradigm of methodology. In fact, however, the considerations by means of which Strohal following Lobachefsky explains the method—leading from the elementary concepts of the spatial, contact and of the having-it-inside to the distinction of dimensions and to the concepts of surface, line and point—are far removed from the precision we are now used to when dealing with such topological questions; on the basis of these considerations one cannot even determine whether those three elementary concepts are sufficient for the topological characterization of space.—

Up to now we have only regarded the part of Strohal's considerations that deals with geometrical *concept formation*. Strohal's standpoint, however, becomes really clear only through the way in which he conceives of the *principles* of geometry.

It is essential to this view that Strohal sticks to the separation of the *κοιναὶ ἔννοιαι* (*communes animi conceptiones*) and the *αιτήματα* (*postulata*) as it is found in Euclid's *Elements*. Strohal regards this distinction as fundamentally significant, and sees an essential shortcoming of recent foundations of geometry in their deviation from this distinction.

Here it has to be remarked first of all that deviating from Euclid on this point is not a result of mere sloppiness but is completely intentional. Euclid puts the propositions of the *theory of magnitude*, which are gathered under the title *κοιναὶ ἔννοιαι*before the *specifically geometrical* postulates as propositions of greater than geometrical generality, which are to be *applied* to geometry.

The kind of application, however, leads to fundamental objections since the subordination of geometrical relations under the concepts occurring in the *κοιναὶ ἔννοιαι* is tacitly presupposed in several cases where the possibility of such a subordination represents a geometrical law that is by no means self-evident.

Hilbert, in particular, has criticized Euclid's application of the principle that the whole is greater than the part in the theory of the areas of plane figures in this way—an application which would only be justified, if one could presuppose without a second thought that one could assign to every rectilinear plane figure a positive quantity as its area (in such a way that congruent figures have the same area and that by joining surfaces the areas add up).[3]

Considering such a case one recognizes that the essential point in applying the *κοιναὶ ἔννοιαι* always lies in the conditions of applicability. If these conditions are recognized as satisfied, the application of the respective principle in most cases becomes entirely superfluous, and sometimes the proposition to be proved by applying the general principle belongs itself to these conditions of applicability.

Putting the *κοιναὶ ἔννοιαι* at the beginning therefore appears to be a continuous temptation to commit logical mistakes and it is more suited to obscure the true geometrical state of affairs than to make it clear, and this is the reason why this method has been completely abandoned.

Strohal seems to be ignorant of these considerations; in any case he does not mention Hilbert's criticism with even a syllable. He aims at emphasizing again the distinction between the two kinds of principles. In particular, this appears to him to be necessary because, in his opinion, the *κοιναὶ ἔννοιαι* have a completely different epistemological character than the postulates,

[3][1] Hilbert has shown that this presupposition in fact need not always be satisfied by constructing a special "non-Archimedian" and "non-Pythagorean" geometry.

namely that of evident analytic propositions, whereas postulates are not expressions of knowledge at all; they are only *suggested* to us by certain experiences.

Strohal therefore calls the κοιναὶ ἔννοιαι the "proper axioms." He considers it a particular success of his theory of geometrical concept formation that it makes the analytic nature of the κοιναὶ ἔννοιαι comprehensible. He finds this comprehensibility in the fact that these axioms, as propositions each concerned with a single elementary relation, have the sense of an *instruction* and specify from which relational intuitions one has to abstract the elementary concept "in order to turn the axiom concerned into an identical proposition" (p. 70). This characterization amounts to the claim that the axioms in question constitute logical identities based on the contentual view of the elementary concepts.

It seems curious that such geometrically empty propositions should be regarded as "proper axioms" of geometry, and one wonders furthermore to what end one needs to posit specifically these propositions as principles at all, since the elementary concepts are introduced contentually anyway.

For instance, one of these axioms is the proposition that if a is indistinguishable from b and b from c, then a is indistinguishable from c. This proposition is, because of the meaning of "indistinguishability," a consequence of the purely logical proposition: if two things a, b behave the same with respect to the applicability or non-applicability of a predicate P and also b, c behave in this respect the same, then a and c also behave in this respect the same.

We now have the following alternative: Either the concept "indistinguishable" is used in its contentual meaning, then we have before us a proposition which can be understood purely logically, and there is no reason to list such a proposition as an axiom, since in geometry we regard the laws of logic as an obvious basis anyway. Or else the concept "indistinguishable" and also the other elementary concepts will not be applied contentually at all; rather, only concept *names* are introduced initially, and the axioms give certain *instructions* about their meaning. Then we are taking the standpoint of formal axiomatics, and the κοιναὶ ἔννοιαι are nothing else than what are called *implicit definitions* following Hilbert.

Those places where Strohal stresses that the κοιναὶ ἔννοιαι do not provide "proper definitions" or "explicit definitions" of elementary relations (pp. 68 and 72) indicate that this is indeed Strohal's view—who very carefully avoids using the term "implicit definition" anywhere.

From this standpoint it is not appropriate, however, to ascribe to the axioms in question the character of *being evident*. They simply constitute *formal conditions* for certain initially indeterminate relations, and then there is also no principled necessity of separating these axioms from the "postulates."

So either the setting up of the axioms, which according to Strohal have the role of *κοιναὶ ἔννοιαι*, is altogether superfluous, or the separation of these axioms as analytically evident propositions from the other principles is not justified.

Furthermore, however, we find the same ills that discredited Euclid's *κοιναὶ ἔννοιαι* again in the application of these axioms in Strohal: the formulation of these propositions, which can easily be confused with geometrically contentful propositions, leads to logical mistakes, and these are in fact committed.

Two cases are especially characteristic. 1. As an example of a proper axiom the proposition is given[4] that in a "cut," i. e., when two adjoining parts of a solid (spatial entity) touch, one always has to distinguish *two sides of the cut* (p. 64). This proposition is tautological, however, since as the two adjoining parts are called "sides" of the cut (p. 23), it says nothing but that if two parts of a solid touch each other (adjoin), two adjoining parts have to be distinguished. This proposition, moreover, is completely irrelevant for geometry. But, it seems to state something geometrically important, since given the wording one thinks of another proposition which expresses a topological property of space.

The following mistake shows that Strohal himself is not immune to confusions of a similar kind. He raises the following question (when discussing the concept of congruence): "Is it possible to find two solids connected by a continuous series of such solids which have one and the same surface in common, i. e., which all touch in *one* surface?" "We have to answer this question in the negative," he continues, "because it follows from the explanation of a surface that only *two* solids are able to touch each other in one and the same surface" (pp. 42–43).

2. The famous axiom: "The whole is greater than the part," which became, as mentioned, the source of a mistake for Euclid, is interpreted by Strohal in the following way: The axiom hints at an elementary concept "greater," "which can be obtained by abstraction from a divided solid." The

4[1] In this example Strohal follows some considerations of Lobachefsky.

procedure of abstraction is characterized "by examining that relation which obtains between the totality of all subsolids (the *whole*) and one of them (the *part*). For the concept "greater" obtained this way, the proposition *Totum parte maius est* is an identity" (p. 77). Here we disregard that in this interpretation the "whole" is wrongly identified with the totality of all part-solids. In any case, it follows from this interpretation that the proposition "a is greater than b" is only another expression for b being a part of a. So we have again a perfect tautology, from which one can infer nothing for geometry; in particular it is impossible to derive from this the proposition that a body cannot be congruent with one of its parts—which also follows from the fact that this proposition is generally valid only under certain restrictions anyway. (For instance, a half line can turn into a part by a congruent translation, and equally a spatial octant into a suboctant by a congruent translation.)

In fact, however, Strohal would have to have some formulation of this proposition at his disposal for the theory of congruence—which he, however, does not develop in this respect; for otherwise it would not be certain that this "indistinguishability disregarding location" does not just mean *topological equality.* Indeed, in the conceptual system that Strohal takes as a basis—the first three elementary concepts, spatial object, adjoining, having-it-inside—all belong to the domain of topological determinations, and only by the concept of congruence the *metric* is introduced into geometry. Therefore, the concept of congruence must contain a *new distinguishing property* besides the element of correspondence. In the concept of indistinguishability disregarding location[5] such a distinguishing property, however, is not really given; for this one also needs a principle according to which certain objects, that are at the outset only determined as different with respect to the position but not as topologically different, can also be recognized as *distinguishable disregarding location.* In other words: it is important to introduce *difference in size.* The principle that the whole is greater than the part should actually help us achieve this. This will be impossible, however, if we interpret the proposition in the way Strohal does; because from this interpretation it cannot be derived that an object a which is greater than b is also *distinguishable* from it, even with respect to location.

This circumstance perhaps escaped Strohal; for otherwise he would have

5[1] The "location" of a solid is, according to the definition Strohal took from Lobachevsky with a certain revision (pp. 24 and 93), synonymous with the boundary of the solid.

realized the fact that his concept of indistinguishability disregarding location does not yet yield geometrical congruence. Thus, we find here a gap very similar to that in Euclid's doctrine of the area.

The result of this consideration is that the method of putting the *κοιναὶ έννοιαι* first becomes even more objectionable through the modified interpretation given to it by Strohal; in any case, it does not appear to be an example that should be followed.

At the same time Strohal's characterization of these axioms has led us to assume that he does not keep the contentual view of elementary concepts even within geometry itself or, as the case may be, he does not make use of it for geometrical proofs. This assumption is confirmed by Strohal's discussion of the *postulates* of geometry.

According to Strohal we are *forced* neither by intuition nor by logical reasons to posit the postulates, "but are caused [to do so] by certain experiences" (p. 97). For pure geometry they have the meaning of stipulations; they are "tools for defining geometrical space, their totality forms the definition of geometrical space" (p. 103). They are characterized contentually as "excluding certain combinations of elementary concepts, which are *a priori* possible" (p. 103).

The point of this characterization emerges from Strohal's view of the deductive development of geometry. According to Strohal, this development proceeds by a continued combination of properties, i. e., by forming synthetical definitions. In forming the first syntheses one is only bound by those restrictions resulting from the *κοιναὶ έννοιαι*. "Incidentally, one can proceed completely arbitrarily in combining elementary concepts," i. e., the decision "whether one wants to unite certain elementary concepts in a synthesis or to exclude such a union," is caused by motives, "which lie outside of pure geometry." "However, in arbitrarily excluding the existence of a certain combination, one introduces a proposition into pure geometry which has to serve as a norm for further syntheses. propositions of this kind are called requirements, *αιτήματα*, postulates." "In forming higher syntheses" one has to show that these "do not contradict the postulates already set up. One must, as we say concisely, prove the *possibility*, the *existence*, of the defined object. Here, existence and possibility mean the same, and amount to nothing but *consistency* with the postulates" (pp. 98–99 and p. 102).

What is most striking in this description of the geometrical method is that here, contrary to all familiar kinds of geometrical axiomatics, only a *negative* content is ascribed to the postulates, namely that of excluding possibilities, whereas all existential propositions in geometry are only interpreted as statements about consistency.

Strohal's view is in accord with the views of his philosophical school; these views include Brentano's theory of judgement as an essential element. According to this theory, all general judgements are negative existential judgements whose content is that the matter of a judgement (a combination of the contents of ideas is rejected (excluded).

In fact every general judgement can be brought into this logical form. By producing such a normal form, however, the existential moment is not removed, but only transferred into the formation of the matters of judgements.

One thus also does not succeed in geometry in excluding existential claims completely or in reducing them to consistency claims. One can only hide an existential claim by a double application of negation. Strohal proceeds in this way for instance when he speaks of an $\alpha\iota\tau\eta\mu\alpha$ which excludes the assumption "that when dividing a geometrical solid no parts can ever be congruent" (p. 93). We find another such example in his discussion of Dedekind's continuity axiom. After having spoken of the divisions of a line segment AB which has the cut property, and furthermore of the creation of a cut by a point C, he continues: "In *excluding* the possibility of such a division of some line segment AB on which such a point C is not found, I assert the $\alpha\iota\tau\eta\mu\alpha$ of continuity for the line segment" (p. 113). Talk of "occurring," "being found," or "existence" all amount to the same thing. And in any case here, where the formulation of of postulates is concerned, the interpretation of existence in the sense of consistency with postulates is not permissible.

The identification of existence and consistency is justifiable in two senses: first, with respect to geometrical space whose existence indeed only consists in the consistency of the postulates defining it; and second also with respect to geometrical objects, but only under the condition of the *completeness of the systems of postulates.*

If the system of postulates is complete, i. e., if, the postulates already decide, for every combination (every synthesis) of elementary concepts whether they are permitted or excluded, then indeed the possibility (consistency) of an object coincides with its existence.

However, as long as one is in the process of obtaining a system of postulates, i. e., of the stepwise determination of geometrical space, one has to distinguish between existence and consistency. From the proof of the consistency of a synthesis it only follows that it agrees with the postulates *already set up*; it may nevertheless be possible to exclude this synthesis by a further postulate. By contrast, an *existence proof* says that already by the prior postulates one is logically *forced* to accept the respective synthesis.

Let us take as an example "absolute geometry," which results from ordinary geometry by excluding the parallel axiom. In this geometry one can assume, without contradiction with the postulates, a triangle with an angular sum of a right angle; if we would identify consistency with existence in this context, we would get the proposition: "In absolute geometry there is a triangle with the angular sum of one right angle." Then the following proposition would equally hold: "In absolute geometry there exists a triangle with an angular sum of two right angles." Hence, in absolute geometry both a triangle with an angular sum of a right angle and one with an angular sum of two right angles would have to exist. This consequence contradicts, however, a theorem proved by Legendre according to which in absolute geometry the existence of a triangle with an angular sum of two right angles implies that *every* triangle has this angular sum.

In order, therefore, to characterize the existence of geometrical objects by the consistency with the postulates, as Strohal intends to do, one has to have a *complete* system of postulates for which no decision concerning the admission of a synthesis remains open. This prerequisite of completeness is not mentioned by Strohal anywhere, and furthermore, it does not follow from his description of the progressive method of forming and excluding syntheses whether this way ever comes to a conclusion.

Disregarding all these objections, however, which concern the special kind of characterization of the postulates and of the progressive method of obtaining them, it has to be remarked above all that, according to the description of geometry which Strohal gives here in the section on the postulates, geometry turns out to be pure conceptual combinatorics,—such as it could not be performed in a more extreme way in formal axiomatics: Combinations of elementary concepts are tried out; in doing so the content of these concepts is not taken into account, but only certain axioms representing this content which act as initial rules of the game. Moreover, certain combinations are excluded by arbitrary stipulations, and now one stands back and sees what remains as possible.

Here, the detachment from the contentual formation of concepts is executed to the same degree as in Hilbert's axiomatics; the initial contentual introduction of elementary concepts does not play a role in this development; it is, so to speak, eliminated with the help of the *κοιναὶ ἔννοιαι*.

Thus we have here—similar to Euclid's foundation of geometry—the state of affairs that the contentual determination of the elementary concepts is completely idle, i. e., precisely that state of affairs for the sake of which one refrains from a contentual formulation of the elementary concepts in the newer axiomatics.

In Euclid's foundation, however, the state of affairs is different insofar as here the postulates are still given in an entirely intuitive way. In the first three postulates the close analogy with geometrical drawing is especially apparent. The constructions required here are nothing but idealizations of graphical procedures. This contentual formulation of the postulates permits the interpretation according to which the postulates are positive existential claims concerning intuitively evident possibilities which receive their verification based on the intuitive content of the elementary concepts. For Strohal, such a standpoint of *contentual axiomatics* is out of the question, since he considers an intuitively evident verification of the postulates to be impossible and therefore he can admit only the character of stipulations for the postulates.

So Strohal's sketch of the geometrical axiomatics ends in a conflict between the intuitive introduction of concepts and the completely non-intuitive way in which the geometrical system is to be developed as a purely conceptual science starting from the definition of geometrical space given by the postulates,—a discrepancy which is barely covered by the twofold role of the the *κοιναὶ ἔννοιαι*, [which function] on the one hand as analytically evident propositions, on the other hand as initial restrictive conditions for conceptual syntheses.

In the light of these unsatisfying results one wonders on what grounds Strohal rejects the simple and systematic standpoint of Hilbert's axiomatics. This question is even more appropriate as Strohal knows full well the reasons leading to Hilbert's standpoint. He himself says: "The intuitions representing the *causa occasionalis* for forming the syntheses, do not enter ... into geometry in the sense that one could immediately prove a proposition correct by referring to intuition;" moreover, shortly thereafter: "As soon as the axioms"—Strohal is here only referring to the *κοιναὶ ἔννοιαι*—"are formulated, the specific nature of elementary concepts has no further influence on the development of geometrical deduction" (pp. 132–133).

Indeed, there are also no conclusive objections in Strohal's polemic against Hilbert's foundation of geometry, which can be found in the final section of his book.

Here his main argument is that in Hilbert's conception of axiomatics the contentual element is only *pushed back* to the formal properties of the basic relations, i. e., to relations of higher order. The formal requirements on the basic relations which are expressed in the axioms would have to be regarded contentually [so Strohal] and the contentual representations necessary for this could again be obtained only by abstraction from the appropriate relational intuitions. Thus, concerning the higher relations which constitute the required properties of the basic geometrical relations, one "has arrived at the reference to intuition which axiomatics precisely wants to avoid" (p. 129).

This argument misses the essential point. What is to be avoided by Hilbert's axiomatics is the reference to *spatial intuition*.

The point of this method is that intuitive contents is retained only when it *essentially enters into* geometrical proofs. By satisfying this demand we free ourselves from the special sphere of ideas in the subject of the spatial, and the only contentual representation we use is the primitive kind of intuition which concerns the elementary forms of the combination of discrete, bounded objects, and which is the common precondition for all exact scientific thinking—which was stressed in particular by Hilbert in his recent investigations on the foundations of mathematics.[6]

This methodological detachment from spatial intuition is not to be identified with ignoring the spatial-intuitive starting point of geometry. It is also not connected with the intention—as Strohal insinuates—"to act as if these and exactly these axioms had been joined in the system of geometry by some inner necessity" (p. 131). On the contrary, the names of spatial objects and of spatial connections of the respective objects and relations are maintained deliberately in order to make the correlation with spatial intuitions and facts evident, and to keep it continuously in mind.

The inadequacy of Strohal's polemic becomes especially apparent when he goes on to create artificially an opportunity for an objection. While reporting on the procedure of proving the consistency of the geometrical axioms, he states: "For this purpose one chooses as an interpretation, e. g., the concepts of ordinary geometry; by this Hilbert's axioms transform into certain propositions of ordinary geometry whose compatibility, i. e., consistency is already established independently. Or one interprets the symbols by numbers or functions; then the axioms are transformed into certain relations of numbers whose compatibility can be ascertained according to the laws of

6[1] Cf. especially the treatise "New foundation of mathematics" (*vide* [**?**]).

arithmetic" (p. 127).

Strohal added the first kind of interpretation himself; in Hilbert there is not a single syllable about an interpretation by "ordinary geometry." Strohal nevertheless has the nerve to connect an objection to Hilbert's method with this arbitrarily added explanation: "If one, say, proves the consistency of Hilbert's axioms by interpreting its "points," "lines," "planes" as points, lines, planes of Euclidean geometry whose consistency is established, then one presupposes ... that these objects are already defined elsewhere" (p. 130).

On the whole one gets the impression that Strohal, out of a resistance against the methodological innovation given by the formal standpoint of axiomatics compared with the contentual-conceptual opinion, rejects the acceptance of Hilbert's standpoint instinctively.

Strohal exhibits this attitude, however, not only against Hilbert's axiomatics, but also against most of the independent and important thoughts that recent science has contributed to the present topic. This spirit of hostility is expressed in the book under review not only by how it divides praise and criticism, but even more in the fact that essential achievements, considerations and results are simply ignored. For instance (as already mentioned earlier), Strohal passes over in complete silence the famous investigation of Helmholtz, which concerns the present topic most closely, and likewise Kant's doctrine of spatial intuition. And as to the strict mathematical proof of the independence of the parallel axiom from the other geometrical axioms, Strohal presents this as if it were still an unsolved problem: "This question will finally be clarified only if one shows that no consequence of the other postulates *can* ever be in conflict with the denial of the parallel postulate" (p. 101). And this statement cannot be explained away by ignorance for, as can be seen from other passages, Strohal knows of Klein's projective determination of measure, and is also familiar with Poincaré's interpretation of non-Euclidean geometry by spherical geometry within Euclidean space (from a review by Wellstein). The explanation instead is to be found in Strohal's oppositional emotional attitude, which refuses to appreciate the significance of the great achievements of recent mathematics.

A naive reader can thus only receive a distorted picture of the development of geometrical science from Strohal's book. Those who are informed about the present state of our science might take Strohal's failed enterprise, in view of the various methodological tendencies that work together in it, as an opportunity to think through anew the fundamental questions of axiomatics.

Chapter 7

Bernays Project: Text No. 9

The Philosophy of Mathematics and Hilbert's Proof Theory
(1930)

Die Philosophie der Mathematik und die Hilbertsche Beweistheorie

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Part I: The Nature of Mathematical Knowledge

When we read and hear today about the foundational crisis in mathematics or the dispute between "formalism" and "intuitionism," those unfamiliar with the activities of mathematical science may think that this science is shaken to its very foundations. In reality, mathematics has long been moving in such quiet waters that one notices instead the absence of stronger impulses, although there has been no shortage of significant systematic advances and brilliant achievements.

In fact, the current discussion of the foundations of mathematics does not spring at all from a predicament of mathematics itself. Mathematics is in a completely satisfactory state of methodological certainty. In particular, the concern raised by the set theoretic paradoxes has long been overcome, since it was recognized that, to avoid the known contradictions, it suffices to impose restrictions that do not in the least impinge on the demands made on set theory by mathematical theories.

The problems, the difficulties, and the differences of opinion really begin only when one inquires not just about the mathematical facts, but about the epistemological foundation and the demarcation of mathematics. These philosophical questions have become particularly urgent since the transformation, which the methodological approach of mathematics underwent towards the end of the nineteenth century.

The characteristic aspects of this transformation are: the emergence of the concept of set, by means of which the rigorous foundation of the infinitesimal calculus was achieved, and further the rise of existential axiomatics, that is, the method of developing a mathematical discipline as the theory of a system of things with certain relations whose properties constitute the content of the axioms. To these we must add, as a consequence of the two aforementioned aspects, a closer connection between mathematics and logic is established.

This development confronted the philosophy of mathematics with a completely new situation and entirely new insights and problems. Since then no agreement has been reached in the discussion of the foundations of mathematics. The present stage of this discussion is centered around the struggle with the difficulties that are caused by the role of the infinite in mathematics.

The problem of the infinite, however, is neither the first nor the most general question which one has to address in the philosophy of mathematics. Here, the first task is to gain clarity about what constitutes the specific nature of mathematical knowledge. We intend to address this question first, and also to recall the development of the different points of view, but only in a rough way and without their exact chronological order.

§ 1 The Development of Conceptions of Mathematics

The older conception of mathematical knowledge proceeded from the division of mathematics into arithmetic and geometry; according to it mathematics was characterized as a theory of two particular kinds of domains, that of numbers and that of geometric figures. This division could no longer be maintained in the face of the rise to prominence of arithmetical methods in geometry. Also geometry was not restricted to the study of the properties of figures but was broadened to a general theory of manifolds. The completely changed situation of geometry found a particularly concise expression in Klein's Erlangen Program, which systematically summarized the various branches of geometry from a group-theoretical point of view.

In the light of this situation the possibility arose to incorporate geometry into arithmetic. And since the rigorous foundations of the infinitesimal calculus by Dedekind, Weierstrass, and Cantor reduced the more general concepts of number—as required by the mathematical theory of quantities (rational number, real number)—to the usual ("natural") numbers $1, 2, \ldots,$ the conception emerged that the natural numbers constitute the true object of mathematics and that mathematics is precisely the *theory of numbers*.

This conception has many supporters. In its favor is the fact that all mathematical objects can be represented through numbers, or combinations of numbers, or through higher set formations obtained from the number sequence. From a foundational perspective the characterization of mathematics as a theory of numbers is already unsatisfactory, because it remains open what one considers here as essential to number. The question concerning the nature of mathematical knowledge is thereby shifted to the question concerning the nature of numbers.

This question, however, appears to be completely idle to the proponents of the conception of mathematics as the science of numbers. They proceed from the attitude common to mathematical thought, that numbers are a sort of things, which by their nature are completely familiar to us, so much so that an answer to the question concerning the nature of numbers could only consist in reducing something familiar to something less familiar. From this standpoint one sees the reason for the special status of numbers in the fact that numbers make up an essential component of the world order. This order is comprehensible to us in a rigorous scientific way just to the extent to which it is governed by the factor of number.

Opposing this view, according to which number is something completely absolute and final, there emerged soon, in the aforementioned epoch of the development of set theory and axiomatics, a completely different conception. This conception denies that mathematical knowledge is of a particular and characteristic kind and holds that mathematics is to be obtained *from pure logic*. One was led naturally to this conception through axiomatics, on the one hand, and through set theory, on the other.

The new methodological turn in axiomatics consisted in giving prominence to the fact that for the development of an axiomatic theory the epistemic character of its axioms is irrelevant. Rigorous axiomatics demands that in the proofs no other knowledge from the given subject be used than what is expressly formulated in the axioms. This was intended already by Euclid in his axiomatics, even though at certain points the program is not completely carried through.

According to this demand, the development of an axiomatic theory shows the logical dependence of the theorems on the axioms. But for this logical dependence it does not matter whether the axioms placed at the beginning are true sentences or not. It represents a purely hypothetical connection: If things are as the axioms say, then the theorems hold. Such a separation of deduction from asserting the truth of the initial statements is in no way idle hair splitting. On the contrary, an axiomatic development of theories, without regard to the truth of the fundamental sentences taken as starting points, can be of great value for our scientific knowledge: in this way, on the one hand, it is possible to test, in relation to the facts, assumptions of doubtful correctness by systematic development of their logical consequences; furthermore, the *possibilities of a priori theory construction* can be investigated mathematically from the point of view of systematic simplicity and, as it were, to develop a supply. With the development of such theories, mathematics takes over the role of the discipline formerly called *mathematical natural philosophy*.

By completely ignoring the truth of the axioms of an axiom system, the content of the basic concepts also becomes irrelevant, and thus one is lead to completely *abstract from all intuitive content of the theory*. This abstraction is further supported by a second feature, which comes as an addition to the newer axiomatics, as it was developed above all in Hilbert's *Foundations of Geometry*, and which is, in general, essential for the formation of recent mathematics, namely, the *existential* conception of the theory.

Whereas Euclid always thinks of the figures under consideration as constructed ones, contemporary axiomatics proceeds from the idea of a *system of objects*, which is fixed in advance. In geometry, for example, one conceives of the points, lines, and planes in their totality as such a system of things. Within this system one considers the relations of incidence (a point lies on a line, or in a plane), of betweenness (a point lies between two others), and of congruence as being determined from the outset. Now, regardless of their intuitive meaning, these relations can be characterized purely abstractly as certain *basic predicates*. (We will use the term "predicate" also in the case of a relation between several objects, so that we also speak of predicates with several subjects.)[1]

Thus, e. g., in Hilbert's system the Euclidean construction postulate, which demands the possibility of connecting two points with a line, is replaced by the existence axiom: For any two points there is always a straight line that belongs to each of the two points. "Belonging to" is here the abstract expression of incidence.

According to this conception of axiomatics, the axioms as well as the theorems of an axiomatic theory are statements about one or several predicates, which refer to the objects of an underlying system. And the knowledge provided to us by the proof of a theorem L, which is carried out by means of the axioms $A_1 \dots A_k$ (for the sake of simplicity we will assume that we are dealing here with only *one* predicate) consists in the realization that, if the statements $A_1 \dots A_k$ hold of a predicate, then so does the statement L.

What we have before us is, however, a very general proposition about predicates, that is, a proposition of pure logic. In this way, the results of an axiomatic theory, according to the purely hypothetical and existential understanding of axiomatics, present themselves as *theorems of logic*.

These theorems, though, are only significant if the conditions formulated in the axioms can be satisfied at all by a system of objects together with certain predicates concerning them. If such a satisfaction is inconceivable, that is, logically impossible, then the axiom system does not lead to a theory at all, and the only logically important statement about the system is then the observation that a contradiction results from the axioms. For this reason every axiomatic theory requires a proof of the *satisfiability*, that is, *consistency*, of its axioms.

Unless one can make do with direct finite model constructions, this proof is accomplished in general by means of the method of *reduction to arithmetic*, that is, by exhibiting objects and relations within the realm of arithmetic that satisfy the axioms to be investigated. As a result, one is again faced with the question of the epistemic character of arithmetic.

Even before this question became acute in connection with axiomatics,

[1]This terminology follows a suggestion of Hilbert. It has certain advantages over the usual distinction between "predicates" and "relations" for the conception of what is logical in principle and also agrees with the usual meaning of the word "predicate."

as just described, set theory and logistics had already taken a position on it in a novel way. Cantor showed that the number concept, both in the sense of cardinal number and in the sense of ordinal number, can be extended to infinite sets. The theory of natural numbers and the theory of positive real numbers (analysis) were subsumed as parts under general set theory. Even if natural numbers lost an essential aspect of their distinguished role, nonetheless, from Cantor's standpoint, the number sequence still constitutes something immediately given, the examination of which was the starting point of set theory.

This was not the end of the matter; rather, the logicians soon adopted the stronger claim: sets are nothing but extensions of concepts and set theory is synonymous with the logic of extensions, and,in particular, the theory of numbers is to be derived from pure logic. With this thesis, that mathematics is to be obtained from pure logic, an old and favorite idea of rational philosophy, which had been opposed by the Kantian theory of pure intuition, was revived.

Now the development of mathematics and theoretical physics had already shown that the Kantian theory of experience, in any case, was in need of a fundamental revision. As to the radical opponents of Kant's philosophy the moment seemed to have arrived for refuting this philosophy in its very starting point, namely the claim that mathematics is synthetic in character.

This [refutation], however, was not completely successful. A first symptom that the situation was more difficult and complicated than the leaders of the logistic movement had thought became apparent in the discovery of the famous *set-theoretic paradoxes*. Historically, this discovery was the signal for the beginning of the critique. If today we want to discuss the situation philosophically it is more satisfactory to consider the matter directly without bringing in the dialectical argument involving the paradoxes.

§ 2 The mathematical element in logic. – Frege's definitions of number

In fact in order to see what is essential we need only to consider the new discipline of theoretical logic itself, the intellectual achievement of the great logicians Frege, Schröder, Peano, and Russell, and see what it teaches us about the relation of the mathematical to the logical.

One sees immediately a peculiar two-sidedness in this relation which shows itself in a varying conception of the task of theoretical logic: Frege strives to subordinate mathematical concepts to the concept formations of logic, but Schröder, on the other hand, tries to bring to prominence the mathematical character of logical relations and develops his theory as an "algebra of logic."

But the difference here is only a matter of emphasis. In the various systems of logistic one never finds the specifically logical point of view dominating by itself; rather, in each case, it is imbued from the outset with the mathematical perspective. Just as in the area of theoretical physics, the mathematical formalism and mathematical concept formation prove here to be the appropriate means of representing interconnections and of gaining a systematic overview.

To be sure, it is not the usual formalism of algebra and analysis that is applied here, but a newly created calculus developed by theoretical logic on the basis of the formula language used to represent the logical connectives. No one familiar with this calculus and its theory will doubt its explicitly mathematical character.

Concerning this situation there arises first of all the requirement to delimit the concept of the mathematical, independently of the actual situation in the mathematical disciplines by means of a principled characterization of the nature of mathematical knowledge. If we examine what is meant by the mathematical character of a deliberation, it becomes apparent that the distinctive feature lies in a certain kind of abstraction that is involved. This abstraction, which may be called *formal* or *mathematical abstraction*, consists in emphasizing and taking exclusively into account the structural aspects of an object, that is, the manner of its composition from parts; "object" is understood here in its widest sense. One can, accordingly, define mathematical knowledge as that which rests on the *structural* consideration of objects.

The study of theoretical logic teaches us, furthermore, that in the relationship between mathematics and logic, the mathematical point of view, in contrast to the contentual logical one, is under certain circumstances the more abstract one. The aforementioned analogy between theoretical logic and theoretical physics extends as follows: just as the mathematical laws of theoretical physics are contentually specialized by their physical interpretation, so the mathematical relationships of theoretical logic are also specialized through their contentual logical interpretation. *The laws of the logical relations appear here as a special model for a mathematical formalism.*

This distinctive relation between logic and mathematics—not only can mathematical judgments and inferences be subjected to logical abstraction, but also logical relationships can be subjected to mathematical abstraction—is based on the special role of the formal realm with respect to logic. Namely, whereas in logic one can otherwise abstract from the specifics of a given subject, this is not possible in the formal realm, because *formal elements enter essentially into logic itself.*

This holds in particular for *logical inference*. Theoretical logic teaches that logical proofs can be "formalized." The method of formalization consists first of all in representing the premises of the proof by specific formulas in the logical formula language, and furthermore in the replacement of the principles of logical inference by rules that specify determinate procedures, according to which one proceeds from given formulas to other formulas. The result of the proof is represented by an end formula, which, on the basis of the interpretation of the logical formula language, presents the proposition to be proved.

Here we use that all logical inference, considered as a process, is reducible to a limited number of logical elementary processes that can be exactly and completely enumerated. In this way it becomes possible to pursue questions of *provability* systematically. The result is a field of theoretical inquiry within which the theory of the different possible forms of categorical inference put forward in traditional logic deals with only a very specific special problem.

The typically mathematical character of the theory of provability reveals itself especially clearly, through the role of the logical *symbolism*. The symbolism is here the *means for carrying out the formal abstraction*. The transition from the point of view of logical content to the formal one takes place when one ignores the original meaning of the logical symbols and makes the symbols themselves representatives of formal objects and connections.

For example, if the hypothetical relation

"if A then B"

is represented symbolically by

$$A \to B$$

then the transition to the formal standpoint consists in abstracting from all meaning of the symbol $\to$ and taking the connection by means of the "sign" $\to$ itself as the object to be considered. To be sure one has here a specification in terms of figures instead of the original specification of the connection in terms of content; this, however, is harmless insofar as it is easily recognized as an accidental feature. Mathematical thought uses the symbolic figure to carry out the formal abstraction.

The method of formal consideration is not introduced here at all artificially; rather it is almost forced upon us when we inquire more closely into the effects of logical inference.

If we now consider why the investigation of logical inference is so much in need of the mathematical method, we discover the following fact. In proofs there are two essential features which work together: the elucidation of concepts, the feature of *reflection*, and the mathematical feature of *combination*.

Insofar as inference rests only on elucidation of meanings, it is analytic in the narrowest sense; progress to something new comes about only through mathematical combination.

This combinatorial element can easily appear to be so obvious that it is not viewed as a separate factor at all. With regard to deductively obtained knowledge, philosophers especially were in the habit of considering only what is the precondition of proof as epistemologically problematic and in need of discussion, namely fundamental assumptions and rules of inference. This standpoint is, however, insufficient for the philosophical understanding of mathematics: for the typical effect of a mathematical proof is achieved only after the fundamental assumptions and rules of inference have been fixed. The remarkable character of mathematical results is not diminished when we modify the provable statements contentually by introducing the ultimate assumptions of thetheory as premises and in addition explicitly state the rules of inference (in the sense of the formal standpoint).

To clarify the situation we can make-use of Weyl's comparison of a proof conducted in a purely formal way with a game of chess; the fundamental assumptions correspond to the initial position in the game, the rules of inference to the rules of the game. Let us assume that a bright chess master has for a certain initial position A discovered the possibility of checkmating his opponent in 10 moves. From the usual point of view we must then say that this possibility is *logically* determined by the initial position and the rules of the game. On the other hand, one can not maintain that the assertion of the possibility of a checkmate in 10 moves is implied by the specification of the initial position A and the rules of the game. The appearance of a contradiction between these claims disappears$_{c_1}$,$_{c_1}$ if we see clearly that the "logical" effect of the rules of the game depends upon *combination* and therefore does not come about just through analysis of meaning but only through genuine presentation.

Every mathematical proof is in this sense a presentation. We will show here by a simple special case how the combinatorial element comes into play in a proof.

We have the rule of inference: "if A and if A implies B, then B." In a formal translation of a proof this inference principle corresponds to the rule that the formula B can be obtained from the two formulas A and $A \to B$. Now let us apply this rule in a formal derivation, and we furthermore assume that A and $A \to B$ do not belong to the initial assumptions. Then we have a sequence of inferences S leading to A and a sequence T leading to $A \to B$ and according to the rule described the formulas A and $A \to B$ yield the formula B.

If we want to analyze what is going on here, we must not prejudge the decisive point by the mode of presentation. The endformula of the sequence of inferences T is initially only given as such, and it is epistemologically a new step to recognize that this formula coincides with the one which arises by connecting with a "$\to$" the formula A obtained in some other way and the formula B to be derived.

The determination of an identity is by no means always an identical or tautological determination. The coincidence to be noted in the present case can not be read off directly from the content of the formal rules of inference and the structure of the initial formulas; rather, it can be read off only from the structure that is obtained by application of the rules of inference, that is to say by the carrying out of the inferences. Thus, a combinatorial element is here present in fact.[2]

If in this way we become clear about the role of the mathematical in logic, then it will not seem astonishing that arithmetic can be subsumed within the system of theoretical logic. But also from the standpoint we have now reached this subsumption loses its epistemological significance. For we know in advance that the formal element is not eliminated by the inclusion of arithmetic in the logical system. But with respect to the formal we have found that the mathematical considerations represent a standpoint of higher abstraction than the conceptual logical ones. We therefore achieve no greater generality at all for mathematical knowledge as a result of its subsumption

[2]Paul Hertz defended the claim that logical inference contains "synthetic elements" in his essay "On thinking" (*vide* [**?**]). His grounds for this claim will be explained in an essay on the nature of logic, to appear shortly; they include the point developed here but rest in addition on still other considerations.

under logic; rather we achieve just the opposite; a *specialization by logical interpretation*, a kind of *logical clothing*.

A typical example of such logical clothing is the method by which Frege and, following him but with a certain modification, Russell defined the natural numbers.

Let us briefly recall the idea underlying Frege's theory. Frege introduces the numbers as cardinal numbers. His premises are as follows:

A cardinal number applies to a *predicate*. The concept of cardinal number arises from the concept of equinumerosity. Two predicates are called equinumerous if the things of which the one predicate holds can be correlated one-one with the things of which the other predicate holds.

If the predicates are divided into classes by reference to equinumerosity in such a way that all the predicates of a class are equinumerous with one another and predicates of different classes are not equinumerous, then every class represents the *cardinal number* which applies to the predicates belonging to it.

In the sense of this general definition of cardinal number, the particular finite numbers like 0, 1, 2, 3 are defined as follows:

0 is the class of predicates which hold of no thing. 1 is the class of "one-numbered" predicates; and a predicate P is called one-numbered if there is a thing x of which P holds and no other thing different from x of which P holds. Similarly, a predicate P is called two-numbered if there is a thing x and a thing y different from it such that P holds of x and y and if there is no thing different from x and y of which P holds. 2 is the class of two-numbered predicates. The numbers 3, 4, 5 etc. are to be explained as classes in an analogous way.

After he has introduced the concept of a number immediately following a number, Frege defines the general concept of finite number in the following way: a number n is called finite if every predicate holds of n, which holds of 0 and which, if it holds of a number a holds of the immediately following number.

The concept of a number belonging to the series of numbers from 0 to n is explained in a similar way. The formulation of these concepts is followed by the derivation of the principles of number theory from the concept of finite number.

We now want to consider in particular Frege's definition of the individual finite numbers. Let us take the definition of the number 2, which is explained as the class of two-numbered predicates. It may be objected to this explanation that the belonging of a predicate to the class of two-numbered predicates depends upon extralogical conditions and the class therefore constitutes no logical object whatsoever.

This objection is, however, eliminated if we adopt the standpoint of Russell's theory with respect to the understanding of classes (sets or extensions of concepts). According to it classes (extensions of concepts) are not actual objects at all; rather they function only as dependent terms within a reformulated sentence. If, for example, K is the class of things with the property E, i. e. the extension of the concept E, then, according to Russell, the assertion that an thing a belongs to the class K is to be viewed only as a reformulation of the assertion that the thing a has the property E.

If we combine this conception with Frege's definition of cardinal number, we arrive at the idea that the number 2 is to be defined not in terms of the class of two-numbered predicates but in terms of the concept the extension of which constitutes this class. The number 2 is then identified with the *property of two-numberedness* for predicates, i. e. with the property of a predicate of holding of an thing x and of an thing y different[3] from x but of no thing different from x and y.

For the evaluation of this definition it is essential to know how the process of defining is understood here and what claims are involved in it. What will be shown here is that this definition is not a correct reproduction of the true meaning of the cardinal number concept "two" by means of which this concept is revealed in its logical purity freed from all inessential features. Rather it will be shown that it is exactly the specifically logical element in the definition that is an inessential addition.

The two-numberedness of a predicate P means nothing else but that there are two things of which the predicate P holds. Here three distinct conceptual features are present: the concept "two things," the existential feature, and the fact that the predicate P holds. The content of the concept "two things" here does not depend on the meaning of either of the other two concepts. "Two things" means something already without the assertion of the existence of two things and also without reference to a predicate which holds of two things; it means simply: "one thing and one more thing."

In this simple definition the concept of cardinal number shows itself to be an elementary *structural concept*. The appearance that this concept is

[3]For the sake of simplicity we shall skip the considerations regarding the concept of difference, resp. its contradictory concept of identity.

reached from the elements of logic results, in the case of the logical definition of cardinal number under consideration, only from the fact that the concept is conjoined with logical elements, namely the existential form and the subject-predicate relation, which are in themselves inessential for the concept of cardinal number. We thus indeed have before us here a formal concept in *logical clothing*.

The result of these considerations is that the claim of the logicists that mathematics is a purely logical field of knowledge shows itself to be imprecise and misleading when theoretical logic is examined more closely. That claim is sound only if the concept of the mathematical is taken in the sense of its historical demarcation and the concept of the logical is systematically broadened. But such a determination of concepts hides what is epistemologically essential and ignores the special nature of mathematics.

§ 3 Formal abstraction

We have determined that formal abstraction, i. e. the focusing on the structural side of objects, is the characteristic feature of mathematical reasoning and have thus demarcated the field of the mathematical in a fundamental way. If we want likewise to gain an epistemological understanding of the concept of the logical, then we are led to separate from the entire domain of the theory of concepts, judgments, and inferences, which is commonly called logic, a narrower subdomain, that of *reflective or philosophical logic*. This is the domain of knowledge which is *analytic in the genuine sense* and which stems from a pure *awareness of meaning*. This philosophical logic is the starting point of systematic logic, which takes its initial elements and its principles from the results of philosophical logic and, using mathematical methods, develops from them a theory.

In this way the extent of genuinely analytic knowledge is separated clearly from that of mathematical knowledge, and what is justified in *Kant's theory of pure intuition* on the one hand, and in the claim of the logicists on the other, comes into play. We can distinguish Kant's fundamental idea that mathematical knowledge and also the *successful application of logical inference rest on an intuitive evidence* from the particular form that Kant gave to this idea in his theory of space and time. By doing so we also arrive at the possibility of doing justice to both the very elementary character of mathematical evidence and to the high degree of abstraction of the mathematical point of view, emphasized in the claim about the logical character of mathematics.

Our conception also gives a simple account of the role of number in mathematics: we have explained mathematics as the knowledge which rests upon the formal (structural) consideration of objects. However, the numbers constitute as cardinal numbers *the simplest formal determinates* and as ordinal numbers the *simplest formal objects*.

Cardinality concepts present a special difficulty for philosophical explication because of their special categorial position, which also makes itself felt in language in the need for a unique species of number words. We do not have to bother here with more detailed explication, but we do have to observe that the determination of cardinal number involves the putting together of a given or imagined complex out of components, which is just what constitutes the structural side of an object. And indeed it is the most elementary structural characteristics that are conveyed by cardinal numbers. Thus cardinal numbers play a role in all domains to which formal considerations are applicable; in particular we encounter cardinal number within theoretical logic in a wide variety of ways: for example, as cardinal number of the subjects of a predicate (or as one says, as cardinal number of the arguments of a logical function); as cardinal number of the variable predicates involved in a logical sentence; as cardinal number of the applications of a logical operation involved in the construciton of a concept or sentence; as cardinal number of the sentences involved in a mode of inference; as the type-number of a logical expression, i. e. the highest number of successive subject-predicate relations involved in the expression (in the sense of the ascent from the objects of a theory to the predicates, from the predicates to the predicates of the predicates, from these latter to their predicates, and so on).

Cardinal numbers, however, provide us only with formal *determinations* and not yet with formal objects. For example, in the conception of the cardinality three there is still no unification of three things into one object. The bringing together of several things into one object requires some kind of ordering. The simplest kind of order is that of mere succession, which leads to the concept of *ordinal number*. An ordinal number in itself is also not determined as an object; it is merely a place marker. We can, however, standardize it as an object, by choosing as *place markers the simplest structures deriving from the form of succession*. Corresponding to the two possibilities of beginning the sequence of numbers with 1 or with 0, two kinds of standardization can be considered. The first is based on a sort of things and a form of adjoining a thing; the objects are figures which begin and end with a thing of the sort under consideration, and each thing, which is not yet the end of the figure, is followed by an adjoined thing of that sort. In the second kind of standardization we have an initial thing and a process; the objects are then the initial thing itself and in addition the figures that are obtained by beginning with the initial thing and applying the process one or more times.

If we want to have the ordinal numbers, according to either standardization, as unique objects free from all inessential features, then we must take as object in each case the bare schema of the respective figures obtained by repetition; this requires a very high degree of abstraction. However, we are free to represent these purely formal objects by concrete objects ("number signs" or "numerals"); these then possess inessential arbitrarily added characteristics, which, however, can be immediately recognized as such. This procedure is based on a certain agreement, which must be kept throughout one and the same deliberation.[4] Such an agreement, according to the first standardization, is the representation of the first ordinal numbers by the figures $1, 11, 111, 1111$. According to an agreement corresponding to the second standardization, the first ordinal numbers are represented by the figures 0, $0'$, $0''$, $0'''$, $0''''$.

Having found a simple access to the numbers in this way by regarding them structurally, our conception of the character of mathematical knowledge receives a new confirmation. For, the dominant role of number in mathematics becomes clear on the basis of this conception; and our characterization of mathematics as a theory of structures seems to be an appropriate extension of the view mentioned at the beginning of this essay that numbers constitute the real object of mathematics.

The satisfactory features of the standpoint we have reached must not mislead us into thinking that we have already obtained all the fundamental insights required for the problem of the grounding of mathematics. In fact, until now we have only dealt with the preliminary question that we wanted to clarify first, namely, what is the specific character of mathematical knowledge? Now, however, we must turn to the problem that raises the main

[4]Philosophers are inclined to treat this relation of representation as a connection of meaning. One must notice, however, that there is an essential difference here from the usual relation of word and meaning; namely the representing thing contains in its constitution the essential properties of the object represented, so that the relationships to be investigated among the represented objects can also be found among the representatives and can be determined by consideration of the latter.

difficulties in grounding mathematics, the problem of the infinite.

Part II: The problem of the infinite and the formation of mathematical concepts

§ 1 The postulates of the theory of the infinite. – The impossibility of a basis in intuition. – The finitist standpoint

The mathematical theory of the infinite is analysis (infinitesimal calculus) and its extension by general set theory. We can restrict ourselves here to consideration of the infinitesimal calculus because the step from it to general set theory requires only additional assumptions, but no fundamental change of philosophical conception.

The foundation given to the infinitesimal calculus by Cantor, Dedekind, and Weierstraß shows that a rigorous development of this theory succeeds if two things are added to the elementary inferences of mathematics:

1. The application of the method of existential inference to the integers, i. e., the assumption of the *system* of integers in the manner of a domain of objects of an axiomatic theory, as is explicitly done in *Peano's axioms for number theory*.

2. The conception of the totality of all sets of integers as a combinatorially surveyable manifold. A set of integers is determined by a distribution of the values 0 and 1 to the positions in the number series. The number n belongs to the set or not depending on whether the nth position in the distribution is 1 or 0. Just as the totality of possible distributions of the values 0, 1 over a finite number of positions, e. g. over five positions, is completely surveyable, by analogy the same is assumed also for the entire number series.

From this analogy follows in particular also the validity of *Zermelo's principle of choice* for collections of sets of numbers. However, for the time being we will put aside the discussion of this principle, it will fit in naturally at a later point.

If we now consider these requirements from the standpoint of our general characterization of mathematical knowledge, it seems at first that there is no fundamental difficulty in justifying them on that basis. For both in the case of the number series and in that of the sets derived from it, one deals with *structures*, which differ from those treated in elementary mathematics only in being structures of infinite manifolds . The existential inference applied to numbers also seems to be justified by their objective character as formal objects the existence of which can not depend on accidental facts about people's conceptions of numbers.

Against this argumentation it is to be remarked, however, that it is premature to conclude from the character of formal objects, i. e. from their being free of accidental empirical features, that formal entities must be related to a domain of existing formal things. As an argument against this conception we could put forward the set-theoretic paradoxes; but it is simpler to point out directly that primitive mathematical evidence does not assume such a domain of existing formal entities and that, in contrast, the connection with that to what is actually imagined is essential as a starting point for formal abstraction. In this sense the Kantian assertion that pure intuition is the form of empirical intuition is valid.

Correspondingly, existence assertions in disciplines that rest on elementary mathematical evidence do not have a proper meaning. In particular, in elementary number theory we only deal with existence assertions that refer to an explicit totality of numbers that can be exhibited, or to an explicit process that can be executed intuitively, or to both together, i. e. to a totality of numbers that can be obtained by such a process.

Examples of such existence claims are: "There is a prime number between 5 and 10," namely 7 is a prime number.

"For every number there is a greater one," namely if n is a number, then construct $n + 1$. This number is greater than n.

"For every prime number there is a greater one," namely if a prime number p is given, then construct the product of this number and all smaller prime numbers and add 1. If k is the number obtained in this way, then there must be a prime number among the numbers between $p + 1$ and k.

In each of these cases the existence assertion is made more precise by a further specification; the existence claim is restricted to explicit processes that can be carried out in intuition and makes no reference to a totality of all numbers. Following Hilbert, we will call this elementary point of view, restricted by the requirements imposed by intuitability in principle, the *finitist* standpoint; and in the same sense we will speak of finitist methods, finitist considerations, and finitist inferences.

It is now easy to see that existential reasoning goes beyond the finitist standpoint. This transcending of the finitist standpoint already takes place with any existence proposition which is put forward without a more exact determination of the existence claim, as for example with the thoerem that there is at least one prime number in every infinite arithmetic sequence

$$a \cdot n + b \qquad (n = 0, 1, 2, 3, \ldots)$$

if a and b are relatively prime numbers.

An especially common and important case of transcending the finitist standpoint is the inference from the failure of an assertion to hold universally (for all numbers) to the existence of acounterexample or, in other words, the principle according to which the following alternative holds for every number predicate $P(n)$: either the universal assertion that $P(n)$ holds of all numbers is valid, or there is a number n of which $P(n)$ does not hold. From the standpoint of existential reasoning this principle results as a direct application of the law of the excluded middle, i. e. from the meaning of negation. This logical consequence fails to hold for the finitist standpoint, because the assertion that $P(n)$ holds for all numbers has here the purely hypothetical sense that the predicate holds for any given number, and thus the negation of this claim does not have the positive meaning of an existence assertion.

But, this does not yet close the discussion of the possibilities of a discerning mathematical foundation for the assumptions of analysis. It has to be admitted that the assumption of a totality of formal objects does not correspond to the standpoint of primitive mathematical evidence, but the demands of the infinitesimal calculus can be motivated by the observation that the totalities of numbers and number sets one deals with are *structures of infinite sets*. In particular, the application of existential reasoning on number would thus not be inferred from the idea of the concept of numbers in the realm of formal objects, but rather from considering the structure of the number sequence in which the individual numbers occur as elements. Indeed we have not yet considered the argument already mentioned that mathematical knowledge can also concern structures of infinite multiplicities .

Herewith we come to the question of the *actual infinite*. For the infinite insofar as infinite manifolds are concerned, is the true actual infinite in contrast to the "potential infinite;" by the latter is meant not an infinite object but merely the unboundedness of the progression from something finite to something that is again finite. For example, this unboundedness also holds from the finitist standpoint for numbers, since for every number a greater one can be constructed.

The question about the actual infinite which we have to ask first is whether it is given to us as an object of intuitive mathematical knowledge.

In harmony with what we have determined so far, one could be of the opinion that we really are capable of an intuitive knowledge of the actual infinite. For even if it is certain that we have a concrete conception only of finite objects, nevertheless an effect of formal abstraction could be exactly the following: that it frees itself from the restriction to the finite and passes to the limit, as it were, in the case of certain indefinitely continuable processes. In particular one may be tempted to invoke geometric intuition and to point to examples of intuitively given infinite manifolds from the domain of geometric objects.

Now in the first place geometric examples are not conclusive. One is easily deceived here by interpreting the spatially intuitive in thesense of an existential conception. For example, a line segment is not intuitively given as an ordered manifold of points but as a uniform whole, although, to be sure, an extended whole within which *positions* are distinguishable. The idea of one position on the line segment is intuitive, but the totality of *all positions* on the line segment is merely a concept of thought. By means of intuition we here reach only the potential infinite since every position on the line segment corresponds to a division into two shorter segments each of which is in turn divisible into shorter segment yet.

Furthermore, one cannot point to infinitely extended things like infinite lines, infinite planes, or infinite space as objects of intuition. In particular, space as a whole is not given to us in intuition. We do indeed represent every spatial figure as situated in space. But this relationship of individual spatial figures to the whole of space is given as an object of intuition only to the extent that a spatial neighborhood is represented along with every spatial object. Beyond this representation, the position in the whole of space is conceivable *only in thought.* (In opposition to Kant, we must maintain this view.)

The main argument that Kant gave in favor of the intuitive character of our representation of space as a whole, in fact proves only that one cannot attain the concept of a single inclusive space through mere generalizing abstraction. But that is not what is claimed by the assertion that our representation of the whole of space is only accessible in thought, i. e. that we are here dealing with a mere general concept.

Rather, we have in mind a more complicated situation: the representation of the whole of space involves two different kinds of thoughts both of which go beyond the standpoint of intuition and of reflective logic. One rests upon the thought that connecting things yields the world as a whole and therefore stems from our belief about what is real. The other is a *mathematical idea* which, to be sure, begins with intuition but does not remain in the domain of the intuitively representable; it is the representation of space as a manifold of points subject to the laws of geometry.[5]

In both of these ways of representing space as a whole this totality is not recognized as given, but rather is posited only tentatively. The representation of the whole of physical space is a fundamental problem; afterall, it is exactly from the standpoint of contemporary physics that there is the possibility of giving this initially very vague thought a more restricted and precise formulation, whereby it becomes significant systematically and accessible to research. The geometric ideas of spatial manifolds are indeed precise from the very beginning, but require a proof of their consistency.

Thus we have no grounds for the assumption that we have an intuitive representation of space as a whole. We cannot point directly to such a representation, nor is there any necessity to introduce that assumption as an explanation. But if we deny the intuitiveness of space as a whole, then we also cannot claim that infinitely extended spatial configurations are intuitively representable.

It should also be noted that the original intuitive conception of elementary Euclidean geometry does not in the least require a representation of infinite figures. After all, we are dealing here only with finitely extended figures. Infinite manifolds of points are also never involved, since there are no underlying general existential assumptions; every existential claim rather asserts a possible geometric construction. For example, that every line segment has a midpoint says from this standpoint only that for every line segment a midpoint can be constructed.[6]

[5]Both of these representations of space are united in the view of nature found in Newtonian physics and are not clearly distinguished from one another. In Newtonian physics Euclidean geometry constitutes the law governing the spatial relation of things in the universe. Only the subsequent development of geometry and physics showed the necessity of distinguishing between space as a physical entity and space as an ideal manifold determined by geometric laws.

[6]In Euclid's axiomatization this standpoint is of course not completely adhered to, since one finds here the notion of an *arbitrarily great extension* of a line segment. This notion

Thus the apparent possibility of displaying an actual infinity in the domain of objects of geometrical intuition is misleading. We can, however, also show in a more general way that there is no question of eliminating the condition of finitude via formal abstraction as would be required for an intuition of the actual infinite. Indeed, the requirement of finitude is no accidental empirical limitation but an essential characteristic of a formal object.

The empirical limitation still lies within the domain of the finite, where formal abstraction must help us to go beyond the boundaries of our actual power of representation. A clear example of this is the unlimited divisibility of a line segment. Our actual power of representation already fails when the division exceeds a certain degree of fineness. This boundary is physically accidental and it can be overcome with the help of optical equipment. But after a certain smallness all optical equipment becomes useless, and finally our spatial and metrical representations lose all physical meaning. Thus, in representing unlimited divisibility we already abstract from the requirements of actual representation as well as from the requirements of physical reality.

The situation is analogous in the case of the representation of unlimited addition in number theory. Here, too, there are limits to the execution of repetitions both with respect to actual representability and to physical realization. Let us consider as an example the number $10^{(10^{1000})}$. We can arrive at it in a finitist way as follows: we start from the number 10, which, according to the standardization given earlier, we represent by the figure,

$$1111111111\,.$$

Let z be an arbitrary number, represented by an analogous figure. If in the representation of 10 we replace each 1 with the figure z, there results, as we can see intuitively, another number-figure, which for purposes of communication is called "$10 \times z$."[7] In this way we get the process of multiplying a number by 10. From this we obtain the process of transforming a number a into 10^a by letting the first 1 in a correspond to the number 10 and every subsequent 1 to the process of multiplication by 10 until the end of the figure a is reached. The number obtained by the last process of multiplying by 10 is called 10^a.

From an intuitive viewpoint this procedure offers no difficulty whatsoever. But, if we want to consider the process in detail our representation already

can in fact be avoided; one needs only formulate the axiom of parallels differently.

[7]Here we use a symbol "with meaning."

fails in the case of rather small numbers. We can again get some further help from instruments or by making use of external objects, which involve the determination of very large numbers. But even with all of these we soon reach a limit: it is easy for us to represent the number 20; 10^{20} far extends our actual power of representation, but is definitely within the domain of physical realizability; it is ultimately very questionable, however,whether the number $10^{(10^{20})}$ occurs in any way in physical reality either as a relation between magnitudes or as a cardinal number.

But intuitive abstraction is not constrained by such limits on the possibility of realization. For limits are accidental from the formal standpoint. Formal abstraction finds no earlier place, so to speak, to make a principled distinction than at the difference between finite and infinite.

This difference is indeed a fundamental one. If we consider more precisely how an infinite manifold as such can be characterized at all, then we find that such a characterization is not possible by means of any intuitive presentation; rather it is possible only by means of the assertion (or assumption or determination) of a lawlike connection. Thus, infinite manifolds are accessible to us only *in thought*. Such thinking is indeed also a kind of representation, by which a manifold is, however, not represented as an object; rather conditions are represented which a manifold satisfies (or has to satisfy).

The fact that formal abstraction is essentially tied to the element of finitude becomes especially apparent through the fact that the property of finitude is not a special limiting characteristic from the standpoint of intuitive evidence when considering totalities and figures. From this standpoint the limitation to the finite is observed immediately and, so to speak, *tacitly*. We do not need a special definition of finitude in this case, because the finitude of objects is taken for granted for formal abstraction. So, for example, the intuitive structural introduction of the numbers is suitable only for the *finite* numbers. From the intuitive formal standpoint, "repetition" is *eo ipso* finite repetition.

This representation of the finite, which is implicit in the formal point of view, contains the epistemological justification for the principle of complete induction and for the admissibility of recursive definition, both procedures here construed in their elementary form, as "finitist induction" and "finitist recursion."

Drawing on this representation of the finite of course goes beyond the intuitive evidence that is necessarily involved in logical reasoning. It corresponds rather to the standpoint from which one *reflects* already on the general characteristics of intuitive objects. Furthermore, the use of the intuitive representation of the finite can be avoided in number theory if one does not insist on treating this theory in an elementary way. But the intuitive representation of the finite forces itself upon us as soon as a formalism itself is made the object of examination, thus in particular in the systematic theory of logical inferences. This brings to the fore the fact that finiteness is an essential feature of the figures of any formalism whatsoever. The limits of any formalism, however, are none other than those of representability of intuitive complexes in general.

Thus our answer to the question whether the actual infinite is intuitively knowable turns out to be negative. A further consequence is that the method of finitist examination is the appropriate one for the standpoint of intuitive mathematical knowledge.

In this way, however, we can not verify the already mentioned assumption for the infinitesimal calculus.

§ 2 Intuitionism. – Arithmetic as a theoretical framework

How should we proceed now in the light of these facts? Concerning this question the opinions are divided. We find here a conflict of views similar to that over the question of characterizing mathematical knowledge. The proponents of the standpoint of primitive intuitiveness conclude immediately from the fact that the postulates of analysis and set theory transcend the finitist standpoint that these mathematical theories must be abandoned in their present form and revised from the ground up. The proponents of the standpoint of theoretical logic, on the other hand, either try to logically justify the postulates of the theory of the infinite, or they deny that these postulates are problematic at all by disputing the fundamental significance of the difference between finite and infinite.

The former view was already held by Kronecker when the method of existential inference first emerged; he was probably the first person to pay close attention to the methodological standpoint that we call finitist and to emphasize most strongly its importance. His attempts to satisfy this methodological requirement in analysis remained fragmentary, however; a more precise philosophical presentation of this standpoint was also lacking. Thus in particular Kronecker's oft quoted dictum, that God has created the whole numbers, but everything else is the work of man, is not at all suited for motivating Kronecker's requirement:[8] if the whole numbers are created by God, one would think that it is permissible to apply existential inference to them, whereas it is just the existential point of view that Kronecker excludes already in the case of the whole numbers.

Brouwer has extended Kronecker's standpoint in two directions: on the one hand with respect to philosophical motivation by putting forward his theory of "intuitionism,"[9] and on the other hand by showing how one can apply the finitist standpoint in analysis and set theory, and finitistically ground at least a considerable portion of these theories by fundamentally revising the formation of concepts and the methods of inference.

The result of this investigation does have its negative side, however; for it turns out that, in the process of treating analysis and set theory finitistically, one must accept with not only great complications, but also serious losses with respect to systematization.

The complications appear already in connection with the first concepts of the infinitesimal calculus such as boundedness, convergence of a number sequence, the difference between rational and irrational. Let us take for example the concept of boundedness of a sequence of integers. According to the usual view one of the following alternatives holds: either the sequence exceeds every bound, and then the sequence is unbounded, or all numbers in the sequence are below some given bound, and then the sequence is bounded. In order to determine here a finitist concept we must sharpen the definition of boundedness and unboundedness as follows: a sequence is called bounded if we can indicate a bound for the numbers in the sequence, either directly or by giving a procedure for producing one; the sequence is called unbounded if there is a law according to which every bound is necessarily exceeded by the sequence, i. e., the assumption that the sequence has a bound leads to an absurdity.

With this formulation of the concepts the definitions do indeed have a finitist character, but we no longer have a complete disjunction between the cases of boundedness and unboundedness. We therefore cannot infer that a sequence is bounded from a refutation of the assumption that the sequence

[8] The methodical standpoint appropriate to this dictum is the one adopted by Weyl in his book *The Continuum* (*vide* [**?**]).

[9] In the interest of clarifying the discussion it seems to me advisable to use the term "intuitionism" to refer to a philosophical view in contrast to the term "finitist," which refers to a particular method of inference and concept formation.

is unbounded. Likewise we cannot consider a claim as established when it is proved, on the one hand, under the assumption that a certain sequence of numbers is bounded and, on the other hand, under the assumption that it is unbounded.

In addition to such complications, which permeate the entire theory, there is a yet more essential disadvantage, namely that many of the general theorems, through which mathematics obtains its systematic clarity, fail. So, for example, in Brouwer's analysis even the theorem that every continuous function has a maximum value on a finite closed interval is not valid.

It seems an unjustified and unreasonable demand that philosophy is putting on mathematics, to give up its simpler and more fruitful method in favor of a cumbersome method, which is also inferior from a systematic point of view, without being forced to do so by an inner necessity. This constraint makes us suspicious of the standpoint of intuitionism.

Let us see what are the main points of this philosophical view that was developed by Brouwer. It includes, first of all, a characterization of mathematical evidence. Our earlier discussion of formal abstraction agrees in essential points with this characterization, in particular with regard to their connection with Kant's theory of pure intuition.

Admittedly there is a divergence insofar as according to Brouwer's view the temporal aspect is an essential feature of the objects of mathematics. But it is not necessary to go into a discussion of this point here, since a decision concerning it is of no consequence for the question of mathematical methodology: what for Brouwer arises as a consequence of the connection between time and the objects of mathematics is nothing other than what is obtained by us from the connection of formal abstraction with its concrete, intuitive starting point, namely the methodological restriction to finitist procedures.

The decisive consequences of intuitionism result first from the further assertion that all mathematical thought with a claim to scientific validity must be carried out on the basis of mathematical evidence, so that the limits of mathematical evidence are at the same time limits for mathematical thought in general.

This demand that mathematical thought be limited to the intuitively evident appears at first to be completely justified. Indeed it corresponds to our familiar conception of mathematical certainty. We must, however, keep in mind that this familiar conception of mathematics originally went together with a philosophical view, according to which the intuitive evidence of the foundations of the infinitesimal calculus was not in question. However, we have departed from such a view since we found that the postulates of analysis cannot be verified by intuition, that in partiular the representation of infinite totalities, which is fundamental in analysis, cannot be grasped in intuition but only through the *formation of ideas*.

Now we can not expect this new view of the limits of intuitive evidence to fit directly with the received conception of the epistemological character of mathematics. Rather, on the basis of what we have determined it seems likely that the generally accepted conception of mathematics represents the situation too simply and that we can not do justice to what goes on in mathematics from the standpoint of evidence alone; we must *acknowledge that thinking has its own distinctive role*.

Thus we arrive at a distinction between the standpoint of elementary mathematics and a systematic standpoint that goes beyond it. This distinction is by no means artificial or merely *ad hoc*; rather it corresponds to the two different starting points from which one is led to arithmetic: on the one hand, the combinatorial consideration of relations between discrete entities, and on the other, the theoretical demand placed on mathematics by geometry and physics.[10] The system of arithmetic by no means arises only from an activity of construction and intuitive consideration, but also, in large part, from the task of precisely conceptualizing and theoretically mastering the geometric and physical representations of quantity, area, impact, velocity, and so on. The method of arithmetization is a means to this end. But in order to serve this purpose, arithmetic must *extend* its methodological standpoint from the original elementary standpoint of number theory *to a systematic perspective* in the sense of the postulates discussed.

Arithmetic, which comprises the encompassing framework within which the geometric and physical disciplines find their place, consists not only of the elementary, intuitive treatment of numbers, but it has itself the character of a *theory* in that it builds on the representation of the totality of numbers as

[10]It is remarkable that Jakob Friedrich Fries, who still ascribed mathematical evidence to a domain going far beyond the finite (in particular, according to his view "the continuous sequence of larger and smaller" is given in pure intuition), nevertheless made a methodical distinction between, on the one hand, "arithmetic as a theory," which conceptualizes and scientifically develops the intuitive representation of magnitude, and, on the other, "combinatory theory or syntactic," which rests only on the postulate of arbitrary ordering of given elements and its arbitrary repeated applications, and which needs no axioms since its operations are "immediately comprehensible in themselves." (Cf. J. F. Fries, *Mathematical Philosophy of Nature* (*vide* [**?**] p. ■).)

a system of things as well as the totality of sets of numbers. This systematic arithmetic achieves its aim in the best possible way, and there are no grounds in its procedures for objections, so long as it is clear that we are here not taking the standpoint of elementary intuitiveness, but that of a thought construction, i. e., the standpoint that Hilbert calls the *axiomatic* one.

The charge of arbitrariness against this axiomatic approach is also unjustified, for in the foundations of systematic arithmetic we are not dealing with an arbitrary axiom system, put together according to need, but with a *natural systematic extrapolation from elementary number theory.* However, the analysis and set theory which develop on this foundation constitute a theory which is already *distinguished in pure intellect*, and which is suited to be taken as the theory $\kappa\alpha\tau$'■ '$\varepsilon\xi o\chi\acute{\eta}\nu$, into which we incorporate the doctrines and theoretical approaches of geometry and physics.

Thus we cannot acknowledge the veto that intuitionism directs against the method of analysis. The observation, on which we agree with intuitionism, that the infinite is not given to us intuitively does indeed require us to modify our philosophical conception of mathematics, but not to transform mathematics itself.

Of course, the problem of the infinite returns again. For in taking a thought construction as the starting point for arithmetic, we have introduced something problematic. A thought construction, may it be ever so plausible and natural from a systematic point of view, contains in and of itself no guarantee that it can be carried out consistently. In apprehending the idea of the infinite totality of numbers and of sets of numbers, the possibility is not excluded that this idea could lead to a contradiction in its consequences. Thus it remains it to investigate the question of the freedom from contradiction, or "consistency,"[11] of the system of arithmetic.

Intuitionism wants to spare us these tasks by restricting mathematics to the domain of finitist considerations; but the price for this elimination of the difficulties is too high: the problem goes away, but the systematic simplicity and clarity of analysis is also lost.

§ 3 The problems with logicism. – The value of the logicistic reduction of arithmetic

[11] We suggest here using this expression, which Cantor used specifically with respect to the construction of sets, more generally with respect to any theoretical approach.

The proponents of the standpoint of logicism believe that they can deal with this problem in a completely different way. In discussing this standpoint we connect with our earlier consideration of logicism. There it was important to recognize that intuitive evidence even plays a role in deductive logic, and that the logical definition of cardinal number does not establish the specifically logical nature of the concept of cardinal number (as a concept of pure reflection) but rather is only a logical normalization of elementary structural concepts.

These reflections concern the demarcation of what is logical in the narrow sense from what is formal. The recognition of the formal element in logic, however, by no means resolves the methodological question of logicism. Logicism is concerned not only with the theoretical development of the science of inference; but, as already explained, it takes as its further task the reduction of all arithmetic to the formalism of logic. This reduction proceeds first via the introduction of cardinal numbers as properties of predicates, as already described, and then (as will not be described more precisely here) by expressing the construction of sets of numbers in terms of the logical formalism, replacing each set with a defining predicate. Thus the totality of predicates of numbers replaces the totality of sets of numbers.

In this way one in fact succeeds in assigning to every arithmetical sentence a sentence from the domain of theoretical logic in which, aside from variables, only "logical constants" occur, i. e. basic logical operations like conjunction, negation, the form of generality, etc.

Now it is clear that the problem of the infinite can not be solved solely by this translation of arithmetic into the formalism of logic. If theoretical logic deductively obtains the system of arithmetic, then its procedures must include either explicit or hidden assumptions through which the actual infinite is introduced.

The justification that is given for these assumptions, and the position adopted with respect to them, has been the weak point of logicism from the start. Indeed, Frege and Dedekind, whose proofs and discussions displayed extreme precision and rigor everywhere else, were relatively unconcerned about the supposed self-evident assumptions they took as the basis for the standpoint of general logic, namely the idea of a closed totality of all conceivable logical objects whatsoever.

If this idea were tenable, it would of course be more satisfactory from a systematic point of view than the more specialized postulates of arithmetic. But, as is well known, it had to be dropped, because of the contradictions to which it lead. Since then logicism has forgone proving the existence of an infinite totality, and has instead explicitly postulated an *axiom of infinity*.

This axiom of infinity, however, is not a sufficient assumption for obtaining arithmetic as logically construed. We could only obtain with it what follows from our first postulate, the admissibility of existential inference with respect to the integers. To conform with our second postulate we still require something further, namely, the application of existential inference *with respect to predicates*. The justification of this way of proceeding might at first seem to be logically self-evident, and in fact it is not questioned under the conception of Frege and Dedekind. But once the idea of the totality of all logical objects is given up, the idea of the totality of all predicates becomes problematic as well, and here closer inspection reveals a particular, fundamental difficulty.

Namely, in accordance with the genuine logicist standpoint, we construe the totality of predicates as a totality which essentially first comes into existence in the frame of the system of logic by applying logical constructions to certain initial, prelogical predicates, e. g., predicates taken from intuition. Further predicates are now obtained by reference to the totality of predicates. An example is the already mentioned Fregean definition of finite number: "a number n is called finite if every predicate holds of n that holds of the number 0 and that, if it holds of a number a also holds of the succeeding number." The predicate of finiteness is defined here by reference to the totality of all predicates.

Definitions of this kind, called "impredicative,"[12] occur everywhere in the foundation of arithmetic , and indeed, right in the crucial places.

Now there is really no objection to determining a thing from a totality by means of a property that refers to this totality. So, for example, in the totality of numbers a particular number is defined by the property of being the greatest prime number, such that its product with 1000 is greater than

[12]The term is due to Poincaré who—in contrast to the other critics of set theory, almost all of whom concerned themselves just with the axiom of choice—brought the issue of impredicative definition into the discussion and put the emphasis on it. His criticism was disputable, however, because he made the use of impredicative definitions appear to be a novelty introduced by set theory. Zermelo could reply to him that impredicative definitions already occur essentially in the usual modes of inference in analysis, which Poincaré in fact accepted.

Since then Russell and Weyl in particular have thoroughly discussed and completely clarified the role of impredicative definition in analysis.

the product of the preceding prime number with 1001.[13]

But it is required here that the totality in question is determined *independently* of the definitions referring to it; otherwise we enter a vicious circle.

However, precisely in the case of the totality of predicates and the impredicative definitions referring to it, this precondition cannot be taken as directly satisfied. For the totality of predicates is determined according to the conception discussed here by the laws for logical constructions, and these include also impredicative definitions.

In order to avoid the vicious circle it would of course suffice to show that every predicate introduced by an impredicative definition can also be defined in a "predicative" way. Indeed, one could even get by with a weaker claim. Since in the logical foundation of arithmetic a predicate is always considered just with respect to its extension, i. e., with respect to the set of things of which it holds, we would only need to know that every predicate introduced by an impredicative definition *is extensionally equal* to a predicatively defined predicate.

This postulate, called the "axiom of reducibility," was imposed along with the axiom of infinity by Russell, who recognized with total clarity the difficulty involved in impredicative definitions.

But how is this axiom of reducibility to be understood? From its formulation it is not clear whether it expresses a logical law or an extralogical assumption.

If, in the first case, in which the axiom of reducibility would be the expression of a logical law, its validity would have to be independent of the basic domain of prelogical initial predicates—at least assuming that this domain satisfies the axiom of infinity. But this would mean that the domain of predicates of an axiomatic theory in which the forms of the universal and the existential judgment (the existential reasoning) are applied only to objects and not to predicates cannot be enlarged by the introduction of impredicative definitions, provided only that the axiom system requires for its satisfaction an infinite system of objects.

But the correctness of such a statement is out of the question. One can easily construct examples which refute this claim.

[13]The example is chosen in such a way that the reference to the totality of numbers can not be eliminated directly as is the case in most of the simpler examples.

Dedekind's introduction of the concept of number furnishes such an example. Dedekind starts with a system in which a thing 0 is distinguished and which permits a one-one mapping onto a subset not containing the thing 0. Suppose we represent this mapping by a predicate with two subjects and formulate the required properties of this predicate as axioms; we then get an elementary axiom system that contains in its axioms no reference to the totality of predicates and that, moreover, can be satisfied only by an infinite system of objects. Let us now consider Dedekind's concept of number; if we translate his definition from the language of set theory into that of the theory of predicates, it can be formulated in full analogy to Frege's definition of finite number: "a thing n of our system is a number if every predicate holds of n which holds of 0 and which, if it holds of a thing a in our system, holds also of the thing to which a is correlated by the one-one mapping." This definition is impredicative; and one can see that it is not possible to obtain a predicate that is extensionally equal to the hereby defined concept of "being a number," by a predicative definition from the basic elements of the theory.[14]

We find, therefore, that for the axiom of reducibility, only the second interpretation comes into consideration, according to which it expresses a *condition on the initial domain of prelogical predicates.*

By introducing such an assumption one abandons the conception that the domain of predicates is generated by logical processes. The aim of a genuinely logical theory of predicates is then given up.

If one decides to do this, then it seems more natural and appropriate to return to the conception of a *logical function* that corresponds to Schröder's standpoint: one construes a logical function as an assignment of the values "true" and "false" to the objects of the domain of individuals. Each predicate defines such an assignment; but the totality of assignments of values is construed, in analogy with the finite, as a *combinatorial manifold which exists independently of conceptual definitions.*

This conception removes the circularity of the impredicative definitions of theoretical logic; we have only to replace any statement about the totality of predicates by the corresponding statement about the totality of logical functions. The axiom of reducibility is thus dispensable.

This step was actually taken by the logicist school at the suggestion of Wittgenstein and Ramsey. These two maintained in particular that in order to avoid the contradictions connected with the concept of the set of

[14]Another example was given by Waismann in a note on "The nature of the axiom of reducibility" (*vide* [**?**]). This, however, requires some modification.

all mathematical objects it is not necessary to distinguish predicates by their definitions, as Whitehead and Russell had done in *Principia Mathematica*. Rather, they maintained, it suffices to delimit clearly the domains of definition of predicates, so that one distinguishes between the predicates of individuals, the predicates of predicates, the predicates of predicates of predicates, and so on.

In this way one has returned from the type theory of *Principia Mathematica* to the simpler conceptions of Cantor and Schröder.

However, one should not be deceived over the fact that with this change one has moved far away from the standpoint of logical self-evidence. The assumptions on which theoretical logic is then based are in principle of exactly the same kind as the basic postulates of analysis, and are also completely analogous to them in content. The axiom of infinity in the logical theory corresponds to the conception of the number sequence as an infinite totality; and in the logical theory one postulates the concept of all logical functions instead of the concept of all sets of numbers, whereby the functions refer to the "domain of individuals" or to a determinate domain of predicates.

Thus, when arithmetic is incorporated into the system of theoretical logic, nothing is saved in terms of assumptions. Contrary to what one might at first think, this incorporation by no means has the significance of a reduction of the postulates of arithmetic to lesser assumptions; its value is rather in the fact that the mathematical theory is placed on a broader basis by joining it with the logical formalism.

In this way the theory attains, first of all, a higher degree of methodological distinction, as follows. Not only do its assumptions result from a natural extrapolation of intuitive numbers, but they are also obtained by *extrapolating* the *logic of extensions* to infinite totalities.

Moreover, by joining arithmetic with theoretical logic we gain an insight into the connection of the processes of set formation with the fundamental operations of logic; and the logical structure of concept formation and of inferences becomes clearer.

Thus, in particular, the meaning of the Principle of Choice becomes fully comprehensible only by means of the formalism of logic. We can express this principle in the following form: if $B(x, y)$ is a two-place predicate (defined in a certain domain) and if for every thing x in the domain of definition there is at least one thing y in this domain for which $B(x, y)$ holds, then there is (at least) one function $y = f(x)$, such that for every thing x in the domain of definition of $B(x, y)$ the value $f(x)$ is again in this domain, and is such that $B(x, f(x))$ holds.

Let us consider what this assertion claims in the special case of a two element domain, the things of which we can represent by the numbers 0, 1. In this case there are only four different courses of values of functions $y = f(x)$ to consider. Then the assertion is a simple application of one of the *distributive laws* governing the relation between conjunction and disjunction, i. e. the following theorem of elementary logic: "If A holds and if, in addition, B or C holds, then either A and B holds or A and C holds."[15]

Also in the case of a subject domain consisting of any determinate finite number of things, the assertion of the Principle of Choice follows from this distributive law. The general assertion of the Principle of Choice is therefore nothing but the extension of a law of elementary logic for conjunction and disjunction to infinite totalities. And thus, the Principle of Choice supplements the logical rules governing universal and existential judgments, i. e. the rules of existential inference, for their application to infinite totalities signifies in the same way that certain elementary laws for conjunction and disjunction are being carried over to the infinite.

The Principle of Choice has a distinctive position with respect to these rules of existential inference only insofar as its formulation requires the *concept of function*. This concept, in turn, receives its sufficient implicit characterization only by means of the Principle of Choice.

This concept of function corresponds to the concept of logical function; the only difference is that the values of the former are not taken to be "true" and "false" but the things of the subject domain. The totality of the functions that are being considered here is therefore the totality of all possible "self-assignments" of the subject domain.

According to this concept of function the existence of a function with the property E in no way means that one can form a concept that uniquely determines a definite function with the property E. Consideration of this circumstance invalidates the usual objections to the Principle of Choice, which rests mostly on the fact that one is misled by the name "Principle of Choice" to the view that this principle asserts the possibility of a choice.

At the same time we recognize that the assumption expressed by the Principle of Choice does not fundamentally go beyond the understanding upon which we have to base, in any event, the procedure of theoretical logic

[15] "Or" is in both cases meant not in the sense of the exclusive "or" but in the sense of the Latin "vel." But of course the theorem also holds for the exclusive "or."

in order to interpret it in a circle-free manner without introducing an axiom of reducibility.

To be sure, we can also give contrary emphasis to this observation: the controversial character of the Principle of Choice, the formulation of which is in line with the systematic elaboration of the standpoint of theoretical logic, brings most strongly to the fore what is problematic about this standpoint.

When we considered the logicist foundation of arithmetic we were also led to this result: the incorporation of arithmetic into theoretical logic provides indeed a broader foundation for the arithmetic theory and contributes to the contentual motivation of its assumptions; but it does not lead beyond the methodological standpoint of the conceptual approach, i. e. beyond the standpoint of axiomatics.

In this way the problem of the infinite is formulated, but it is not solved. For there remains the open question whether the analogies between the finite and the infinite, postulated as assumptions for the development of analysis and set theory, constitute an admissible approach, i. e. one which can be carried out consistently.

Intuitionism tries to avoid this question by excluding the problematic assumptions, while most logicists dispute its legitimacy by denying a fundamental difference between the finite and the infinite; *Hilbert's proof theory* begins to address this question in a positive way.

§ 4 Hilbert's proof theory

In order to better grasp the leading ideas of proof theory let us first call to mind once again the character of the problem to be solved here. At issue is proving the consistency of the mathematical concept formation on which the theory of arithmetic rests.

On the philosophical side, the question has frequently been raised whether a proof of consistency alone provides a justification for this concept formation . This way of putting the question is however misleading; it does not take into account the fact that the scientific motivation for the theoretical approach of arithmetic has been provided in essence already by science and that the proof of consistency is the only *desideratum* that remains to be fulfilled.

The edifice of arithmetic is built on the foundation of conceptions which are of greatest relevance for scientific systematization in general: namely the principle of *conservation* ("*permanence*") *of laws*, which occurs here as the postulate of the unlimited applicability of the usual logical forms of judgment and inference, and the demand for a purely *objective* formulation of the theory, by which it is freed from all reference to our *cognition*.

The fundamental methodological significance of these requirements yields the *inner* motivation and distinctive character of the approach of the arithmetic theory.

In addition to this inner motivation we have the splendid corroboration of the conceptual system of arithmetic in the form of its deductive fruitfulness, its systematic success, and the coherence of its consequences. This conceptual system is clearly suited in a truly remarkable way to treating the relations of numbers and of magnitudes. The systematicity of this magnificent theory, obtained by combining function theory with number theory and algebra, has no equal. And as a comprehensive conceptual apparatus for the construction of scientific theories, arithmetic proves to be suited not only to the formulation and development of laws, but it has also been used with great success, and to an extent which had not been anticipated, in the search for laws.

Regarding the coherence of the consequences, it has been most strongly corroborated by the intensive theoretical development of analysis and its many numerical applications.

What is still lacking here is only this: that the merely empirical trust, gained by many trials, in the consistency of the arithmetic theory, i. e. in the thorough coherence of its results, be replaced by a real insight into this consistency; to effect this is the purpose of a proof of consistency.

Thus, it is not the case that the conceptual system of arithmetic must first be established by means of a proof of its consistency. Rather, the sole purpose of this proof is to give us with regard to this conceptual system (which is already motivated on internal systematic grounds and has proved itself as an intellectual tool in its applications), the evident certainty that it cannot be undermined by the incoherence of its consequences.

If this succeeds, we will know that the idea of the actual infinite can be developed systematically. And we can rely on the results of applying the basic arithmetic postulates just as if we were in the position to verify them intuitively. For when we recognize the consistency of the application of these postulates, it follows immediately that their consequences, if they are intuitively, i. e. finitistically, meaningful, can never contradict an intuitively recognizable fact. In the case of finitist sentences, the ascertainment of their nonrefutability is equivalent to the ascertainment of their truth.

From this consideration of the need for and the purpose of a consistency proof, it follows in particular that for such a proof only one thing matters; namely to recognize, in the literal sense of the word, the freedom from contradictions of the arithmetic theory, i. e., the *impossibility of its immanent refutation*.

The novel feature of Hilbert's approach was that he limited himself to this problem; previously, one had always carried out consistency proofs for axiomatic theories by positively exhibiting the simultaneously satisfaction of the axioms by certain objects. There was no basis for this method of exhibition in the case of arithmetic; in particular, Frege's idea of taking the objects to be exhibited from the domain of logic does not succeed, because, as we have recognized, the application of ordinary logic to the infinite is just as problematic as arithmetic, the consistency of which was to be shown. Indeed, the basic postulates of the arithmetic theory concern exactly the extended application of the usual forms of judgment and inference.

By focusing on this aspect, we are led directly to the *first guiding principle of Hilbert's proof theory*: it says that, in proving the consistency of arithmetic, we must consider the laws of logic as applied in arithmetic to be in the domain of what is to be shown consistent; thus, the proof of consistency covers *logic and arithmetic together*.

The first essential step in carrying out this idea is already taken by incorporating arithmetic into the system of theoretical logic. Because of this incorporation the task of proving the consistency of arithmetic reduces to establishing the consistency of theoretical logic, or, in other words, determining the consistencyof the axiom of infinity, of impredicative definitions, and of the Principle of Choice.

In this connection it is advisable to replace Russell's axiom of infinity with Dedekind's characterization of the infinite.

Russell's axiom of infinity requires the existence of an n-numbered predicate for every finite number n (in the sense of Frege's definition of finite cardinal) and thus implicitly requires also that the domain of individuals (the basic domain of things) be infinite. Now it is an unnecessary and also from a principled standpoint objectionable complication that here three infinities in different layers run concurrently: that of the infinitely many things in the domain of individuals, furthermore that of the infinitely many predicates, and then that of the resulting infinitely many cardinals, which are after all defined as predicates of predicates.

We can avoid this multiplicity by determining the infinity of the domain of individuals not by an infinite series of unary predicates, but rather by a single binary predicate, namely a predicate that provides a one-one mapping of the domain of individuals onto a proper subdomain, i. e. a subdomain which excludes at least one thing. This characterization of the infinite, due to Dedekind, can be introduced in the most simple and elementary way if we do not postulate the one-one mapping by means of an existence axiom, but introduce it explicitly from the start by taking as basic elements of the theory an initial object and a basic process.

In this way we achieve that the numbers occur already as things in the domain of individuals, rather than as predicates of predicates of things.

However, this consideration already refers to the particular form of the systematic development, and there are several ways of pursuing it. But we must first orient ourselves as to how in general how a proof of consistency in the intended sense can be carried out at all. This possibility is not immediately obvious. For how can one survey all possible consequences that follow from the assumptions of arithmetic or of theoretical logic?

Here the investigation of mathematical proofs by means of the logical calculus comes into play in a decisive way. This has shown that the methods of forming concepts and making inferences which are used in analysis and set theory are reducible to a limited number of processes and rules; thus, one succeeds in completely formalizing these theories in the framework of a precisely specified symbolism.

Hilbert inferred from the possibility of this formalization, which was done originally only for the sake of a more precise logical analysis of proof, the *second guiding idea of his proof theory*, namely that the task of proving the consistency of arithmetic is a *finitist problem*.

An inconsistency in the contentual theory must indeed show itself by means of the formalization in the following way: two formulas are derivable according to the rules of the formalism, one of which results from the other though that process which is the formal image of negation. The claim of consistency is therefore equivalent to the claim that two formulas standing in the above relation can not be derived by the rules of the formalism. But this claim has fundamentally the same character as any general statement of finitist number theory, e. g., the statement that it is impossible to produce three integers a, b, c (different from 0) such that $a^3 + b^3 = c^3$.

The proof of consistency for arithmetic thus becomes in fact a finitist problem of the theory of inferences. The finitist investigation having formalized theories of mathematics as its object is called by Hilbert *metamathematics*. The task falling to metamathematics *vis-à-vis* the system of mathematics is analogous to the one which Kant ascribed to the critique of reason *vis-à-vis* the system of philosophy.

In accord with this methodological program, proof theory has already been developed to a substantial degree;[16] but there are still considerable mathematical difficulties to be overcome. The proofs of Ackermann and von Neumann secure the consistency of the first postulate of arithmetic, i. e., the applicability of existential reasoning to the integers. Ackermann developed in some detail an approach to the further problem of the consistency of the general concept of a set (resp. numerical function) of numbers together with a corresponding Principle of Choice.

If this problem were solved, then almost the entire domain of existing mathematical theories would be proved to be consistent.[17] This proof would in particular be sufficient to recognize the consistency of the geometric and physical theories.

One can also extend the problem still further and investigate the consistency of more inclusive systems, e. g., axiomatic set theory. Axiomatic set theory, as first formulated by Zermelo and supplemented and extended by Fraenkel and von Neumann, with its construction processes, already goes far beyond what is actually used in mathematics; and the proof of its consistency would also establish the consistency of the system of theoretical logic.

This does not achieve an absolute completion of this formation of concepts, because formalized set theory motivates metamathematical considerations which have the formal constructions of set theory as their object and in this way go beyond these constructions.[18]

In spite of this possibility of extending the concept formation a formalized theory can nevertheless be closed in the following sense: no new results are

[16]Hilbert gave a first sketch of a theory of proofs already in his 1904 Heidelberg lecture "On the foundations of logic and arithmetic" (*vide* [**?**]). The first guiding idea of a joint treatment of logic and arithmetic is expressly formulated here; the methodological principle of the finitist standpoint is also intended, but not yet explicitly stated.— The investigation of Julius Koenig, *New Foundations for Logic, Arithmetic, and Set Theory* (*vide* [**?**]), falls between this lecture and Hilbert's more recent publications on proof theory; it comes very close to Hilbert's standpoint and gives already a proof of consistency which is in full accord with proof theory. This proof covers only a very narrow domain of formal operations and is therefore only of methodological significance.

[17]Cantor's theory of numbers of the second number class is also included here.

[18]The more detailed discussion of this point is connected to the *Richard paradox*, of which Skolem has recently given a more precise formulation. These considerations are not conclusive since they are made in the framework of a non-finitist metamathematics. A final answer to the question discussed here would be obtained only if one succeeded in producing in a finitist way a set of numbers which could be shown not to occur in axiomatic set theory.

obtained in the domain of the laws that can be formulated in terms of the concepts of the theory by extending the concept formation.

This condition is satisfied whenever the theory is *deductively closed*, i. e., when it is impossible to add a new axiom, which is expressible in terms of the concepts of the theory but not already derivable, without producing a contradiction,—or, what amounts to the same thing: if every statement that can be formulated within the framework of the theory is either provable or refutable.[19]

We believe that number theory as delimited through Peano's axioms with the addition of definition by recursion is deductively closed in this sense; but the problem of giving an actual proof of this is still entirely unsolved. The question becomes even more difficult if we go beyond the domain of number theory and ascend to analysis and the further set theoretic concept formations.

In the realm of these and related questions there lies a considerable field of open problems. But these problems are not of such a kind that they represent an objection to the standpoint we have adopted. We must only keep in mind that the formalism of theorems and proofs that we use to represent our ideas does not coincide with the formalism of the structure that we intend in thought. The formalism suffices to formulate our ideas of infinite manifolds and to draw the logical consequences from them; but in general it is not able to produce the manifold combinatorially out of itself, so to speak.

The position we have reached concerning the theory of the infinite can be viewed as a form of the philosophy of the "as if." It differs fundamentally from the Vaihinger's philosophy thus designated, however, by placing weight on the consistency and the permanence of ideas; in contrast, Vaihinger considers the demand for consistency to be a prejudice and indeed claims that the contradictions in the infinitesimal calculus are "not only not to be disavowed, but . . . [<u>are</u>] precisely the means by which progress was attained."[20]

Vaihinger's considerations are focused exclusively on scientific *heuristics*. He considers only the "fictions" that occur as mere temporary aids for think-

[19]Notice that this requirement of being deductively closed does not go as far as the requirement that every question of the theory be *decidable*. The latter says that there should be a procedure for deciding for any arbitrarily given pair of contradictory claims belonging to the theory which of the two is provable ("correct").

[20]Vaihinger, *The Philosophy of "As if"*, 2nd edition, ch. XII (*vide* [**?**], p. ■).

ing. In introducing these fictions, thought does itself violence and their contradictory character (if we are dealing with "genuine fictions") can be rendered harmless only by a skillful adjustment for the contradictions.

Ideas in our sense are a permanent possession of the mind. They are distinguished forms of systematic extrapolation and of idealizing approximation to what is real. They are also by no means arbitrary nor yet forced upon thought; on the contrary, they constitute a world in which our thinking feels at home and from which the human mind, absorbed in this world, gains satisfaction and joy.

Postscript

Because of various insights that have been gained since the publication of the above essay, some of the considerations presented here have to be corrected.

First of all, as far as intuitionism is concerned, it was initially believed that the methodology of intuitionistic proofs agreed with that of Hilbert's "finitist standpoint." It has become clear, however, that the methods of intuitionism go beyond the finitist proof procedures intended by Hilbert. In particular, Brouwer uses the general concept of a contentual proof, to which the concept of "absurdity" is also connected, but which is not employed in finitist reasoning.

Then, as far as Hilbert's proof theory is concerned, the view that the consistency proof for arithmetic amounts to a finitist problem is justified only in the sense that the consistency statement can be formulated finitistically. This does not imply at all that the problem can be solved with finitist methods. By a theorem of Gödel the possibility of a finitist solution was made most implausible, though not directly excluded, already for number theory; moreover, it turned out that the above mentioned consistency proofs that were available at the time did not extend to the full formalism of number theory. The methodological standpoint of proof theory was consequently broadened, and various consistency proofs were carried out, first for formalized number theory and then also for formal systems of analysis; their methods, although not restricted to finitist, i. e., elementary combinatorial considerations, require neither the usual methods of existential reasoning, nor the general concept of contentual proof.

In connection with the theorem of Gödel mentioned above, the assumption that number theory, when axiomatically delimited and formalized, is deductively complete turned out to be incorrect. Even more generally, Gödel showed that formalized theories satisfying certain very general conditions of expressiveness and formal rigor cannot be deductively complete as long as they are consistent.

On the whole the situation is as follows: Hilbert's proof theory, together with the discovery of the possibility of formalizing mathematical theories, has opened a rich area of research, but the epistemological perspective which motivated its formulation has become problematic.

This suggests revising the epistemological remarks in the above essay. Of course, the positive remarks are hardly in need of revision, in particular those exhibiting the mathematical element in logic and emphasizing the evidence of elementary arithmetic. However, the sharp distinction between the intuitive and the non-intuitive, which was employed in the treatment of the problem of the infinite, apparently cannot be drawn so strictly, and the reflections on the formation of mathematical ideas still need to be worked out in more detail in this respect. Various considerations for this are contained in the following essays.[a]

[a]This refers to the remaining essays in the collection [**?**].

Chapter 8

Bernays Project: Text No. 10

The of Friesean Philosophy in Relation to Contemporary Science (1930)

Die Grundgedanken der Fries'schen Philosophie in ihrem Verhältnis zum heutigen Stand der Wissenschaft.

(*Abhandlungen der Fries'schen Schule*, NF, 5 (1929–1933), no. 2 (1930), pp. 97–113)

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Dear schoolmates! In having the honor to speak in the context of a meeting devoted to the memory of Leonard Nelson, I would like first to set forth briefly the purpose of my presentation.

Nelson was the appointed head of his school not only because of the sharpness of his thought but also on account of his overall personality. Such a personality gathers among its followers people who in part differ very widely in their opinions, each of whom takes from the whole of the philosophical doctrine what for him is essential in this doctrine. This fact makes itself felt when such an intellectual leader passes away. For the members of the school, and indeed for every single one of them, the question arises in what manner he should preserve and elaborate the received thoughts for himself, and also how to further bring them to bear outwardly. In our case this question arises all the more as Nelson's system of thought is devoted to the reawakening and further elaboration of a philosophy which one had more or less considered wrapped up; and it arises all the more as in this philosophy the work of a philosopher has already been elaborated by a thinker differing in many respects, as Fries did in comparison to Kant.

Each of us can answer this question only for himself. However, allow me to propose certain ideas in this respect, whereby I do not make any claim to a complete treatment of the topic, if only for the reason that I will speak here only about questions concerning the critique of knowledge. I would like to stress a certain, uniform complex of thoughts of the Kant-Friesian school which seems to me, in any case, to retain its important role in philosophy.

As you know, there is a certain discrepancy between several claims of the Kant-Friesian philosophy and present-day scientific theories. This discrepancy is very clear and coarse. But it is not that striking that many things in present-day science develop in such a way that makes it possible, if stressed in the proper way, to bring to bear the thoughts of the Kant-Friesian philosophy again, provided only that one is prepared to make certain modifications to it.

Above all I mean those thoughts that constitute transcendental idealism and the difference between intuitive knowledge and purely conceptual knowledge.

If we consider the most recent philosophical doctrines, we find that most of them oppose transcendental idealism in principle. It is especially the philosophy of immanence of Mach's school that is widespread among researchers in the exact sciences and indeed it dominates almost absolutely. This philosophy claims to be able to eliminate the notion of existence in general and to get by with the notion of phenomenon. According to it, there is fundamentally no other kind of knowledge than perceiving, remembering, following the sequence of representations and comparing the contents of representation.

The difficulties of this position are known to you. I need not consider them more closely. I would only like to point out that Willy Freytag in his book *Realism and the Problem of Transcendence* (*vide* [**?**]) explains very well the weaknesses of the position of immanence. Moritz Schlick also follows this book in certain parts of his *General Theory of Knowledge* (*vide* [**?**]);

however, he again slips back into the position of immanence in another way when characterizing cognition as recognition from the outset, thus restricting cognition again to a mere comparison of the given.

Phenomenalism has received certain refinements. One of these is found today in the Russellian school of mathematical logic. Here the domain of the intuitively given is enlarged by certain logical constructions. It is characteristic in this connection that one essentially deals here only with class constructions, that is only with an abstract kind of comparison. What is united into classes are either contents of representations or classes already constructed. In principle one does not go with this beyond phenomenalism, for Mach and his school as well have considered the construction of concepts as essential in addition to direct intuitive representations.

But the tendency to a restriction to the immanent is quite widespread not only in those approaches to philosophy that are tied to the exact sciences but also in the philosophy characterized as spiritual. An especially remarkable and engaging form of the standpoint of immanence is that adopted in Husserl's phenomenological school. There the principle of *displayability of every single phenomenon* is posed as a methodological guideline, i. e. the requirement that every concept or term introduced be justified by displaying a *phenomenon* determined by it. If this principle is understood in a sufficiently wide sense, there is nothing to object against it. But there is the obvious interpretation, and it is applied by many followers of the school, according to which our reasoning has to remain in the domain of *phenomena*, i. e. contentual representation, that therefore nothing beyond the given can be reasonably thought of at all. By the way, it is remarkable that Oskar Becker in his book *Mathematical Existence* (*vide* [**?**]) recently called this standpoint transcendental idealism.

Among the philosophical directions known today there is arguably not a single one that is opposed to the positions just mentioned as fundamentally as the doctrine of Fries. Fries laid stress on exactly *what* all these philosophers endeavor to argue away, namely the fundamental transcending of the contentual standpoint by the forms of thought. The categorial formation of the judgment can only be understood as the expression of a "demand of cognition," as expression of a search, guided by a belief that is already inherent opaquely in every perception and generally in every state of consciousness, but which makes itself explicit in a clearer form only through thought. This belief gives us the conviction that the contents found in experience are to be related to a *reality*, to a unity of *existing* objects, that is in itself real and structured into real connections.

It can be explained why one has serious problems in making up one's mind to accept this doctrine. First of all, one would like to have a standpoint with as few presuppositions as possible, and with the assumption of the rational belief too much seems to be postulated at the outset. Upon closer examination, this objection does not apply to the Friesian doctrine of conceptual knowledge as such, but to the view that the content of this knowledge can be rendered in entirely distinct, definitely formulated principles. Anyway, I would like to point out that the fundamental idea of the Friesian doctrine is by all means compatible with the fact that the way in which, in the investigation of nature, we relate the contents of experience to existing objects by reasoning is not determined in knowledge but belongs itself to the task of research that is given to us by reason.

There is however another reason for the resistance against the Friesian doctrine. I leave out of consideration here the known difficulties related to the question of the correct characterization of the mode of existence of reason and its expressions. It has been very much discussed, especially in our school, whether conceptual knowledge has to be regarded psychologically as a faculty or as a continuous activity. These are difficulties and problems but not really objections; they are objections only for the one who, again in the domain of psychology, intends to carry out the standpoint of a complete restriction to contents. Fries thought in this respect more vitally; he did not want to be content with a theory of psychological phenomena, but aimed at a theory of the unit of life; and I think we have reason to agree with him in this respect. What forms, however, a more substantial reason for the resistance against the Friesian claims is that on closer inspection one recognizes that one is thereby already necessarily pushed towards *transcendental idealism*. Because in the fact that conceptual knowledge makes itself felt in the form of a categorial requirement of an existential relation (otherwise no more precisely determined) to a world of *existing*, there lies already the division of truth. Both the contentual and the categorical form belong to knowledge as such. According to the position of naive realism we believe to find both united and to have in common perception complete knowledge before us. Closer inspection forces us in a well-known way to give up this position; it becomes manifest to us that the experiential uniform perception consists of two distinct parts in regard to knowledge: the givenness of a contentual material and the existential reference to the unity of reality in which the former has to be integrated in a manner initially unknown.

The fundamental imperfection of our knowledge is based on this. We know the contents of our experience and can talk about them; but how to interpret them as proper truth is only very fragmentarily known, although to an extent that is sufficient for the purposes of our practical standards of living, within which we help ourselves with a general attitude based on beliefs in those domains where our scientific knowledge no longer suffices.

If we introduce transcendental idealism that way independently from the doctrine of antinomies we can thereby remain in complete agreement with Fries. For the doctrine of the division of truth, which Fries subsequently puts forward for the resolution and explanation of the antinomies, does not need the antinomies for its grounding. And this is a methodological advantage, since the doctrine of the antinomies contains very many problematic arguments. Above all there is the risk of proving too much by posing statements in the antithesis that are by no means irrevocable in principle for scientific thinking and therefore assign boundaries to science, which in fact it does not have. Transcendental idealism must not be understood in such a way that it produces a factual-structural discrepancy between what is given in reality and what is asserted in the scientific world view. If science has to have meaning we have to hold the standpoint that what is claimed in science as factual—as far as it is not a common error in the sense of science itself—also expresses a fact of reality, and in any case it does not deviate from reality in such a way that is expressible in the framework of science itself. The limitation of scientific knowledge has therefore to be based in a proper sense on the conditions of possibility of the scientific investigation of nature as such.

Such a condition is, in the first place, the connection to perception. The considerations which force us to give up naive realism and in general to eliminate sensible qualities in the physical reflection have to be imputed to the antinomies. The discursive character of science is a further essential condition that comes from the fact that conceptual knowledge is conveyed to us through thought. In fact even here something arises which in any case is inadequate to reality, namely the hypothetical form of the laws of nature. It is not in accordance with the idea of a real connection that the latter consists in a law according to which something takes place under certain circumstances. Such a law can only be a reason but not a real cause. Thus, while the aforementioned antinomy refers to the fact that we don't have knowledge of *existing* in its essence but only as something that stands in certain relations, the second antinomy concerns the lack of essence of the *connection*. The existence of still other antinomies, especially of the kind posed by Kant, should in no way be disputed in principle. But in any case a revision of the given, which goes farther than what has been carried out so far in our school, is necessary.

If we observe how factual natural science relates to the program of pure immanence we find that one has departed from the observance of a phenomenological program more than ever, despite the conscious emphasis on Mach's thoughts, which were also propounded in particular by Einstein. There we have completely abstract existence claims, which are related to perception only in their consequences. This is especially true for present-day quantum theory. According to this theory the physical state is related to perception only through *probability* statements, i. e. the physical states whose temporal connection is a wave-theoretical causal one have relevance for perception only through the fact that they involve certain discrete processes in a statistical frequency, computable from the state variables, and these frequencies and also other quantitative determinations of those processes present themselves for the experiment through intuitive quantities, e. g., color and intensity of spectral lines.

Likewise, Einstein's general relativity theory by no means conforms to the tendencies of a pure phenomenalism. The lawfulness of the space-time manifold is here introduced purely conceptually through the assumption of a metric field that forms a physical object analogous to the electromagnetic field. The quantitative distribution of this field is correspondingly determined by spatio-temporal measurements, similarly to the way the shape of the earth is determined by measurements of lengths on the base of our ordinary intuition of space. However, whereas the earth transcends our power of imagination only because of its size, the metric field is in principle out of the range of the intuitively imaginable on account of the union of the spatial and the temporal which takes place in it.

The establishment of such theories, which are very far removed from observation, speaks very strongly in favor of the Friesian doctrine of only conscious conceptual knowledge. Sure enough, these theories cannot be reconciled with the Kant-Friesian doctrine of *pure intuition*. But we do not need, also in this case, to give up this doctrine as a whole in order to stay in harmony with contemporary scientific theories, but only its specific formulation.

Thus, one rightly disputes the Kantian claim that geometry and physics are bound by the framework of our intuitive representations of space and time as a condition of possibility of scientific knowledge. In fact, in its abstractions geometry goes far beyond the framework of the intuitive representation of space by having developed into a general theory of ordered manifolds endowed with topological relations within which the laws of Euclidean geometry form only a special structural lawfulness distinguished by systematic advantages.

Moreover, concerning theoretical physics, its recent development has shown with full clarity that the possibility of theoretical knowledge of nature is completely independent from the acceptance of a determinate structural lawfulness of space and time.

In another respect, however, the Kantian doctrine of pure intuition has curently again gained recognition. For a long time heretofore, the dominant opinion was that mathematics could be developed purely out of logic. The attempt to carry out this idea, as it was initially undertaken by Frege and then by Whitehead and Russell has not succeeded, regardless of the systematic unity of the work *Principia Mathematica*. Rather, the investigation of the foundations of mathematics has shown two things. First, that a certain kind of purely intuitive knowledge has to be taken as a starting point for mathematics; indeed, that even logic as the theory of judgments and inferences cannot be developed without appealing to such an intuitive knowledge to some extent. It is an issue of the intuitive representation of the discrete from which we draw the most primitive combinatorial representations, in particular that of succession. Constructive arithmetic develops by means of this elementary intuitive knowledge. Secondly, it appears that constructive arithmetic is not sufficient for the theory of real numbers, that rather for the latter we have to add certain notions related to the totality of collections of mathematical objects, e. g. the totality of all the numbers and the totality of all sets of numbers.

It is now remarkable that Fries—in his *Mathematical Philosophy of Nature* (*vide* [**?**])—already separated the elementary kind of mathematical knowledge under the name "syntactics" from arithmetic in the sense of a theory of quantities. He says about syntactics:

> It "contains the most general abstraction which can be done for mathematical knowledge whatsoever. It is solely based on the postulates of the *arbitrary order of given elements* and their *arbitrary repetition without end*. It has no proper theory, for it does not know any axioms; its operations are for themselves immediately comprehensible . . . " (p. 70)

In his considerations on syntactics, however, Fries only thought of the doctrine of permutations and combinations, whereas he treated number theory only in connection with analysis. He stated:

> "The purpose of the number system is generally to reduce the knowledge of quantity to concepts, i. e. to recognize the relationships between quantities not only intuitively but also through thought." (p. 121)
>
> "The specific pure intuition of arithmetic is the continuous series of the larger and the smaller. By scientifically developing this pure intuition we should *think* the idea of quantity or reduce it to concepts." (p. 77)

In order to pass from these Friesian views to a conception in accordance with the present state of research one does not need very substantial modifications. Of course, we have to include elementary number theory in the domain of syntactics. Moreover, it cannot be taken for granted that the scientific development of the concept of quantity consists only in the clarification of pure intuitive knowledge. Rather, we have to take the possibility into account that we are dealing here with a conceptual sharpening, an "idealization"—as Felix Klein called it—of the intuitive representation of the larger and the smaller. Even so, the rational element would not yet have been excluded from the arithmetical theory of quantities (of analysis). This is because that conceptual sharpening takes place, as already said, by including certain representations of totality, and thereby we would have to see what reason adds to the intuitive representation. This is supported especially by the fact that the representations of totality applied in analysis become relevant to the system of mathematics by making possible the unrestricted application of the logical *forms of the general and the particular judgment* in the domain of real numbers and functions. And according to Fries the logical forms of judgments are exactly those through which we become aware of conceptual knowledge in thought.

In the sense of such a conception, analysis would already contain a component of conceptual knowledge grasped only by thought. It would thus have the same epistemological character that Fries assigns to pure natural science, and indeed, in contemporary science, mathematics has entirely the role of pure natural science, the "armory of hypotheses," as Fries puts it.

It is also characteristic that—right from the beginning of the rigorization of infinitesimal methods—some sort of phenomenological opposition arose against the rational element in analysis. At first Kronecker and at present Brouwer and his school propound a position that calls for the restriction to the intuitively representable and according to which the totality assumptions of analysis mentioned above are categorically rejected. Lately Weyl has hinted at the analogy between this "Intuitionism" and Mach's standpoint.

Hilbert shows, in a completely different way than this opposition, the relevance of his *proof theory* for the special epistemological position of elementary intuitive (syntactical) or, as Hilbert calls it, "finitist" mathematics, *vis-à-vis* systematic mathematics, based on concept formations, in particular analysis and set theory. Hilbert here subjects systematic mathematics to a sort of critique of proof by which, using elementary finite methods, the deductive consequences of the concept formations of systematic mathematics are investigated, whereby the aim is to show that the application and the pursuit of these concept formations can never lead to discrepancies in the consequences and thus also, in particular, that it cannot lead to contradictions with the elementary intuitively recognizable facts.

For a *philosophical completion* of this proof theory a *methodological explication* is necessary by which those principles systematized in proof theory receive some kind of deduction in the sense of a clarification of their epistemological methodological significance. This explication should at the same time clarify the methods of mathematical idealization and with this give a satisfying answer to Nelson's question, what the norm for an idealization could consist, if it does not lie in pure intuition.

In conclusion, I would still like to indicate how the special status of aesthetics becomes understandable through the doctrine of transcendental idealism. In the language of our school the expression "aesthetic" is used for all those objective evaluations, whose measure cannot be conceptually grasped. It appears to be appropriate—on the one hand with respect to the ordinary use of language and also for pointing out essential differences—to restrict the use of the word "aesthetic" to that kind of evaluation in which an object is valued as a *symbolical representation* for something which is not directly accessible to our finite knowledge of nature. According to this, the value of an aesthetic object as such does not attach to the thing as actually existing, as in the case of the value of a noble character whose existence has value for itself, but rather that value is principally related to the representing subject, i. e. the object is only valuable as represented. The objective character of an aesthetic value consists in the objective determination of the *suitability* of an object to serve as a symbolic expression. The interest for such a symbolic expression depends essentially on the imperfection of our view of nature, i. e. the division of truth. We value the symbolic expression of ethical values in the beauty of figures of nature and art, because we cannot directly intuitively represent the value of a being but only assign it conceptually. Likewise, we value the conceptual unity of scientific systems of thought as a surrogate for an immediate intuitive grasp of the unity in the connection of the real.

According to this view, theoretical science has—leaving out of consideration its vital significance for our orientation and our action—an *aesthetical significance*, in so far as we regard it solely under a systematic viewpoint. This conception indeed remains as the only option, if we do not want to exaggerate the role of exact sciences to that of a perfect worldview, or reduce it to that of a mere tool. Accordingly, the scientific systematization has not only the purpose of saving labor, but also an esthetic task that is given to us by reason. Only the doctrine of the belief of reason makes the search for a systematic unity and the success of such a search understandable; from Mach's standpoint this success is a pure miracle. On the other hand, from the doctrine of transcendental idealism we take the advice to moderate our expectation of a systematic completeness in the knowledge of nature.

With this I have sketched in what sense I think possible a vital preservation and continuation of the basic thoughts of the Friesian doctrine. You know that it was Nelson's special concern to ensure that the thoughts of Fries's philosophy would not be forgotten again. I believe that one also has to avert another risk, namely that these thoughts, although preserved in the tradition, be considered only from a historical perspective and not as standing in vital interaction with philosophical intellectual life. The purpose of my presentation was to show that the Friesian doctrine is capable of such a lively interaction with contemporary philosophy and that we do not need to worry that the basic ideas of this doctrine will be lost by modifications that take into account the development of science. Let us also take into consideration that it was Nelson's own intention to tackle, after completion of his system of ethics, the domain of speculative philosophy, and with this in particular, the philosophical methodology of natural science in the sense of a revision and a new treatment of Fries's thought.

Chapter 9

Bernays Project: Text No. 11

Methods for demonstrating consistency and their limitations (1932)

Methoden des Nachweises von Widerspruchsfreiheit und ihre Grenzen

(*Internationaler Mathematiker-Kongreß Zürich, II. Band: Sektionsvorträge*, pp. 201–202)

Translation by: *Dirk Schlimm*
Revised by: *CMU*
Final revision by: *Bill Tait, Richard Zach*

The methods that were used to prove the consistency of formalized theories from the finitist standpoint can be surveyed according to the following classification.

1. *Method of valuation.* It has obtained its essential development by Hilbert's procedure of trial valuation. Using this procedure Ackermann and v. Neumann demonstrated the consistency of number theory—admittedly, under the restrictive condition that the application of the inference from n to $(n+1)$ is only allowed to formulas with just free variables.

2. *Method of integration.* This can only be applied to such domains that are completely mastered mathematically. For those it allows one to give a completely positive answer not only to the question of consistency, but also to those of completeness and decidability. Such domains are in particular:

a) the monadic function calculus, which was treated conclusively by Löwenheim, Skolem, and Behmann.

b) Fragments of number theory. To such [formalisms] Herbrand and Presburger have applied the method. Thereby it becomes obvious that the Peano axioms, using the function calculus of "first order" (with the axioms for equality) as a foundation, do not yet suffice for the development of number theory. Only by adding the recursive equations for addition and multiplication do we arrive at full number theory[1].

3. *Method of elimination.* Its idea can already be found in Russell and Whitehead, in particular in the application to the concept "that which." However, the actual implementation of the idea is tedious. A significant simplification is brought about by Hilbert's approach, which ties in with the introduction of the "ε-symbol." First, this approach yields again—as has been shown by Ackermann—the result of the method of valuation in a simpler way.

From here, moreover, one arrives at a new proof of a theorem, which was discovered and proved for the first time by Herbrand. It is a converse to Löwenheim's famous theorem about satisfiability in countable domains and it also yields a general procedure for the treatment of questions about consistency.

The limitation of the results at hand presents itself, despite the insights obtained in multiple ways, as a fundamental one; this is because of Gödel's new theorem—and a conjecture by v. Neumann connected to it—on the limits of decidability in formal systems.

[1] The situation is different if one, like Dedekind, takes the standpoint of the logic of classes as basic from the outset; this standpoint, however, contains stronger assumptions than are needed for number theory.

Chapter 10

Bernays Project: Text No. 12

Foundations of Mathematics Vol. 1, §§ 1–2 (1934)

Grundlagen der Mathematik

(Berlin: Springer 1934, 2nd ed. 1968)

Translation by: *Ian Müller*
Revised by: *Volker Peckhaus, Mark Ravaglia; Michael Hallet, Dirk Schlimm; Bernd Buldt, Peter Winslow*
Final revision by: *CMU*

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§ 1 The problem of consistency in axiomatics as a logical decision problem

The state of research in the field of foundations of mathematics, to which our deliberations are related, is characterized by the outcome of three kinds of investigation:

1. the refinement of the axiomatic method, especially with the help of the foundations of geometry,

2. the grounding of analysis according to today's rigorous method by the reduction of the theory of magnitudes to the theory of numbers and sets of numbers;

3. investigations of the foundations of number theory and set theory.

On the basis of stricter methodological demands a broader set of tasks is connected to the standpoint reached through these investigations that involves a new way of dealing with the problem of the infinite. We want to introduce this problem by considering axiomatics.

The term "axiomatic" is used partly in a wider, partly in a narrower sense. We call the development of a theory axiomatic in the widest sense of the word, if the fundamental concepts and fundamental presuppositions are set out as such from the beginning, and the further content of the theory is logically derived from these with the help of definitions and proofs. In this sense EUCLID gave an axiomatic foundation for geometry, Newton for mechanics, and Clausius for thermodynamics.

The axiomatic standpoint was made more rigorous in HILBERT's *Foundations of Geometry*. The greater rigour consists in the fact that in the axiomatic development of a theory one retains only that portion of the representational subject matter, from which the fundamental concepts of the theory are formed, that is formulated as an extract in the axioms; one abstracts, however, from all other content. The *existential form* comes along as a further moment in axiomatics in its most narrow meaning, through which the axiomatic method is distinguished from the *constructive* or *genetic* method of grounding a theory.[1] Whereas in the constructive method the objects of a theory are introduced merely as a *genus* of things,[2] in an axiomatic theory one is concerned with a fixed system of things (or several such systems) which constitutes, from the beginning, a *delimited/circumscribed domain of subjects* for all the predicates out of which the statements of the theory are constituted.

Except in the trivial cases in which a theory has to do just with a finite, fixed domain of things, the presupposition of such a totality of the "domain of individuals" involves an idealizing assumption over and above the assumptions formulated in the axioms.

It is a characteristic of this more rigouros form of axiomatics that results through both abstraction from the subject matter and the existential form—we want to call it "formal axiomatics" for short—that it requires a *proof of consistency*, while contentual axiomatics introduces its fundamental concepts

[1]For this comparison, see Appendix VI to Hilbert's *Grundlagen der Geometrie*: Über den Zahlenbegriff [sic], 1900.

[2]Brouwer and his school use the word "species" in this sense.

by reference to known facts of experience and presents its basic principles either as obvious facts, which one can make clear to oneself, or as extracts from complexes of experiences, thereby expressing the belief that one is on the track of laws of nature and at the same time intending to support this belief through the success of the theory.

In any case, formal axiomatics requires certain evidence for the execution of deductions as well as for the proof of consistency, but with the essential difference that this kind of evidence is not based upon any particular epistemological relation to the respective subject domain. Rather, it is one and the same for every axiomatization; namely, that primitive kind of knowledge which forms the precondition of every exact theoretical investigation whatsoever. We will have to consider this kind of evidence more closely.

The following aspects are, above all, to be considered for the proper assessment of the relation between contentual and formal axiomatics with regards to their significance for knowledge:

Formal axiomatics necessarily requires contentual axiomatics as its supplement because only in terms of this supplement does one receive guidance for the selection of formalisms and, further, in the case of a given formal theory, does one then receive instruction of its applicability to some domain of reality.

On the other hand we cannot rest content at the level of contentual axiomatics, since in science we are if not always, so nevertheless predominantly, concerned with such theories that do not completely reproduce the actual state of affairs, but represent a *simplifying idealization* of that state of affairs and gain their meaning through this idealization. A theory of that kind cannot receive its foundation by appealing to the evident truth of its axioms or to experience; rather, the grounding can be brought about only in the sense that the idealization performed in the theory, i. e., the extrapolation—through which the concept formations and principles of the theory transcend the range of either intuitive evidence or the data of experience—is seen to be consistent. Addionally, any appeal to the approximate validity of the principles is of no use to us for this knowledge of consistency; for, of course, a contradiction can come about just from taking a relation to be strictly valid which only holds in a restricted sense.

We are therefore forced to investigate the consistency of theoretical systems, suppressing any considerations of matters of fact, and we thus already find ourselves on the standpoint of formal axiomatics.

Concerning the treatment of this problem up until now, this is done, both in the case of geometry and the branches of physics, with the help of the *method of arithmetization*: One represents the objects of a theory through numbers or systems of numbers and the basic relations through equations and inequalities in such a way that on the basis of this translation the axioms of the theory become either arithmetic identities or provable sentences (as in the case of geometry) or (as in physics) a system of conditions, the simultaneous satisfiability of which can be proved on the basis of arithmetical existential claims.

With this procedure arithmetic is assumed to be valid; i. e., the theory of real numbers (analysis) is presupposed. And so we come to the question of what kind this validity is.

But before we busy ourselves with this question, we want to see whether there is not a direct way of tackling the problem of conistency. Yet, we want clearly and generally to present the structure of this problem. At the same time, we want also to take the opportunity to somewhat familiarize ourselves with the *logical symbolism*, which proves to be very useful for the purpose at hand and which we will have to consider in more detail in the following.

As an example of axiomatics, we take the *geometry of the plane*; but we apologize that we will, for the sake of simplicity, be considering only the axioms of the geometry of position (the axioms that Hilbert lists as "axioms of connection" and "axioms of order" in his *Foundations of Geometry*) together with the parallel axiom.

Whereby it suggests itself for our purpose to diviate from Hilbert by *taking only points as individuals* and not points and lines as the two basic systems of things that underly the axiom system.

Then, the relation "points x and y determine the line g" is replaced by the relation between *three* points "x, y, z lie on one line," for which we make use of the notation $Gr(x, y, z)$.

In addition to this relation, there is a second basic relation, that of betweenness: "x lies between y and z", which we denote by $Zw(x,\ y,\ z)$.[3] Further, the concept of the identity of x and y is taken as one belonging to logic; for this we use the usual equality sign $x = y$.

[3]The method of taking only points as individuals is carried out in particular in the axiomatic system given by Oswald Veblen in the memoir "A system of axioms for geometry" [Trans. Amer. Math. Soc. Bd. 5 (1904) S. 343–384]. Furthermore, all the geometrical relations are there defined in terms of the relation "between".

For the symbolic presentation of the axioms, we now require in addition the logical signs. Firstly, there are the signs for generality and existence: if $P(x)$ is a predicate referring to the thing x, then $(x)P(x)$ means "All x have the property $P(x)$", and $(Ex)P(x)$ means "There is an x with the property $P(x)$." (x) is called the "for-all sign," and (Ex) the "there-is sign." The for-all and there-is signs can also refer to any other variable y, z, u in the same way that they refer to x. The variable belonging to such a sign becomes "bound" by this sign, just as a variable of integration is bound by the integral sign in such a way that the whole statement does not depend on the value of the variables.

In addition we only need the logical signs the symbolic presentation of the axioms, nan first the signs for generality and existence: $P(x)$ is a predicate referring to the object x, t $(x)P(x)$ means, "all x have the property $P($ and $(Ex)P(x)$ means "there is an x with property $P(x)$." (x) is named the "for-all-sig and (Ex) the "there-is-sign." The for-all-sign there-is-sign can refer to any other variable y, in the same way they can refer to x. The vari belonging to such a sign is "bound" by this sign the same way an integration variable is bound the integration sign, so that the whole statem does not depend on the value of the variables.

As further logical signs, we have signs for negation and for propositional connectives [*Satzverbindungen*]. We denote the negation of an assertion [*Aussage*] by a line over the assertion. overstriking. In the case where an assertion begins with a for-all or there-is sign, the negation stroke is to be set only above this sign, and instead of $\overline{x=y}$ the shorter '$x \neq y$' will be used. The sign & ("and") between two assertions means that both statements hold (conjunction). The sign $\vee$ ("or" in the sense of "vel") between two assertions means that at least one of the two assertions holds ("disjunction").

Signs for negation and the joining of sentences added as further logical signs. We designate negation of a statement by overstriking. In case of a preceding for-all-sign or there-is-sign negation stroke is to be set only above this s and instead of $\overline{x=y}$ the shorter $x \neq y$ should written. The sign & ("and") between two st ments means that both statements hold (conju tion). The sign $\vee$ ("or" in the sense of "vel") tween two statements means that at least on the two statements holds ("disjunction").

The sign $\rightarrow$ between two *[statements]* **assertions** means that the holding of the first *[entails]* **has as a consequence** the holding of the second, or *[with]* **in** other words, that the first *[statement]* **assertion** *[does not]* **cannot** hold *[,]* **Komma tilgen** without the second holding as well ("implication").

Accordingly, an implication $\mathfrak{A} \rightarrow \mathfrak{B}$ between two assertions $\mathfrak{A}$ and $\mathfrak{B}$ is only false if $\mathfrak{A}$ is true and $\mathfrak{B}$ is false; in all other cases it is true.

An implication $\mathfrak{A} \rightarrow \mathfrak{B}$ between two statement and $\mathfrak{B}$ is accordingly only then wrong, if $\mathfrak{A}$ is t and $\mathfrak{B}$ is false. In all other cases it is true.

The combination of the implication sign with the for-all sign results in the representation of general hypothetical statements. For example, the formula

$$(x)(y)\,(\mathfrak{A}(x,y) \rightarrow \mathfrak{B}(x,y))\ \ ,$$

|[5] where $\mathfrak{A}(x,y)$, $\mathfrak{B}(x,y)$ represent certain relations between x and y, represents the statement "If $\mathfrak{A}(x,y)$ holds, then $\mathfrak{B}(x,y)$," or also: "for every pair of individuals x, y for which $\mathfrak{A}(x,y)$ holds, then so does $\mathfrak{B}(x,y)$."[a]

[a]The relation between disjunction and implication as defined here and disjunctive and hypothetical assertions in the usual sense will be discussed in § 3.

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holds, $\mathfrak{B}(x,y)$ holds as well."[a]

[a]The relation between disjunction and i
fined here and disjunctive and hypothetica
statements in the usual sense will be discuss

We use brackets in the usual way for *[linking together]* **joining***[parts]* **components** of formulas. *[For saving]* **To minimize the use of** brackets *[KEIN KOMMA]* **Komma ist hinzuzufügen** we stipulate that for the separation of symbolic expressions $\rightarrow$ takes precedence over $\&$ and $\vee$, $\&$ over $\vee$, and that $\rightarrow$, $\&$, $\vee$ all have precedence over the *[for-all-sign and the there-is-sign]* **for-all sign and the there-is sign**.

Brackets are omitted if there is no danger of ambiguity. For example, instead of the expression

$$(x)\,((Ey)R(x,y))\,,$$

in which $R(x,y)$ designates an arbitrary relation between x and y, we write simply $(x)(Ey)R(x,y)$ because in this case only one reading comes into question: "For every x, there is a y for which the relation $R(x,y)$ holds."—

Brackets are omitted if no ambiguities
We write, for example, instead of the

$$(x)\,((Ey)R(x,y))\,,$$

in which $R(x,y)$ designates an arbit
between x and y, simply $(x)(Ey)R(x,$
this case only one reading is possible:
there is a y for which the relation $R(x,$

We are now in position to write down the axiom system *[considered]* **in question**.

To make it easier to digest, the first few axioms are accompanied by a written version.
The formulation of the axioms does not correspond completely to that in Hilbert's *Grundlagen der Geometrie*. Therefore, for each group of axioms, we specify the relationship of the axioms here formulated to those of Hilbert.[a]

[a]This is meant in particular for those familiar with Hilbert's *Grundlagen der Geometrie*. All references are to the seventh edition.

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[a]This information is especially meant for those fam
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I. Axioms of *[connection]* **Incidence**.

Für Dirk: Ich habe die Auflistungsmarkierung zu der von Bernays abgeändert, sodaß '1)' usw. statt '1.' usw. erzeugt wird.

1) $(x)(y)Gr(x,x,y)$.
"x,x,y always lie on one *[line]* **straight line**."

2) $(x)(y)(z)(Gr(x,y,z) \to Gr(y,x,z)\ \&\ Gr(x,z,y))$.
"If x,y,z lie on a *[line]* **straight line**, then *[so do]* **TILGEN** y,x,z *[as well as]* **respectively** x,z,y *[lie]* **also lie** on a *[line]* **straight line**."

3) $(x)(y)(z)(u)(Gr(x,y,z)\ \&\ Gr(x,y,u)\ \&\ x \neq y \to Gr(x,z,u))$.
"If x,y are different points and if x,y,z as well as x,y,u lie on a *[line]* **straight line,** then *[also]* TILGEN x,z,u *[lie]* **also lie** on a line."

4) $(Ex)(Ey)(Ez)\overline{Gr(x,y,z)}$.
"There are points x,y,z which do not lie on a *[line]* **straight line**."

Because of the different concept of straight line, 1) and 2) of these axioms replace the axioms in Hilbert's group I 1, 3) corresponds to the axiom I 2, and 4) corresponds to the second part of the axiom I 3.

Of these axioms, 1) and 2) replace the axion
1,—because of the changed concept of line; 3)
responds to the axiom I 2; and 4) correspond
the second part of I 3.

II. Axioms of *[order]* **Order**

Für Dirk: Nochmal Auflistungsänderung.

1) $(x)(y)(z)(Zw(x,y,z) \to Gr(x,y,z))$

2) $(x)(y)\overline{Zw(x,y,y)}$.

3) $(x)(y)(z)(Zw(x,y,z) \to Zw(x,z,y) \;\&\; \overline{Zw(y,x,z)})$.

4) $(x)(y)(x \neq y \to (Ez)Zw(x,y,z))$.
"If x and y are *[different]* **distinct** points, there is always a point z such that x lies between y and z."

5) $(x)(y)(z)(u)(v)\left(\overline{Gr(x,y,z)} \;\&\; Zw(u,x,y) \;\&\; \overline{Gr(v,x,y)} \;\&\; \overline{Gr(z,u,v)} \to (Ew)\{Gr(u,v,w) \;\&\; Zw(w,x,z) \vee Zw(w,y,z)\}\right)$.

1) and 2) together constitute the first part of Hilbert's axioms II 1; 3) unites the last part of Hilbert's *[axioms]* **axiom** II 1 with II 3; 4) is the axiom II 2; and 5) is the axiom of plane order *[KEIN KOMMA]* **Komma hinfügen** II 4.

III. Parallel *[axiom]* **Axiom**

Since we are leaving aside congruence axioms, we must take the Parallel Axiom in the more general form: "For every straight line and any point outside this line, there is exactly one straight line through this point which does not intersect the given line".[a]

[a]Cf. p. 83 of Hilbert's *Grundlagen der Geometrie*.

Since we are not including congrue we must take the parallel axiom in t broader sense: "For every straight lin actly one line through a point outside i not intersect it."[a]   [a]Cf. p. 83 of Hilbert's "Grundlagen der G

To make the symbolic formulation easier, we will use

$$Par(x,y;u,v)$$

as an abbreviation for the expression

To make symbolic formulation easier $Par(x,y;u,v)$ will be used as an abbreviation for th

$$\overline{(Ew)}(Gr(x,y,w) \;\&\; Gr(u,v,w))$$

"There is no point w which lies on a *[line]* **straight line** both with x and y and with u and v."

The axiom is then

$$(x)(y)(z)\left(\overline{Gr(x,y,z)} \to (Eu)\{Par(x,y;z,u) \;\&\; (v)(Par(x,y;z,v) \to Gr(z,u,v))\}\right).$$

If we

conjoin the axioms here enumerated

imagine the axioms here enumerated and u
them

, we get a single logical formula which represents an assertion about the predicates 'Gr', 'Zw' and which we designate as

$$\mathfrak{A}(Gr, Zw).$$

In the same way we could represent a theorem of plane geometry involving only position and order relations *[as]* **by** a formula

$$\mathfrak{S}(Gr, Zw).$$

This presentation still corresponds to the contentual axiomatic system, in which the basic relations are viewed as demonstrable either in experience or in |[7] intuitable representation, and with this are thus contentually definite. The statements of the theory can thus be seen as assertions about this content.

This representation still accords with conten
axiomatics in which the fundamental relations
viewed as something that can be shown in exp
ence or in the intuitive imagination and thus d
nite in content about which the statements of
theory make assertions.

In formal axiomatics on the other hand, the fundamental relations are not assumed to be determined in content from the beginning; rather, they obtain their determination at the outset *implicitly*, through the axioms. And in all considerations of an axiomatic theory, only what is expressly formulated in the axioms about the fundamental relations is used.

On the other hand, in formal axiomatics the fun
mental relations are not conceived from the be
ning as determined in content; rather they rec
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only what is expressly formulated in the axi
about the fundamental relations is used.

[As a result] **Consequently**, if in axiomatic geometry the respective names for relations in intuitive geometry like "lie on" or "between" are used, this is *[only]* **merely** a concession to custom and a means of *[simplifying]* **easing** the connection *[of]* **between** the theory *[with]* **and** intuitive facts. In *[fact]* **truth**, however,

the fundamental relations in formal axiomatics play the role of *variable* predicates.

in formal axiomatics the fundamental relati
play the role of *variable* predicates.

Here and in the sequel we understand "predicate" in the wider sense so that it also applies to predicates with two or more subjects.

Depending on the number of subjects, we speak of "one-place", "two-place",… predicates.

We speak of "one-place" place",… predicates according to of subjects.

In *[the]* **that** part of axiomatic geometry considered by *[us]* **us,** there are two variable three-place predicates:

$$R(x, y, z), \quad S(x, y, z) .$$

The axiom system consists of a demand on two such predicates expressed in the logical formula $\mathfrak{A}(R, S)$, *[that we get]* **obtained** from $\mathfrak{A}(Gr, Zw)$ when we replace $Gr(x, y, z)$ with $R(x, y, z)$, *[$Zw(x, y, z)$]* **and** $Zw(x, y, z)$ with $S(x, y, z)$.

Appearing alongside the variable predicates, there is also the identity relation $x = y$, and this is to be interpreted contentually. That we accept this predicate as determinate in content is not a violation of our methodological standpoint. The contentual determination of identity—which is not a relation at all in the true sense—is not dependent on the particular circle of ideas concerning the actual domain to be investigated axiomatically. Rather, it merely/solely concerns the separation of the individuals, and must be taken as given to us when the domain of individuals is laid down. *check this last para against the german.*

The identity relation $x = y$ which is preted contentually appears in this fo with the variable predicates. The a this predicate as contentually determi olation of our methodological standpo contentual determination of identity— relation at all in the true sense—does on the particular range of imagination being investigated axiomatically; rath related to a question of distinguishing which must be taken as already give domain of individuals is laid down.

Following this view, to a theorem of the form $\mathfrak{S}(Gr, Zw)$, there corresponds a determinate logical content, namely that for *any* predicates $R(x, y, z)$, $S(x, y, z)$ satisfying the requirements expressed by $\mathfrak{A}(R, S)$, the relation $\mathfrak{S}(R, S)$ also obtains, thus that for any two predicates $R(x, y, z)$, $S(x, y, z)$, the formula

$$\mathfrak{A}(R, S) \rightarrow \mathfrak{S}(R, S)$$

represents a true asssertion. In this way, a geometrical theorem is transformed into a theorem of pure predicate logic.|[8]

From this point of view a sentence $\mathfrak{S}(Gr, Zw)$ corresponds to the logica that for *any* predicates $R(x, y, z)$, S isfying the demand $\mathfrak{A}(R, S)$, the relat also holds; in other words, for any tw $R(x, y, z)$, $S(x, y, z)$ the formula

$$\mathfrak{A}(R, S) \rightarrow \mathfrak{S}(R, S)$$

represents a true statement. In this metrical sentence is transformed into pure predicate logic.|[8]

[From this point of view] **Correspondingly,** the problem of consistency presents itself *[in a corresponding way]* **TILGEN** as a problem of pure predicate logic. *[In fact]* **Indeed,** it is a question of whether two three-place predicates $R(x, y, z), S(x, y, z)$ can satisfy the conditions expressed *[in]* **through** the formula $\mathfrak{A}(R, S)$[4] or whether, on the contrary, the assumption that the formula $\mathfrak{A}(R, S)$ is satisfied for a certain pair of predicates leads to a *[contradiction]* **contradiction,** so that *[in general]* **generally** for every pair of predicates R, S the formula $\overline{\mathfrak{A}(R, S)}$ represents a correct assertion. HERE.

A question like the one given here is part of the "*decision problem.*" In newer logic this problem is understood to be that of discovering general methods for deciding the "validity" or "satisfiability" of logical formulas.[5]

In this connection the formulas investigated are composed with the help of logical signs out of predicate variables and equalities—together with variables in subject positions which we call "individual variables"—, and it is assumed that every variable is bound by a for-all sign or there-is sign.

A formula of this kind is called logically valid when it represents a true assertion for *every* determination of the variable predicates; it is called satisfiable when it represents a true assertion for some *appropriate* determination of the predicate variables.

Simple examples for logically valid formulas are the following:

$$(x)F(x) \;\&\; (x)G(x) \rightarrow (x)(F(x) \;\&\; G(x))$$
$$(x)P(x, x) \rightarrow (x)(Ey)P(x, y))$$
$$(x)(y)(z)(P(x, y) \;\&\; y = z \rightarrow P(x, z)).$$

Examples for satisfiable formulas are:

$$(Ex)F(x) \;\&\; (Ex)\overline{F(x)}$$
$$(x)(y)(P(x, y) \;\&\; P(y, x) \rightarrow x = y)$$
$$(x)(Ey)P(x, y) \;\&\; (Ey)(x)\overline{P(x, y)}\ .$$

These formulas result, e. g., in true assertions for the domain of individuals of the numbers 1, 2, if in the first formula for $F(x)$ "x is even" is set, in the second formula for $P(x, y)$ the predicate $x \leqq y$, and in the third formula for $P(x, y)$ the predicate $x \leqq y \;\&\; y \neq 1$.

[4]This imprecise way of *[putting]* **posing** the question will be *[sharpened]* **made more precise** in the sequel.

[5]This explanation is correct only for the decision problem in its narrower sense. We have no need here to consider the broader conception of this decision problem.

It is to be observed that along with the determination of the predicates the *domain of individuals* over which the variables $x, y, \ldots$ range has to be fixed. This enters into a logical formula as a kind of *hidden variable.* However, the logical formula in respect to satisfiability is invariant with respect to a one-one mapping of a domain of individuals onto another, since the individuals enter into the formulas only as variable subjects; as a result the only essential determination for a domain of individuals is the *number of individuals.*

Accordingly, we have to distinguish the following questions in relation to logical validity and satisfiability:

1. The question of logical validity for *every* domain of individuals, and also of satisfiability for *any* domain of individuals respectively.

2. The question of logical validity or satisfiability for a given number of individuals.

3. The question for which numbers of individuals is a formula logically valid or satisfiable.

It should be noted that it is best to leave out of consideration the domain of 0 individuals on principle, since formally zero-numbered domains of individuals have a special status, and on the other hand consideration of them is trivial and worthless for applications.[6]

Furthermore one should take into account that only the "value-range" of a predicate is relevant to its determination; that is to say, all that is relevant is for which values of the variables in subject positions the predicate holds or does not hold (is "true" or "false").

This circumstance has as a consequence that for a *given finite* number of individuals the logical validity or satisfiability of a specific given logical formula represents a pure *combinatorial fact* which one can determine through elementary testing of all cases.

[6] The stipulation that every domain of individuals should contain at least one thing, so that a true general judgement must hold of at least one thing, ought not to be confused with the convention prominent in Aristotelean logic that a judgment of the form "all S are P" counts as true only if there are in fact things with the property S. This convention has been dropped in newer logic. A judgment of this kind is represented symbolically in the form $(x)(S(x) \to P(x))$; it counts as true if a thing x, insofar as it has the property $S(x)$, always has the property $P(x)$ as well—independently of whether there is anything with the property Sx at all. We will take up this topic again in connection with the deductive construction of predicate logic. (See § 4 pp. 106–107.)

To be specific, if n is the number of individuals and k the number of subjects ("places") of a predicate, then n^k is the number of different systems of values for the variables; and since for every one of these systems of values the predicate is either true or false, there are

$$2^{(n^k)}$$

different possible value-ranges for a k-place predicate.

If then

$$R_1, \ldots, R_t$$

are the distinct predicate variables occurring in a given formula, with arities

$$k_1, \ldots, k_t$$

then

$$2^{(n^{k_1}+n^{k_2}+\ldots+n^{k_t})}$$

is the number of systems of value-ranges to be considered, or the number of different possible predicate systems for short.

Accordingly logical validity of the formula means that for all of these

$$2^{(n^{k_1}+n^{k_2}+\ldots+n^{k_t})}$$

explicitly enumerable predicate systems the formula represents a true assertion; and its satisfiability means that the formula represents a true assertion for one of these predicate systems. Moreover, for a fixed predicate system the truth or falsity of the assertion represented by the formula is again decidable by a finite testing of cases; the reason is that only n values come into consideration for a variable bound by a for-all sign or there-is sign so that 'all' has the same meaning as a conjunction with n members and 'there is' a disjunction with n members.

For example, consider the formulas mentioned above

$$(x)P(x,x) \rightarrow (x)(Ey)P(x,y)$$

$$(x)(y)(P(x,y) \ \& \ P(y,x) \rightarrow x = y)$$

of which the first has been referred to as a logically valid, the second as a satisfiable, formula. We refer these formulas to a domain of two individuals.

We can indicate both individuals with the numerals 1, 2. In this case we have $t = 1, n = 2, k_1 = 2$; therefore the number of different predicate systems is

$$2^{(2^2)} = 2^4 = 16.$$

In place of $(x)P(x, x)$ we can put

$$P(1, 1) \ \&\ P(2, 2)$$

in place of $(x)(Ey)P(x, y)$

$$P(1, 1) \vee P(1, 2) \ \&\ P(2, 1) \vee P(2, 2)\ ,$$

so that the first of the two formulas becomes

$$P(1, 1) \ \&\ P(2, 2) \to P(1, 1) \vee P(1, 2) \ \&\ P(2, 1) \vee P(2, 2)\ .$$

This implication is true for those predicates P for which $P(1, 1) \ \&\ P(2, 2)$ is false, as well as for those for which

$$P(1, 1) \vee P(1, 2) \ \&\ P(2, 1) \vee P(2, 2)$$

is true. One can now verify that for each of the 16 value-ranges that one gets when one assigns one of the truth values "true" or "false" to each of the pairs of values

$$(1, 1),\ (1, 2),\ (2, 1),\ (2, 2)$$

one of the two conditions is satisfied; thus the whole expression always receives the value "true." [Verification is simplified in this example because already the determination of the values of $P(1, 1)$ and $P(2, 2)$ suffices to fix the correctness of the expression.] In this way the validity of our first formula for domains of two individuals can be determined through directly trying it out.

For domains of two individuals the second formula has the same meaning as the conjunction

$$\begin{aligned} &(P(1, 1) \ \&\ P(1, 1) \to 1 = 1) \quad \& \quad (P(2, 2) \ \&\ P(2, 2) \to 2 = 2) \\ \&\ &(P(1, 2) \ \&\ P(2, 1) \to 1 = 2) \quad \& \quad (P(2, 1) \ \&\ P(1, 2) \to 2 = 1)\ . \end{aligned}$$

Since $1 = 1$ and $2 = 2$ are true the first two members of the conjunction are always true assertions. The last two members are true if, and only if,

$$P(1,2) \mathbin{\&} P(2,1)$$

is false.

Therefore, to satisfy the formula under consideration one has only to eliminate those determinations of value for P in which the pairs $(1,2)$ and $(2,1)$ are both assigned the value "true." Every other determination of value produces a true assertion. The formula is therefore satisfiable in a domain of two elements.

These examples should make clear the purely combinatorial character of the decision problem in the case of a given finite number of individuals. One result of this combinatorial character is that for a prescribed finite number of individuals the logical validity of a formula $\mathfrak{F}$ has the same meaning as the unsatisfiability of the formula $\overline{\mathfrak{F}}$; likewise the satisfiability of a formula $\mathfrak{F}$ has the same meaning as that $\overline{\mathfrak{F}}$ is not valid. Indeed $\mathfrak{F}$ represents a true assertion for those predicate systems for which $\overline{\mathfrak{F}}$ represents a false assertion and vice-versa.

Let us return to the question of the consistency of an axiom system. Let us consider an axiom system written down symbolically and combined into one formula like our example.

The question of the satisfiability of this formula for a prescribed finite number of individuals can be decided, in principle at least, through trying it out. Suppose then the satisfiability of the formula is determined for a definite finite number of individuals. The result is a proof of the consistency of the axiom system, namely a proof by the *method of exhibition*, since the finite domain of individuals together with the value-ranges chosen for the predicates (to satisfy the formula) constitutes a model in which we can show concretely that the axioms are satisfied.

We give an example of such an exhibition from axiomatics in geometry. We start from from the axiom system presented in the beginning, but replace the axiom I 4), which postulates the existence of three points not lying on a line, with the weaker axiom

I 4′) $(Ex)(Ey)(x \neq y)$.

"There are two distinct points."

Furthermore we drop the axiom of plane order II 5); in its place we add to the axioms[7] two sentences which can be proved using II 5) by, firstly, expanding II 4) to

II 4′) $(x)(y)\{x \neq y \rightarrow (Ez)Zw(z,x,y) \ \& \ (Ez)Zw(x,y,z)\}$,

and, secondly, adding

II 5) $(x)(y)(z)\{x \neq y \ \& \ x \neq z \ \& \ y \neq z \rightarrow Zw(x,y,z) \vee Zw(y,z,x) \vee Zw(z,x,y)\}$.

We keep the parallel axiom. The resulting axiom system corresponds to a formula $\mathfrak{A}'(R,S)$ instead of the earlier $\mathfrak{A}(R,S)$; it is satisfiable in a domain of individuals of 5 things, as O. Veblen remarked.[8] The value-ranges for the predicates R, S are so chosen that first of all the predicate Gr is determined to be true for every value triple x, y, z—we can here use the symbols 'Gr', 'Zw' with no danger of misunderstanding. One sees immediately that then all axioms I as well as II 1) and III are satisfied. In order that the axioms II 2), 3), 5′), and 4′) be satisfied it is necessary and also sufficient that the following three conditions be placed on the predicate Zw:

1. Zw is always false for a triple x, y, z in which two elements coincide.

2. For any combination of three different of the 5 individuals, Zw is true for 2 orderings with a common first element (of 6 possible orderings of the elements), false for the remaining 4 orderings.

3. Each pair of different elements occurs as an initial as well as a final pair in one of the triples for which Zw is true.

The first demand can be directly fulfilled by stipulation. The joint satisfaction of the other two conditions is accomplished as follows: We designate the 5 elements with the numerals 1, 2, 3, 4, 5. The number of value-triples of three distinct elements for which Zw still has to be defined is $5 \cdot 4 \cdot 3 = 60$. Every six of these belong to a combination; for two of these Zw should be true and false for the rest. We must therefore indicate those 20 of the 60

[7]Both of these sentences were introduced as axioms in earlier editions of Hilbert's "Grundlagen der Geometrie." It turned out that they are provable using the axioms of plane order. See pp. 5–6 of the seventh edition.

[8]In the investigation already mentioned "A system of axioms for geometry," Trans. Amer. Math. Soc. vol. 5, p. 350.

triples for which Zw will be defined as true. They are those which one obtains from the four triples

$$(1\ 2\ 5),\ (1\ 5\ 2),\ (1\ 3\ 4),\ (1\ 4\ 3)$$

by applying the cyclical permutation (1 2 3 4 5).

It is easy to verify that this procedure satisfies all the conditions. Thus the axiom system is recognized as consistent by the method of exhibition.[9]

The method of exhibition presented in this example has very many different applications in newer axiomatic investigations. It is especially used for *proofs of independence*. The assertion that a sentence $\mathfrak{S}$ is independent of an axiom system $\mathfrak{A}$ has the same meaning as the assertion of the consistency of the axiom system as the claim that the axiom system

$$\mathfrak{A}\ \&\ \overline{\mathfrak{S}}$$

which we get when we add the negation of the sentence $\mathfrak{S}$ as an axiom to $\mathfrak{A}$. The consistency can be determined by the method of exihibition if this axiom system is satisfiable in a finite domain domain of individuals.[10] Thus this method provides a sufficient extension of the method of progressive inferences for many fundamental investigations in the sense that the unprovability of a sentence from certain axioms can be proved through exhibition, its provability through inference.

But is the application of the method of exhibition restricted in its application to finite domains of individuals? We cannot derive this from what we have said up until now. However, we do see immediately that in the case of an infinite domain of individuals the possible systems of predicates no longer constitute a surveyable multitude and there can be no talk of testing all value-ranges. Nevertheless in the case of given axioms we might be in a position to show their satisfiability by given predicates. And this is actually

[9] It follows immediately from the fact that the modified axiom system $\mathfrak{A}'$ is satisfiable in a domain of 5 individuals that the axioms of this system do not completely determine linear ordering.

[10] A great number of examples of this procedure can be found in the works on linear and cyclical order by E. V. Huntington and his collaborators. See especially "A new set of postulates for betweenness with proof of complete independence", Trans. Amer. Math. Soc. vol. 26 (1924) pp. 257–282. Here one also finds references to previous works.

the case. Consider for example the system of three axioms

$$
\begin{gathered}
(x)\overline{R(x,x)}\ , \\
(x)(y)(z)(R(x,y)\ \&\ R(y,z) \rightarrow R(x,z) \\
(x)(Ey)R(x,y).
\end{gathered}
$$

Let us clarify what these say: We start with an object a in the domain of individuals. According to the third axiom there must be a thing b for which $R(a,b)$ is true; and because of the first axiom, b must be different from a. For b there must further be a thing c for which $R(b,c)$ is true, and because of the second axiom $R(a,c)$ is also true; according to the third axiom c is distinct from a and b. For c there must again be a thing d for which $R(c,d)$ is true. For this thing $R(a,d)$ and $R(b,d)$ are also true, and d is distinct from $a,\ b,\ c$. The method of this consideration here has no end; and it shows us we cannot satisfy the axioms with a finite domain of individuals. On the other hand we can easily show satisfaction by an infinite domain of individuals: We take the integers as individuals and substitute the relation "x is less than y" for $R(x,y)$; one sees immediately that all three axioms are satisfied.

It is the same with the axioms

$$
\begin{gathered}
(Ex)(y)\overline{S(y,x)}\ , \\
(x)(y)(u)(v)(S(x,u)\ \&\ S(y,u)\ \&\ S(v,x) \rightarrow S(v,y))\ , \\
(x)(Ey)S(x,y)\ .
\end{gathered}
$$

One can easily ascertain that these cannot be satisfied with a finite domain of individuals. On the other hand they are satisfied in the domain of positive integers if we replace $S(x,y)$ with the relation "y immediately follows x."

However, we notice in these examples that exhibiting in these cases does by no means conclusively settle the question of consistency; rather the question is *reduced* to that of the *consistency of number theory*. In the earlier example of finite exhibition we took integers as individuals. There, however, this was only for the purpose of having a simple way to designate individuals. Instead of numbers we could have taken other things, letters for example. And also the properties of numbers which were used could have been established by a concrete exhibition.

In the case now before us, however, a concrete idea of number is not enough; for we essentially need the assumption that the *integers* constitute *a domain of individuals* and therefore a ready totality.

We are, of course, quite familiar with this assumption since in newer mathematics we are constantly working with it; one is inclined to consider it perfectly natural. It was Frege who vigorously and with a sharp and witty critique first established that the idea of the sequence of integers as a ready totality must be justified by a proof of consistency.[11] According to Frege, such a proof had to be carried out in the sense of an exhibition, as an existence proof; and he believed he could find the objects for such an exhibition in the domain of logic. His method of exhibition amounts to defining the totality of integers with the help of the totality (presupposed to exist) of all conceivable one-place predicates. However, the underlying assumption, which under impartial consideration seems very suspect anyway, was shown to be untenable by the famous logical and set-theoretic paradoxes discovered by Russell and Zermelo. And the failure of Frege's undertaking has made us even more conscious of the problematic character of assuming the totality of the number sequence than did his dialectic.

In the light of this difficulty we might try to use some other infinite domain of individuals instead of the sequence of integers for the purpose of proving consistency, a domain taken from the realm of sense perception or physical reality rather than being a pure product of thought like the sequence of integers. However, if we look more closely we will realize that wherever we think we encounter infinite manifolds in the realm of sensible qualities or in physical reality there can be no talk of the actual presence of such a manifold; rather the conviction that such a manifold is present rests on a mental extrapolation, the justification of which is as much in need of investigation as the conception of the totality of the sequence of integers.

A typical example in this connection is those cases of the infinite which gave rise to the well-known paradox of Zeno. Suppose some distance is traversed in a finite time; the traversal includes infinitely many successive subprocesses: the traversal of the first half, then of the next quarter, then the next eighth, and so on. If we are considering an actual motion, then these subtraversals must be real processes succeeding one another.

People have tried to refute this paradox with the argument that the sum of infinitely many time intervals may converge producing a finite duration. However, this reply does not come to grips with an essential point of the paradox, namely the paradoxical aspect that lies in the fact that an infinite succession, the completion of which we could not accomplish in the imagination either actually or in principle, should be accomplished in reality.

[11]Gottlob Frege, "Grundlagen der Arithmetik", Breslau 1884, and "Grundgesetze der Arithmetik", Jena 1893.

Actually there is a much more radical solution of the paradox. It consists in considering that we are by no means forced to believe that the mathematical space-time representation of movement remains physically meaningful for arbitrarily small segments of space and time; rather there is every reason to assume that a mathematical model extrapolates the facts of a certain domain of experience, e.g., just the movements, within the range of magnitudes accessible to our observation up to now for the purpose of a simple conceptual structure; this is similar to continuum mechanics which carries out an extrapolation in taking as a basis the idea of space as filled with matter; it is no more the case that unbounded division of a movement always produces something characterizable as movement than that unbounded spatial division of water always produces quantities of water. When this is accepted the paradox vanishes.

Notwithstanding, the mathematical model of movement has, as an *idealizing concept* formation, its value for the purpose of simplified representation. For this purpose it must not only coincide approximately with reality, but it has to meet the condition that the extrapolation it involves must be consistent in itself. From this point of view the mathematical conception of movement is not in the least shaken by Zeno's paradox; the mathematical counterargument just referred to has in this case complete validity. It is another question however, whether we possess a real proof of the consistency of the mathematical theory of motion. This theory depends essentially on the mathematical theory of the continuum; this in turn depend essentially on the idea of the set of all integers as a ready totality. We therefore come back by a roundabout way to the problem we tried to avoid by referring to the facts about motion.

It is much the same in every case in which a person thinks he can show directly that some infinity is given in experience or intuition, for example the infinity of the tone row extending from octave to octave to infinity, or the continuous infinite manifold involved in the passage from one color quality to another. Closer consideration shows in every case that in fact no infinity is given at all; rather it is interpolated or extrapolated through some mental process.

These considerations make us realize that reference to non-mathematical objects can not settle the question whether an infinite manifold exists; the question must be solved within mathematics itself. But how should one make a start with such a solution? At first glance it seems that something impossible is being demanded here: to present infinitely many individuals is impossible in principle; therefore an infinite domain of individuals as such can only be indicated through its structure, i. e., through relations holding among its elements. In other words: a proof must be given that for this domain certain formal relations can be satisfied. The existence of an infinite domain of individuals can *not be represented in any other way than through the satisfiability of certain logical formulas*; but these are exactly the kind of formulas we were led to through investigating the question of the existence of an infinite domain of individuals; and the satisfiability of these formulas was to have been demonstrated by the exhibition of an infinite domain of individuals. The attempt to apply the method of exhibition to the formulas under consideration leads then to a vicious circle.

But exhibition should serve only as a means in proofs of the consistency of axiom systems. We were led to this procedure through considering domains with a given finite number of individuals, and just through recognizing that in such domains the consistency of a formula has the same significance as its satisfiability.

The situation is more complicated in the case of infinite domains of individuals. It is true in this case also that an axiom system represented by a formula $\mathfrak{A}$ is inconsistent if, and only if, the formula $\overline{\mathfrak{A}}$ is logically valid. But since we are no longer dealing with a surveyable supply of value-ranges for the variable predicates, we can no longer conclude that if $\overline{\mathfrak{A}}$ is not logically valid, there is some model for satisfying the axiom system $\mathfrak{A}$ at our disposal.

Accordingly, when an infinite domain of individuals is under consideration, the satisfiability of an axiom system is a sufficient condition for its consistency, but it is not proved to be a necessary condition. We cannot therefore expect that in general a proof of consistency can be accomplished by means of a proof of satisfiability. On the other hand we are not forced to prove consistency by establishing satisfiability; we can just hold to the original negative sense of inconsistency. That is to say—if we again imagine an axiom system represented by a formula $\mathfrak{A}$—we do not have to show that satisfiability of the formula $\mathfrak{A}$, but only need to prove that the assumption that $\mathfrak{A}$ is satisfied by certain predicates cannot lead to a logical contradiction.

To attack the problem in these terms we must first aim at an overview of the possible logical inferences that can be made from an axiom system. The *formalization of logical inference* as developed by Frege, Schröder, Peano, and Russell presents itself as an appropriate means to this end.

We have thus arrived at the following tasks: 1. to formalize rigorously the principles of logical inference and by this turn them into a completely surveyable system of rules; 2. to show for a given axiom system $\mathfrak{A}$ (which is to be proved consistent), that starting with this *system* $\mathfrak{A}$ *no contradiction can arise via logical deductions*, that is to say, no two formulas of which one is the negation of the other can be proved.

However, we do not have to carry out this proof for each axiom system individually; for we can make use of the method of *arithmetizing* to which we referred at the beginning. From the point of view we have reached now this procedure can be characterized as follows: we chose

Chapter 11

Bernays Project: Text No. 13

Platonism in mathematics (1935)

Sur le platonisme dans les mathématiques

(*L'enseignement mathematique* 24, pp. 52–69;
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With your permission, I shall now address you on the subject of the present situation in research in the foundations of mathematics. Since there remain open questions in this field, I am not in a position to paint a definitive picture of it for you. But it must be pointed out that the situation is not so critical as one could think from listening to those who speak of a foundational crisis. From certain points of view, this expression can be justified; but it could give rise to the opinion that mathematical science is shaken at its roots.

The truth is that the mathematical sciences are growing in complete security and harmony. The ideas of Dedekind, Poincaré, and Hilbert have been systematically developed with great success, without any conflict in the results.

It is only from the philosophical point of view that objections have been raised. They bear on certain ways of reasoning peculiar to analysis and set theory. These modes of reasoning were first systematically applied in giving a rigorous form to the methods of the calculus. [**According to them,**] the objects of a theory are viewed as elements of a totality such that one can reason as follows: For each property expressible using the notions of the theory, it is [an] objectively determinate [fact] whether there is or there is not an element of the totality which possesses this property. Similarly, it follows from this point of view that either all the elements of a set possess a given property, or there is at least one element which does not possess it.

An example of this way of setting up a theory can be found in Hilbert's axiomatization of geometry. If we compare Hilbert's axiom system to Euclid's, ignoring the fact that the Greek geometer fails to include certain [**necessary**] postulates, we notice that Euclid speaks of figures to be [***constructed***] whereas, for Hilbert, system of points, straight lines, and planes exist from the outset. Euclid postulates: One can join two points by a straight line; Hilbert states the axiom: Given any two points, there exists a straight line on which both are situated. "Exists" refers here to existence in the system of straight lines.

This example shows already that the tendency of which we are speaking consists in viewing the objects as cut off from all links with the reflecting subject.

Since this tendency asserted itself especially in the philosophy of Plato, allow me to call it "platonism."

The value of platonistically inspired mathematical conceptions is that they furnish models of abstract imagination. These stand out by their simplicity and logical strength. They form representations which extrapolate from certain regions of experience and intuition.

Nonetheless, we know that we can arithmetize the theoretical systems of geometry and physics. For this reason, we shall direct our attention to platonism in arithmetic. But I am referring to arithmetic in a very broad sense, which includes analysis and set theory.

The weakest of the "platonistic" assumptions introduced by arithmetic is that of the totality of integers. The *tertium non datum* for integers follows from it; viz.: if P is a predicate of integers, either P is true of each number, or there is at least one exception.

By the assumption mentioned, this disjunction is an immediate consequence of the logical principle of the excluded middle; in analysis it is almost continually applied.

For example, it is by means of it that one concludes that for two real numbers a and b, given by convergent series, either $a = b$ or $a < b$ or $b < a$; and likewise: a sequence of positive rational numbers either comes as close as you please to zero or there is a positive rational number less than all the members of the sequence.

At first sight, such disjunctions seem trivial, and we must be attentive in order to notice that an assumption slips in. But analysis is not content with this modest variety of platonism; it reflects it to a stronger degree with respect to the following notions: set of numbers, sequence of numbers, and function. It abstracts from the possibility of giving definitions of sets, sequences, and functions. These notions are used in a "quasi-combinatorial" sense, by which I mean: in the sense of an analogy of the infinite to the finite.

Consider, for example, the different functions which assign to each member of the finite series $1, 2, \ldots, n$ a number of the same series. There are n^n functions of this sort, and each of them is obtained by n independent determinations. Passing to the infinite case, we imagine functions engendered by an infinity of independent determinations which assign to each integer an integer, and we reason about the totality of these functions.

In the same way, one views a set of integers as the result of infinitely many independent acts deciding for each number whether it should be included or excluded. We add to this the idea of the totality of these sets. Sequences of real numbers and sets of real numbers are envisaged in an analogous manner. From this point of view, constructive definitions of specific functions, sequences, and sets arc only ways to pick out an object which exists independently of, and prior to, the construction.

The axiom of choice is an immediate application of the quasi-combinatorial concepts in question. It is generally employed in the theory of real numbers in the following special form. Let

$$M_1, M_2 \ldots$$

be a sequence of non-empty sets of real numbers, then there is a sequence

$$a_1, a_2 \ldots$$

such that for every index n, a_n is an element of M_n.

The principle becomes subject to objections if the effective construction of the sequence of numbers is demanded.

A similar case is that of Poincaré's impredicative definitions. An impredicative definition of a real number appeals to the hypothesis that all real numbers have a certain property P, or the hypothesis that there exists a real number with the property T.

This kind of definition depends on the assumption of [**the existence of**] the totality of sequences of integers, because a real number is represented by a decimal fraction, that is to say, by a special kind of sequence of integers.

It is used in particular to prove the fundamental theorem that a bounded set of real numbers always has a least upper bound.

In Cantor's theories, platonistic conceptions extend far beyond those of the theory of real numbers. This is done by iterating the use of the quasi-combinatorial concept of a function and adding methods of collection. This is the well-known method of set theory.

The platonistic conceptions of analysis and set theory have also been applied in modern theories of algebra and topology, where they have proved very fertile.

This brief summary will suffice to characterize platonism and its application to mathematics. This application is so widespread that it is not an exaggeration to say that platonism reigns today in mathematics.

But on the other hand, we see that this tendency has been criticized in principle since its first appearance and has given rise to many discussions. This criticism was reinforced by the paradoxes discovered in set theory, even though these antinomies refute only extreme platonism.

We have set forth only a restricted platonism which does not claim to be more than, so to speak, an ideal projection of a domain of thought. But the matter has not rested there. Several mathematicians and philosophers interpret the methods of platonism in the sense of conceptual realism, postulating the existence of a world of ideal objects containing all the objects and relations of mathematics. It is this absolute platonism which has been shown untenable by the antinomies, particularly by those surrounding the Russell-Zermelo paradox.

If one hears them for the first time, these paradoxes in their purely logical form can seem to be plays on words without serious significance. Nonetheless one must consider that these abbreviated forms of the paradoxes are obtained by following out the consequences of the various requirements of absolute platonism.

The essential importance of these antinomies is to bring out the impossibility of combining the following two things: the idea of the totality of all mathematical objects and the general concepts of set and function; for the totality itself would form a domain of elements for sets, and arguments and values for functions.

We must therefore give up absolute platonism. But it must be observed that this is almost the only injunction which follows from the paradoxes. Some will think that this is regrettable, since the paradoxes are appealed to on every side. But avoiding the paradoxes does not constitute a univocal program. In particular, restricted platonism is not touched at all by the antinomies.

Still, the critique of the foundations of analysis receives new impetus from this source, and among the different possible ways of escaping from the paradoxes, eliminating platonism offered itself as the most radical.

Let us look and see how this elimination can be brought about. It is done in two steps, corresponding to the two essential assumptions introduced by platonism. The first step is to replace by constructive concepts the concepts of a set, a sequence, or a function, which I have called quasi-combinatorial. The idea of an infinity of independent determinations is rejected. One emphasizes that an infinite sequence or a decimal fraction can be given only by an arithmetical law, and one regards the continuum as a set of elements defined by such laws.

This procedure is adapted to the tendency toward a complete arithmetization of analysis. Indeed, it must be conceded that the arithmetization of analysis is not carried through to the end by the usual method. The conceptions which are applied there are not completely reducible, as we have seen, to the notion of integer and logical concepts.

Nonetheless, if we pursue the thought that each real number is defined by an arithmetical law, the idea of the totality of real numbers is no longer indispensable, and the axiom of choice is not at all evident. Also, unless we introduce auxiliary assumptions—as Russell and Whitehead do—we must do without various usual conclusions. Weyl has made these consequences very clear in his book *The Continuum* (*vide* [**?**]).

Let us proceed to the second step of the elimination. It consists in renouncing the idea of the totality of integers. This point of view was first defended by Kronecker and then developed systematically by Brouwer.

Although several of you heard in March [**1934**] an authentic exposition of this method by Professor Brouwer himself, I shall allow myself a few words of explanation.

A misunderstanding about Kronecker must first be dissipated, which could arise from his often-cited aphorism that the integers were created by God, whereas everything else in mathematics is the work of man. If that were really Kronecker's opinion, he ought to admit the concept of the totality of integers.

In fact, Kronecker's method, as well as that of Brouwer, is characterized by the fact that it avoids the supposition that there exists a series of natural numbers forming a determinate ideal object.

According to Kronecker and Brouwer, one can speak of the series of numbers only in the sense of a process that is never finished, surpassing each limit which it reaches.

This point of departure carries with it the other divergences, in particular those concerning the application and interpretation of logical forms: Neither a general judgment about integers nor a judgment of existence can be interpreted as expressing a property of the series of numbers. A general theorem about numbers is to be regarded as a sort of prediction that a property will present itself for each construction of a number; and the affirmation of the existence of a number with a certain property is interpreted as an incomplete communication of a more precise proposition indicating a [**particular**] number having the property in question or a method for obtaining such a number; Hilbert calls it a "partial judgment."

For the same reasons the negation of a general or existential proposition about integers does not have precise sense. One must strengthen the negation to arrive at a mathematical proposition. For example, it is to give a strengthened negation of a proposition affirming the existence of a number with a property P to say that a number with the property P cannot be given, or further, that the assumption of a number with this property leads to a contradiction. But for such strengthened negations the law of the excluded middle is no longer applicable.

The characteristic complications to be met with in Brouwer's "intuitionistic" method come from this.

For example, one may not generally make use of disjunctions like these: a series of positive terms is either convergent or divergent; two convergent sums represent either the same real number or different ones.

In the theory of integers and of algebraic numbers, we can avoid these difficulties and manage to preserve all the essential theorems and arguments.

In fact, Kronecker has already shown that the core of the theory of algebraic fields can be developed from his methodological point of view without appeal to the totality of integers.[1]

As for analysis, you know that Brouwer has developed it in accord with the requirements of intuitionism. But here one must abandon a number of the usual theorems, for example, the fundamental theorem that every continuous function has a maximum in a closed interval. Very few things in set theory remain valid in intuitionist mathematics.

We would say, roughly, that intuitionism is adapted to the theory of numbers; the semiplatonistic method, which makes use of the idea of the totality of integers but avoids quasi-combinatorial concepts, is adapted to the arithmetic theory of functions, and the usual platonism is adequate for the geometric theory of the continuum.

There is nothing astonishing about this situation, for it is a familiar procedure of the contemporary mathematician to restrict his assumptions in each domain of the science to those which are essential. By this restriction, a theory gains methodological clarity, and it is in this direction that intuitionism proves fruitful.

But as you know, intuitionism is not at all content with such a role; it opposes the usual mathematics and claims to represent the only true mathematics.

On the other hand, mathematicians generally are not at all ready to exchange the well-tested and elegant methods of analysis for more complicated methods unless there is an overriding necessity for it.

We must discuss the question more deeply. Let us try to portray more distinctly the assumptions and philosophic character of the intuitionistic method.

What Brouwer appeals to is evidence. He claims that the basic ideas of intuitionism are given to us in an evident manner by pure intuition. In relying on this, he reveals his partial agreement with Kant. But whereas for Kant there exists a pure intuition with respect to space and time, Brouwer acknowledges only the intuition of time, from which, like Kant, he derives

[1]To this end, Kronecker set forth in his lectures a manner of introducing the notion of algebraic number which has been almost totally forgotten, although it is the most elementary way of defining this notion. This method consists in representing algebraic numbers by the changes of sign of irreducible polynomials in one variable with rational integers as coefficients; starting from that definition, one introduces the elementary operations and relations of magnitude for algebraic numbers and proves that the ordinary laws of calculation hold; finally one shows that a polynomial with algebraic coefficients having values with different signs for two algebraic arguments a and b has a zero between a and b.

the intuition of number.

As for this philosophic position, it seems to me that one must concede to Brouwer two essential points: first, that the concept of integer is of intuitive origin. In this respect nothing is changed by the investigations of the logicists, to which I shall return later. Second, one ought not to make arithmetic and geometry correspond in the manner in which Kant did. The concept of number is more elementary than the concepts of geometry.

Still it seems a bit hasty to deny completely the existence of a geometrical intuition. But let us leave that question aside here; there are other, more urgent ones. Is it really certain that the evidence given by arithmetical intuition extends exactly as far as the boundaries of intuitionist arithmetic would require? And finally: Is it possible to draw an exact boundary between what is evident and what is only plausible?

I believe that one must answer these two questions negatively. To begin with, you know that men and even scholars do not agree about evidence in general. Also, the same man sometimes rejects suppositions which he previously regarded as evident.

An example of a much-discussed question of evidence, about which there has been controversy up to the present, is that of the axiom of parallels. I think that the criticism which has been directed against that axiom is partly explained by the special place which it has in Euclid's system. Various other axioms had been omitted, so that the parallels axiom stood out from the others by its complexity.

In this matter I shall be content to point out the following: One can have doubts concerning the evidence of geometry, holding that it extends only to topological facts or to the facts expressed by the projective axioms. One can, on the other hand, claim that geometric intuition is not exact. These opinions are self-consistent, and all have arguments in their favor. But to claim that metric geometry has an evidence restricted to the laws common to Euclidean and Bolyai-Lobachevskian geometry, an exact metrical evidence which yet would not guarantee the existence of a perfect square, seems to me rather artificial. And yet it was the point of view of a number of mathematicians.

Our concern here has been to underline the difficulties to be encountered in trying to describe the limits of evidence.

Nevertheless, these difficulties do not make it impossible that there should be anything evident beyond question, and certainly intuitionism offers some such. But does it confine itself completely within the region of this elementary evidence? This is not completely indubitable, for the following reason: Intuitionism makes no allowance for the possibility that, for very large numbers, the operations required by the recursive method of constructing numbers can cease to have a concrete meaning. From two integers k, l one passes immediately to k^l; this process leads in a few steps to numbers which are far larger than any occurring in experience, e. g., $67^{(257^{729})}$.

Intuitionism, like ordinary mathematics, claims that this number can be represented by an Arabic numeral. Could not one press further the criticism which intuitionism makes of existential assertions and raise the question: What does it mean to claim the existence of an Arabic numeral for the foregoing number, since in practice we are not in a position to obtain it?

Brouwer appeals to intuition, but one can doubt that the evidence for it really is intuitive. Isn't this rather an application of the general method of analogy, consisting in extending to inaccessible numbers the relations which we can concretely verify for accessible numbers? As a matter of fact, the reason for applying this analogy is strengthened by the fact that there is no precise boundary between the numbers which are accessible and those which are not. One could introduce the notion of a "practicable" procedure, and implicitly restrict the import of recursive definitions to practicable operations. To avoid contradictions, it would suffice to abstain from applying the principle of the excluded middle to the notion of practicability. But such abstention goes without saying for intuitionism.

I hope I shall not be misunderstood: I am far from recommending that arithmetic be done with this restriction. I am concerned only to show that intuitionism takes as its basis propositions which one can doubt and in principle do without, although the resulting theory would be rather meager.

It is therefore not absolutely indubitable that the domain of complete evidence extends to all of intuitionism. On the other hand, several mathematicians recognize the complete evidence of intuitionistic arithmetic and moreover maintain that the concept of the series of numbers is evident in the following sense: The affirmation of the existence of a number does not require that one must, directly or recursively, give a bound for this number. Besides, we have just seen how far beyond a really concrete presentation such a limitation would be.

In short, the point of view of intuitive evidence does not decide uniquely in favor of intuitionism.

In addition, one must observe that the evidence which intuitionism uses in its arguments is not always of an immediate character. Abstract reflections are also included. In fact, intuitionists often use statements, containing a general hypothesis, of the form “if every number n has the property $A(n)$, then B holds.”

Such a statement is interpreted intuitionistically in the following manner: “If it is proved that every number n possesses the property $A(n)$, then B.” Here we have a hypothesis of an abstract kind, because since the methods of demonstration are not fixed in intuitionism, the condition that something is proved is not intuitively determined.

It is true that one can also interpret the given statement by viewing it as a partial judgment, i. e., as the claim that there exists a proof of B from the given hypothesis, a proof which would be effectively given. (This is approximately the sense of Kolmogorov’s interpretation of intuitionism.) In any case, the argument must start from the general hypothesis, which cannot be intuitively fixed. It is therefore an abstract reflection.

In the example just considered, the abstract part is rather limited. The abstract character becomes more pronounced if one superposes hypotheses; i. e., when one formulates propositions like the following: “$_{c_1}\mathbf{I}_{c_1}$f from the hypothesis that $A(n)$ is valid for every n, one can infer B, then C holds,” or “If from the hypothesis that A leads to a contradiction, a contradiction follows, then B,” or briefly “If the absurdity of A is absurd, then B.” This abstractness of statements can be still further increased.

It is by the systematic application of these forms of abstract reasoning that Brouwer has gone beyond Kronecker’s methods and succeeded in establishing a general intuitionistic logic, which has been systematized by Heyting.

If we consider this intuitionistic logic, in which the notions of consequence are applied without reservation, and we compare the method used here with the usual one, we notice that the characteristic general feature of intuitionism is not that of being founded on pure intuition, but rather [**that of being founded**] on the relation of the reflecting and acting subject to the whole development of science.

This is an extreme methodological position. It is contrary to the customary manner of doing mathematics, which consists in establishing theories detached as much as possible from the thinking subject.

This realization leads us to doubt that intuitionism is the sole legitimate method of mathematical reasoning. For even if we admit that the tendency away from the [**thinking**] subject has been pressed too far under the reign of platonism, this does not lead us to believe that the truth lies in the opposite extreme. Keeping both possibilities in mind, we shall rather aim to bring about in each branch of science, an adaptation of method to the character of the object investigated.

For example, for number theory the use of the intuitive concept of a number is the most natural. In fact, one can thus establish the theory of numbers without introducing an axiom, such as that of complete induction, or axioms of infinity like those of Dedekind and Russell.

Moreover, in order to avoid the intuitive concept of number, one is led to introduce a more general concept, like that of a proposition, a function, or an arbitrary correspondence, concepts which are in general not objectively defined. It is true that such a concept can be made more definite by the axiomatic method, as in axiomatic set theory, but then the system of axioms is quite complicated.

You know that Frege tried to deduce arithmetic from pure logic by viewing the latter as the general theory of the universe of mathematical objects. Although the foundation of this absolutely platonistic enterprise was undermined by the Russell-Zermelo paradox, the school of logicists has not given up the idea of incorporating arithmetic in a system of logic. In place of absolute platonism, they have introduced some initial assumptions. But because of these, the system loses the character of pure logic.

In the system of *Principia Mathematica*, it is not only the axioms of infinity and reducibility which go beyond pure logic, but also the initial conception of a universal domain of individuals and of a domain of predicates. It is really an *ad hoc* assumption to suppose that we have before us the universe of things divided into subjects and predicates, ready-made for theoretical treatment.

But even with such auxiliary assumptions, one cannot successfully incorporate the whole of arithmetic into the system of logic. For, since this system is developed according to fixed rules, one would have to be able to obtain by means of a fixed series of rules all the theorems of arithmetic. But this is not the case; as Gödel has shown, arithmetic goes beyond each given formalism. (In fact, the same is true of axiomatic set theory.)

Besides, the desire to deduce arithmetic from logic derives from the traditional opinion that the relation of logic to arithmetic is that of general to particular. The truth, it seems to me, is that mathematical abstraction does not have a lesser degree than logical abstraction, but rather another direction.

These considerations do not detract at all from the intrinsic value of that research of logicists which aims at developing logic systematically and formalizing mathematical proofs. We were concerned here only with defending the thesis that for the theory of numbers, the intuitive method is the most suitable.

On the other hand, for the theory of the continuum, given by analysis, the intuitionist method seems rather artificial. The idea of the continuum is a geometrical idea which analysis expresses in terms of arithmetic.

Is the intuitionist method of representing the continuum better adapted to the idea of the continuum than the usual one?

Weyl would have us believe this. He reproaches ordinary analysis for decomposing the continuum into single points. But isn't this reproach better addressed to semiplatonism, which views the continuum as a set of arithmetical laws? The fact is that for the usual method there is a completely satisfying analogy between the manner in which a particular point stands out from the continuum and the manner in which a real number defined by an arithmetical law stands out from the set of all real numbers, whose elements are in general only implicitly involved, by virtue of the quasi-combinatorial concept of a sequence.

This analogy seems to me to agree better with the nature of the continuum than that which intuitionism establishes between the fuzzy character of the continuum and the uncertainties arising from unsolved arithmetical problems.

It is true that in the usual analysis the notion of a continuous function, and also that of a differentiable function, have a generality going far beyond our intuitive representation of a curve. Nevertheless in this analysis, we can establish the theorem of the maximum of a continuous function and Rolle's theorem, thus rejoining the intuitive conception.

Intuitionist analysis, even though it begins with a much more restricted notion of a function, does not arrive at such simple theorems; they must instead be replaced by more complex ones. This stems from the fact that on the intuitionistic conception, the continuum does not have the character of a totality, which undeniably belongs to the geometric idea of the continuum. And it is this characteristic of the continuum which would resist perfect arithmetization.

These considerations lead us to notice that the duality of arithmetic and geometry is not unrelated to the opposition between intuitionism and platonism. The concept of number appears in arithmetic. It is of intuitive origin, but then the idea of the totality of numbers is superimposed. On the other hand, in geometry the platonistic idea of space is primordial, and it is against this background that the intuitionist procedures of constructing figures take place.

This suffices to show that the two tendencies, intuitionist and platonist, are both necessary; they complement each other, and it would be doing oneself violence to renounce one or the other.

But the duality of these two tendencies, like that of arithmetic and geometry, is not a perfect symmetry. As we have noted, it is not proper to make arithmetic and geometry correspond completely: the idea of number is more immediate to the mind than the idea of space. Likewise, we must recognize that the assumptions of platonism have a transcendent character which is not found in intuitionism.

It is also this transcendent character which requires us to take certain precautions in regard to each platonistic assumption. For even when such a supposition is not at all arbitrary and presents itself naturally to the mind, it can still be that the principle from which it proceeds permits only a restricted application, outside of which one would fall into contradiction.

We must be all the more careful in the face of this possibility, since the drive for simplicity leads us to make our principles as broad as possible. And the need for a restriction is often not noticed.

This was the case, as we have seen, for the principle of totality, which was pressed too far by absolute platonism. Here it was only the discovery of the Russell-Zermelo paradox which showed that a restriction was necessary.

Thus it is desirable to find a method to make sure that the platonistic assumptions on which mathematics is based do not go beyond permissible limits. The assumptions in question reduce to various forms of the principle of totality and of the principle of analogy or of the permanence of laws. And the condition restricting the application of these principles is none other than that of the consistency of the consequences which are deduced from the fundamental assumptions.

As you know, Hilbert is trying to find ways of giving us such assurances of consistency, and his proof theory has this as its goal.

This theory relies in part on the results of the logicists. They have shown that the arguments applied in arithmetic, analysis, and set theory can be formalized. That is, they can be expressed in symbols and as symbolic processes which unfold according to fixed rules. To primitive propositions correspond initial formulae, and to each logical deduction corresponds a sequence of formulae derivable from one another according to given rules. In this formalism, a platonistic assumption is represented by an initial formula or by a rule establishing a way of passing from formulae already obtained to others. In this way, the investigation of the possibilities of proof reduces to problems like those which are found in elementary number theory. In particular, the consistency of the theory will be proved if one succeeds in proving that it is impossible to deduce two mutually contradictory formulae A and $\overline{A}$ (with the bar representing negation). This statement which is to be proved is of the same structure as that, for example, asserting the impossibility of satisfying the equation $a^2 = 2b^2$ by two integers a and b.

Thus by symbolic reduction, the question of the consistency of a theory reduces to a problem of an elementary arithmetical character.

Starting from this fundamental idea, Hilbert has sketched a detailed program of a theory of proof, indicating the leading ideas of the arguments [**for the main consistency proofs**]. His intention was to confine himself to intuitive and combinatorial considerations; his "finitary point of view" was restricted to these methods.

In this framework, the theory was developed up to a certain point. Several mathematicians have contributed to it: Ackermann, von Neumann, Skolem, Herbrand, Gödel, Gentzen.

Nonetheless, these investigations have remained within a relatively restricted domain. In fact, they did not even reach a proof of the consistency of the axiomatic theory of integers. It is known that the symbolic representation of this theory is obtained by adding to the ordinary logical calculus formalizations of Peano's axioms and the recursive definitions of sum $(a + b)$ and product $(a \cdot b)$.

Light was shed on this situation by a general theorem of Gödel, according to which a proof of the consistency of a formalized theory cannot be represented by means of the formalism considered. From this theorem, the following more special proposition follows: It is impossible to prove by elementary combinatorial methods the consistency of a formalized theory which can express every elementary combinatorial proof of an arithmetical proposition.

Now it seems that this proposition applies to the formalism of the axiomatic theory of numbers. At least, no attempt made up to now has given us any example of an elementary combinatorial proof which cannot be expressed in this formalism, and the methods by which one can, in the cases considered, translate a proof into the aforementioned formalism, seem to suffice in general.

Assuming that this is so,[2] we arrive at the conclusion that means more

[2] In trying to demonstrate the possibility of translating each elementary combinatorial

powerful than elementary combinatorial methods are necessary to prove the consistency of the axiomatic theory of numbers. A new discovery of Gödel and Gentzen leads us to such a more powerful method. They have shown (independently of one another) that the consistency of intuitionist arithmetic implies the consistency of the axiomatic theory of numbers. This result was obtained by using Heyting's formalization of intuitionist arithmetic and logic. The argument is conducted by elementary methods, in a rather simple manner. In order to conclude from this result that the axiomatic theory of numbers is consistent, it suffices to assume the consistency of intuitionist arithmetic.

This proof of the consistency of axiomatic number theory shows us, among other things, that intuitionism, by its abstract arguments, goes essentially beyond elementary combinatorial methods.

The question which now arises is whether the strengthening of the method of proof theory obtained by admitting the abstract arguments of intuitionism would put us into a position to prove the consistency of analysis. The answer would be very important and even decisive for proof theory, and even, it seems to me, for the role which is to be attributed to intuitionistic methods.

Research in the foundations of mathematics is still developing. Several basic questions are open, and we do not know what we shall discover in this domain. But these investigations excite our curiosity by their changing perspectives, and that is a sentiment which is not aroused to the same degree by the more classical parts of science, which have attained greater perfection.

I wish to thank Professor Wavre, who was kind enough to help me improve the text of this lecture for publication. I also thank M. Rueff, who was good enough to look over the first draft to improve the French.

proof of an arithmetical proposition into the formalism of the axiomatic theory of numbers, we are confronted with the difficulty of delimiting precisely the domain of elementary combinatorial methods.

Chapter 12

Bernays Project: Text No. 14

Hilbert's investigations of the foundations of arithmetic (1935)

Hilberts Untersuchungen über die Grundlagen der Arithmetik

(Hilbert's *Gesammelte Abhandlungen*, Berlin: Springer, Bd. 3, pp. 196–216)

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Hilbert's first investigations of the foundations of arithmetic follow temporally as well as conceptually his investigations of the foundations of geometry. Hilbert begins the paper "On the concept of number"[1] by applying to arithmetic, just as to geometry, the axiomatic method, which he contrasts to the otherwise usually applied "genetic" method.

> Let us first recall the manner of introducing the concept of number. Starting from the concept of the number 1, usually one

[1] *Vide* [**?**].

thinks at first the further rational positive numbers 2, 3, 4,... as arising through the process of counting, and their laws of calculation as being developed in the same way; then one arrives at the negative number by the requirement of the general execution of subtraction; one further defines the rational number say as a pair of numbers—then every linear function has a zero—, and finally the real number as a cut or a fundamental sequence—thereby obtaining that every whole rational indefinite, and generally every continuous indefinite function has a zero. We can call this method of introducing the concept of number the *genetic method*, because the most general concept of real number is *generated* by successive expansion of the simple concept of number.

One proceeds fundamentally differently with the development of geometry. Here one tends to begin with the assumption of the existence of all elements, i. e., one presupposes at the outset three systems of things, namely the points, the lines, and the planes, and then brings these elements—essentially after the example of Euclid—into relation with each other by certain axioms, namely the axioms of incidence, of ordering, of congruency, and of continuity. Then the necessary task arises of showing the *consistency* and *completeness* of these axioms, i. e., it must be proven that the application of the axioms that have been laid down can never lead to contradictions, and moreover that the system of axioms suffices to prove all geometric theorems. We shall call the procedure of investigation sketched here the *axiomatic method*.

We raise the question, whether the genetic method is really the only one appropriate for the study of the concept of number and the axiomatic method for the foundations of geometry. It also appears to be of interest to contrast both methods and to investigate which method is the most advantageous if one is concerned with the logical investigation of the foundations of mechanics or other physical disciplines.

My opinion is this: *Despite the great pedagogical and heuristic value of the genetic method, the axiomatic method nevertheless deserves priority for the final representation and complete logical securing of the content of our knowledge.*

Already Peano developed number theory axiomatically.[2] Hilbert now sets up an axiom system for analysis, by which the system of real number is characterized as a real Archimedean field which cannot be extended to a more extensive field of the same kind.

A few illustrative remarks about dependencies follow the enumeration of the axioms. In particular it is mentioned that the law of commutativity of multiplication can be deduced from the remaining properties of a field and the order properties with the help of the Archimedean axiom, but not without it.

The requirement of non-extendibility is formulated by the "axiom of completeness." This axiom has the advantage of conciseness; however, its logical structure is complicated. In addition it is not immediately apparent from it that it expresses a demand of continuity. If one wants, instead of this axiom, one that clearly has the character of a demand of continuity and on the other hand does not already include the requirement of the Archimedean axiom, it is recommended to take Cantor's axiom of continuity, which says that if there is a series of intervals such that every interval includes the following one, then there is a point which belongs to every interval. (The formulation of this axiom requires the previous introduction of the concept of number series).[3]

[2][1] G. Peano, *The Principles of Arithmetic* (*vide* [**?**]). The introduction of recursive definitions is here not unobjectionable; the proof of the solvability of the recursion equations is missing. Such a proof was provided already by Dedekind in his essay *The Nature and Meaning of Numbers* (*vide* [**?**]). If one bases the introduction of recursive functions on Peano's axioms, it is best to proceed by first proving the solvability of the recursion equations for the sum following L. Kalmár by induction on the parameter argument, then defining the concept "less than" with the help of the sum, and finally using Dedekind's consideration for the general recursive definition. This procedure is presented in Landau's textbook *Foundations of Analysis* (*vide* [**?**]). Admittedly here the concept of function is used. If one wants to avoid it, the recursion equations of the sum and product have to be introduced as axioms. The proof of the general solvability of recursion equations follows then by a method by K. Gödel (cf. "On formally undecidable propositions of *Principia Mathematica* and related systems I" (*vide* [**?**]), and also Hilbert-Bernays *Foundations of Mathematics I* (*vide* [**?**], ■ pp. 412)).

[3][1] Concerning the independence of the Archimedean axiom from the mentioned axiom of Cantor, cf. P. Hertz: "On the axioms of Archimedes and Cantor" (*vide* [**?**]).

R. Baldus has recently called attention to Cantor's axiom. See his essay "On the axiomatics of geometry:" "I. On Hilbert's axiom of completeness," "II. Simplifications of the Archimedean and Cantorian axiom," "III. On the Archimedean and Cantorian axiom" (*vide* [**?**], [**?**], [**?**]) as well as the following essay by A. Schmidt: "The continuity in absolute

The aim which Hilbert pursues with the axiomatic version of analysis appears particularly clearly at the end of the essay in the following words:

> The objections that have been raised against the existence of the totality of all real numbers and infinite sets in general lose all their legitimacy with the view identified above: we do not have to conceive of the set of real numbers as, say, the totality of all possible laws according to which the elements of a fundamental sequence can proceed, but rather—as has just been explained—as a system of things whose relations between each other are given by the *finite and completed* system of axioms I–IV, and about which new propositions are valid only if they can be deduced from those axioms in a finite number of logical inferences.

But the methodical benefit which this view brings also involves a further requirement: for the axiomatic formulation necessarily entails the task of proving the consistency of the axiom system in question.

Therefore, the problem of the proof of consistency for the arithmetical axioms was mentioned in the list of problems that Hilbert posed in his lecture in Paris "Mathematical problems."[4]

To accomplish the proof Hilbert thought to get by with a suitable modification of the methods used in the theory of real numbers.

But in the more detailed engagement with the problem he was immediately confronted with the considerable difficulties that exist for this task. In addition, the set theoretic paradox that was discovered in the meantime by Russell and Zermelo prompted increased caution in the inference rules. Frege and Dedekind were forced to withdraw their investigations in which they thought they had provided unobjectionable foundations of number theory—Dedekind using the general concepts of set theory, Frege the framework of pure logic[5]—since it resulted from that paradox that their considerations contained inadmissible inferences.

The talk[6] "On the foundations of logic and arithmetic" held in 1904 shows us a completely novel point of view. Here first the fundamental difference

geometry" (*vide* [?]).

4[2] Held at the International Congress of Mathematicians 1900 in Paris (*vide* [?]).

5[1] R. Dedekind: *The Meaning and Nature of Numbers* (*vide* [?]). G. Frege: *Basic Laws of Arithmetic* (*vide* [?]).

6[2] At the International Congress of Mathematicians in Heidelberg 1904 (*vide* [?]).

is pointed out between the problem of the consistency proof for arithmetic and for geometry. The proof of consistency for the axioms of geometry uses an arithmetical interpretation of the geometric axiom system. However, for the proof of consistency of arithmetic "it seems that the appeal to another foundational discipline is not allowed."

To be sure, one could think of a reduction to logic.

> But by attentive inspection we become aware that certain arithmetical basic concepts are already used in the traditional formulation of the laws of logic, e. g., the concept of set, in part also the concept of number, in particular cardinal number. So we get into a quandary, and to avoid paradoxes a partly simultaneous development of the laws of logic and arithmetic is required.

Hilbert now presents the plan of such a joint development of logic and arithmetic. This plan contains already in great part the leading viewpoints for proof theory, in particular the idea of transforming the proof of consistency into a problem of elementary-arithmetic character by translating the mathematical proofs into the formula language of symbolic logic. Also rudiments of the consistency proofs can be already found here.

But the execution remains still in its beginnings. In particular, the proof for the "existence of the infinite" is carried out only in the framework of a very restricted formalism.

The methodical standpoint of Hilbert's proof theory is also not yet developed to its full clarity in the Heidelberg talk. Some passages suggest that Hilbert wants to avoid the intuitive idea of number and replace it with the axiomatic introduction of the concept of number. Such a procedure would lead to a circle in the proof theoretic considerations. Also the viewpoint of the restriction in the contentual application of the forms of the existential and general judgment is not yet brought to bear explicitly and completely.

In this preliminary state Hilbert interrupted his investigations of the foundations of arithmetic for a long period of time.[7] Their resumption is found

[7][1] A continuation of the direction of research that was inspired by Hilbert's Heidelberg talk was carried out by J. König, who, in his book *New Foundations for Logic, Arithmetic, and Set Theory* (*vide* [**?**]), surpasses the Heidelberg talk both by a more exact formulation and a more thorough presentation of the methodical standpoint, as well as by the execution. Julius König died before finishing the book; it was edited by his son as a fragment. This work, which is a precursor of Hilbert's later proof theory, exerted no influence

announced in the 1917 talk[8] “Axiomatic thinking.”

This talk comes in the wake of the manifold successful axiomatic investigations that had been pursued by Hilbert himself and other researchers in the various fields of mathematics and physics. In particular in the field of the foundations of mathematics the axiomatic method had led in two ways to an extensive systematization of arithmetic and set theory. Zermelo formulated in 1907 his axiom system for set theory[9] by which the processes of set formation are delimited in such a way that on the one hand the set theoretic paradoxes are avoided and on the other hand the set theoretic inferences that are customary in mathematics are retained. And Frege’s project of a logical foundation of arithmetic—for which to be sure the method that Frege employed himself turned out to be faulty—was reconstructed by Russell and Whitehead in their work *Principia Mathematica*[10].[11]

on Hilbert. But later J. v. Neumann followed the approach of König in his investigation “Concerning Hilbert’s proof theory” (*vide* [**?**]).

[8][2] At *Naturforscherversammlung Zürich* (*vide* [**?**]).

[9][3] E. Zermelo, “Research in the foundations of set theory I” (*vide* [**?**]). More recently there have been various investigations building on this axiom system. A. Fraenkel added the axiom of replacement, an extension of the admissible formation of sets in the spirit of Cantor’s set theory; J. v. Neumann added an axiom, which rules out that the process of going from a set to one of its elements can, for any given set, be iterated arbitrarily many times. Moreover, Th. Skolem, Fraenkel, and J. v. Neumann have made more precise, all in a different way, in the sense of a sharper implicit characterization of the concept of set, the concept of “definite proposition” which was used by Zermelo in vague generality. The result of these refinements is presented in the most concise way in v. Neumann’s axiomatic$_{c_1}\mathbf{s}_{c_1}$; namely it is achieved here, that all axioms are of the “first order” (in the sense of the terminology of symbolic logic). Zermelo rejects such a refinement of the concept of set, in particular in the light of the consequence that was first discovered by Skolem that such a sharper axiom system of set theory can be realized in the domain of individuals of the whole numbers.—A presentation of these investigations up to the year 1928, with detailed references, is contained in the textbook by A. Fraenkel, *Introduction to Set Theory* (*vide* [**?**]). See also: J. v. Neumann, “Concerning a consistency question in axiomatic set theory,” Th. Skolem, “On a foundational question of mathematics,” E. Zermelo, “On limit numbers and domains of sets” (*vide* [**?**], [**?**], [**?**]).

[10][1] *Vide* [**?**].

[11][2] The axiomatic form of the set up is also present in Frege’s system. In Russell and Whitehead’s way of proceeding, the contradiction found in Frege’s system is removed by refusing to treat concept-extensions (classes) as individuals (objects): rather, a statement about the extension of a concept is treated as a re-formulation of a statement about the concept itself. In this way the distinction between levels is transferred from concepts to classes. Incidentally, for this way of removing the contradiction a simpler distinction of

Hilbert says about this axiomatization of logic that one could "see the crowning of the work of axiomatization in general" in the completion of this enterprise. But this praise and acknowledgment is immediately followed by the remark that the completion of the project "still needs new work on many fronts."

In fact, the viewpoint of *Principia Mathematica* contains an unsolved problematic. What is supplied by this work is the elaboration of a clear system of assumptions for a simultaneous deductive development of logic and mathematics, as well as the proof that this set-up in fact succeeds. For the reliability of the assumptions, besides their contentual plausibility (which also from the point of view of Russell and Whitehead does not yield a guarantee of consistency), only their testing in the deductive use is put forward. But this testing too provides us in regard to consistency only an empirical confidence, not complete certainty. The complete certainty of consistency, however, is regarded by Hilbert as a requirement of mathematical rigor.

Thus the task of providing a consistency proof remains also for those assumptions, according to Hilbert. To handle this task as well as various further fundamental questions, e. g., "the problem of the solvability in principle of every mathematical question" or "the question of the relation between content and formalism in mathematics and logic," Hilbert thinks it necessary to make "the concept of specifically mathematical proof itself the object of investigation."

In the following years, in particular since 1920, Hilbert devoted himself especially to the plan, hereby taken up anew, of a proof theory.[12] His drive in this direction was strengthened by the opposition which Weyl and Brouwer directed at the usual procedure in analysis and set theory.[13].

Thus Hilbert begins his first communication about his "New foundation of mathematics"[14] by discussing the objections of Weyl and Brouwer. It is noteworthy in this dispute that Hilbert, despite his energetic rejection of the

levels, already to be found in Frege, is sufficient.

12[1] To collaborate on this enterprise Hilbert then invited P. Bernays with whom he has regularly discussed his investigations since then.

13[2] H. Weyl, *The Continuum. Critical Investigations Into the Foundations of Analysis*, "The vitious circle in the current founding of analysis," "On a new foundational crisis in mathematics" (*vide* [?], [?], [?]). – L. E. J. Brouwer, "Intuitionism and formalism", "Foundation of set theory independent of the principle of excluded middle. I–II," "Intuitionistic set theory," "Has every real number a decimal expansion" (*vide* [?], [?], [?], [?], [?]).

14[3] Talk, given in Hamburg 1922 (*vide* [?]).

objections that have been raised against analysis, and despite his advocacy for the legitimacy of the usual inferences, agrees with the opposing standpoint that the usual treatment of analysis is not immediately evident and does not conform to the requirements of mathematical rigor. The "legitimacy" that Hilbert, from this point of view, grants to the usual procedure is not based on evidence, but on the reliability of the axiomatic method, of which Hilbert explains that if it is appropriate anywhere at all, then it is here. This is a conception from which the problem of a proof of consistency for the assumptions of analysis arises.

Moreover, as for the methodical attitude on which Hilbert bases his proof theory and which he explains using the intuitive treatment of number theory, there is a great drawing near to the standpoint of Kronecker[15]—despite the position Hilbert took against Kronecker. This consists in particular in the application of the intuitive concept of number, and also in the fact that the intuitive form of complete induction (i. e., the inference which is based on the intuitive idea of the "setup" of the numerals) is regarded as acceptable and as not requiring any further reduction. By deciding to adopt this methodical assumption Hilbert also got rid of the basis of the objection that Poincaré had raised at that time against Hilbert's enterprise of the foundation of arithmetic based on the exposition in the talk in Heidelberg.[16]

The beginning of proof theory, as it is laid down in the first communication, already contains the detailed formulation of the formalism. In contrast to the Heidelberg talk, the sharp separation of the logical-mathematical formalism and the contentful "metamathematical" consideration is prominent, and is expressed in particular by the distinction of signs "for communication" and symbols and variables of the formalism.

But the formal restriction of negation to inequalities appears as a remnant of the stage when this separation had not yet been performed, while a restriction is really only needed in the metamathematical application of negation.

A characteristic of Hilbert's approach, the formalization of the *tertium*

[15][1] In a later talk "The founding of elementary arithmetic" (held in Hamburg, *vide* [**?**]), Hilbert has spoken more clearly about this. After mentioning Dedekind's investigation *The meaning and nature of numbers* he explains: "Around the same time, thus already more than a generation ago, Kronecker clearly articulated a view which today in essence coincides with our finite attitude, and illustrated it with many examples."

[16][2] H. Poincaré, "Mathematics and logic" (*vide* [**?**]).

non datur by transfinite functions, appears already in the first communication. In particular, the *tertium non datur* for the whole numbers is formalized with the function function $\chi(f)$, whose argument is a number theoretic function, and which has the value 0 if $f(a)$ has the value 1 for all number values a, but otherwise represents the smallest number value a for which $f(a)$ has a value different than 1.

The leading idea for the proof of the consistency of the transfinite functions (i. e., of their axioms), which Hilbert already possessed, is not presented in this communication. A proof of consistency is rather provided here only for a certain part of the formalism; but this proof is only important as an example of a metamathematical proof.[17]

In the Leipzig talk "The logical foundations of mathematics,"[18] which followed soon after the first communication, we find the approach and realization of proof theory developed further in various respects. I want to mention briefly the main respects in which the presentation of the Leipzig talk goes beyond those of the first communication:

1. The fundamental way in which ordinary mathematics goes beyond the intuitive approach (which consists in the unrestricted application of the concepts "all," "there exists" to infinite totalities) is pointed out and the concept of "finite logic" is elaborated. Furthermore, a comparison between the role of "transfinite" formulas and that of ideal elements is carried out here for the first time.

2. The formalism is freed from unnecessary restrictions (in particular the avoidance of negation).

3. The formalization of the *tertium non datur*, and also of the principle of choice using transfinite functions, is simplified.

4. The main features of the formalism of analysis are developed.

5. The proof of consistency is provided for the elementary number theoretic formalism that results from the exclusion of bound variables. The task of proving the consistency of number theory and analysis is then focused on the treatment of the "transfinite axiom"

$$A(\tau(A)) \;\rightarrow\; A(a),$$

[17][1] The method of proof rests here mainly on the fact that the elementary inference rules for the implication, which are formalized by the "Axioms of logical inference" (numbered 10 through 13), are not included in the part of the formalism under consideration.

[18][2] Held at the *Deutscher Naturforscher-Kongreß* 1922 (*vide* [?]).

which is employed in two ways, since the argument of A is related on the one hand to the domain of ordinary numbers and on the other hand to the number series (functions).

6. A method (which is successful at least in the simplest cases) is stated for the treatment of the "transfinite axiom" in the consistency proof.

The basic structure of proof theory was reached with its formulation as presented in the Leipzig talk.

Hilbert's next two publications on proof theory, the Münster talk "On the infinite"[19] and the (second) talk in Hamburg "The foundations of mathematics,"[20] in which the basic idea and the formal approach of proof theory is presented anew and in more detail, still show various changes and extensions in the formalism. However, they serve only in smaller part the original goal of proof theory; they are used mainly with respect to the plan to solve Cantor's continuum problem, i. e., the proof of the theorem that the continuum (the set of real numbers) has the same cardinality as the set of numbers of the second number class.

Hilbert had the idea of ordering the number theoretic functions, i. e., the functions that map every natural number to another—(the elements of the continuum surely can be represented by such functions)—in accordance with the type of the variables which are needed for their definition, and to achieve a mapping of the continuum to the set of numbers of the second number class on the basis of the ascent of the variable types, which is analogous to that of the transfinite ordinal numbers. But the pursuit of this goal did not get beyond a sketch, and Hilbert therefore left out the parts which refer to the continuum problem in the reprints of both mentioned talks in *Foundations of Geometry*.[21]

Hilbert's considerations about the treatment of the continuum problem have nevertheless produced various fruitful suggestions and viewpoints.

Thus W. Ackermann has been inspired to his investigation "Concerning Hilbert's built-up of the real numbers"[22] by the considerations regarding the

[19][1] Presented in 1925 on the occasion of a meeting organized in honor of the memory of Weierstrass (*vide* [**?**]).

[20][2] Presented in 1927 (*vide* [**?**]).

[21][3] Both talks are included in the seventh edition of *Foundations of Geometry* as appendix VIII and IX. Other than the omissions also small editorial changes have been $_{c_1}$**made**$_{c_1}$, in particular with respect to the notation of the formulas.

[22][4] *Vide* [**?**].

recursive definitions. Hilbert lectures in his talk in Münster on the question and the result of this paper (which had not been published at the time):

> Consider the function
>
> $$a + b;$$
>
> by iterating n times and equating it follows from this:
>
> $$a + a + \cdots + a = a \cdot n.$$
>
> Likewise one arrives from
>
> $$a \cdot b \quad \text{zu} \quad a \cdot a \cdots a = a^n,$$
>
> further from
>
> $$a^b \quad \text{to} \quad a^{(a^a)}, a^{(a^{(a^a)})}, \dots .$$
>
> So we successively obtain the functions
>
> $$\begin{aligned} a + b &= \varphi_1(a, b), \\ a \cdot b &= \varphi_2(a, b), \\ a^b &= \varphi_3(a, b). \end{aligned}$$
>
> $\varphi_4(a, b)$ is the b^{th} value in the series:
>
> $$a, a^a, a^{(a^a)}, a^{(a^{(a^a)})}, \dots .$$
>
> In analogous way one obtains $\varphi_5(a, b), \varphi_6(a, b)$ etc.
>
> It would now be possible to define $\varphi_n(a, b)$ for variable n by substitution and recursion; but these recursions would not be ordinary successive ones, but rather one would be led to a crossed recursion of different variables at the same time (simultaneous), and it is only possible to resolve this into ordinary successive recursions by using the concept of a function variable: the function $\varphi_a(a, a)$ is an example for a function of the number variable a, which can not be defined by substitution and ordinary successive recursion alone, if one allows only for number variables.[23] How

[23][1] W. Ackermann has provided a proof for this claim. (Footnote in Hilbert's text.)

> the function $\varphi_a(a, a)$ can be defined using function variables is shown by the following formulas:
>
> $$\begin{aligned}\iota(f, a, 1) &= a,\\ \iota(f, a, n+1) &= f(a, \iota(f, a, n));\\ \varphi_1(a, b) &= a + b,\\ \varphi_{n+1}(a, b) &= \iota(\varphi_n, a, b).\end{aligned}$$
>
> Here ι stands for an individual function with two arguments, of which the first one is itself a function of two ordinary number variables.

The investigation of recursive definitions has been recently carried forward by Rozsa Péter. She proved that all recursive definitions which proceed only after the values of *one* variable and which do not require any other sort of variables than the free number variables, can be reduced to the simplest recursion schema. Using this result she also simplified substantially the proof of the paper of Ackermann just mentioned.[24]

These results concern the use of recursive definitions to obtain number theoretic functions. In Hilbert's proof plan recursive definitions also occur in a different way, namely, as a procedure for constructing numbers of the second number class and also types of variables. Here Hilbert presupposes certain general ideas concerning the sorts of variables, of which he gives the following short summary in the talk "The foundations of mathematics:"

> The *mathematical variables* are of two sorts:
>
> $_a$______________$_a$1. the *basic variables*,
>
> $_a$______________$_a$2. the *types of variables*.
>
> 1. While one gets by with the ordinary whole number as the only basic variable in all of arithmetic and analysis, now a basic variable for each one of Cantor's transfinite number classes is added, which is able to assume the ordinal numbers belonging to this class. To each basic variable there accordingly corresponds a proposition that characterizes it as such; this is defined implicitly by axioms.

[24][1] See R. Péter, "On the relation between the different notions of the recursive functions" and "Construction of non-recursive functions" (*vide* [?], [?]).

> To each basic variable belongs a kind of recursion, which is used to define functions whose argument is such a basic variable. The recursion belonging to the number variable is the "usual recursion" by which a function of a number variable n is defined by specifying which value it has for $n = 0$ and how the value for n' is obtained from the value at n.[25] The generalization of the usual recursion is transfinite recursion, whose general principle is to determine the value of the function for a value of the variable using the previous values of the function.
>
> 2. We derive further kinds of variables from the basic variables by applying logical connectives to the propositions for the basic variables, e. g., to Z.[26] The so defined variables are called types of variables, and the statements defining them are called type-statements; for each of these a new individual symbol is introduced. Thus the formula
>
> $$\Phi(f) \sim (x)(Z(x) \to Z(f(x)))$$
>
> yields the simplest example of a type of variables; this formula defines the type of function variables (being a function). A further example is the formula
>
> $$\Psi(g) \sim (f)(\Phi/\blacksquare\ /\Psi(f) \to Z(g(f)));$$
>
> it defines "being a function-function;" the argument g is the new function-function variable.
>
> For the construction of higher variable types the type-statements have to be equipped with indices which enables a method of recursion.

These concept formations are applied in particular in the theory of numbers of the second number class. Here a new suggestion emerged from Hilbert's conjecture that every number of the second number class can be defined without transfinite recursion, but using ordinary recursion alone—assuming a basic element 0, the operation of progression by one ("stroke-function") and the limit process, as well as the number variable and the basic variable of the second number class—.

25[2] Here n' is the formal expression for "the number following n."

26[3] The formula $Z(a)$ corresponds to the proposition "a is an ordinary whole number."

The first examples of such definitions that go beyond the most elementary cases, namely the definition of the first ε-number (in Cantor's terminology) and the first critical ε-number,[27] have already been given by P. Bernays and J. v. Neumann. Hereby already recursively defined types of variables are used.[28]

But these various considerations, which refer to the recursive definitions, already go beyond the narrower domain of proof theoretic questions. Since Hilbert's Leipzig talk it was the task of this narrower field of investigation of proof theory to prove consistency according to Hilbert's approach, including the transfinite axiom. Shortly after the talk in Leipzig the transfinite axiom was brought into the form of the logical "ε-axiom"

$$A(a) \;\rightarrow\; A(\varepsilon_x A(x))$$

by the introduction of the choice function $\varepsilon(A)$ (in detail: $\varepsilon_x A(x)$) replacing the earlier function $\tau(A)$. The role of this ε-axiom is explained by Hilbert in his talk in Hamburg $_{c_1 c_1}$ with $_{c_1}$**the**$_{c_1}$ following words:

> The ε-function is applied in the formalism in three ways.
>
> $_a$1.$_a$ It is possible to define "all" and "there exists" with the help of ε, namely as follows:[29]
>
> $$(x)A(x) \sim A(\varepsilon_x \overline{A(x)}),$$
> $$(Ex)A(x) \sim A(\varepsilon_x A(x)).$$

[27][1] An ε-number is a transfinite ordinal number α with the property $\alpha = \omega^\alpha$. The first ε-number is the limit of the series

$$\alpha_0, \alpha_1, \alpha_2, \ldots,$$

where $\alpha_0 = 1$, $\alpha_{n+1} = \omega^{\alpha_n}$; the first critical ε-number is the limit of the series

$$\beta_0, \beta_1, \beta_2, \ldots,$$

where $\beta_0 = 1$, β_{n+1} is the β_n-th ε-number.

[28][2] Cf. the statement in Hilbert's talk "The foundations of mathematics" ([**?**], pp. 81.).—The examples mentioned have not been published yet.

[29][1] Instead of the double arrow used by Hilbert the symbol of equivalence $\sim$ is applied in both $_{c_1}$**of the**$_{c_1}$ following formulas; the remarks on the introduction of the symbol $\sim$ in Hilbert's text are thus dispensable.

Based on this definition the ε axioms yields the valid logical notations for the "for all" and "there exists" symbols, like

$$\begin{array}{lll} (x)A(x) \;\rightarrow\; A(a) & & \text{(Aristotelian axiom)}, \\ \overline{(x)}A(x) \;\rightarrow\; (Ex)\overline{A(x)} & & (\textit{Tertium non datur}). \end{array}$$

$_a$2.$_a$ If a proposition $\mathfrak{A}$ is true of one and only one thing, then

$_{aa}$

$$\varepsilon(\mathfrak{A}) \textit{ that thing}, \text{ for which } \mathfrak{A} \text{ holds. } _{aa}$$

Thus, the ε-function allows one to resolve such a proposition $\mathfrak{A}$ that holds of only one thing into the form

$$a = \varepsilon(\mathfrak{A}).$$

$_a$3.$_a$ Moreover, the ε plays the role of a choice function, i. e., in the case that $\mathfrak{A}$ holds of more than one thing, $\varepsilon(\mathfrak{A})$ is *any* of the things a of which $\mathfrak{A}$ holds.

The ε-axiom can be applied to different types of variables. For a formalization of number theory the application to number variables suffices, i. e., the type of natural numbers. In this case the number theoretic axioms

$$\begin{gathered} a' \neq 0, \\ a' = b' \rightarrow a = b, \end{gathered}$$

as well as the recursion equations for addition and multiplication[30] and the principle of inference of complete induction, have to be added to the the logical formalism and the axioms of equality. This principle of inference can be formalized using the ε symbol by the formula

$$\varepsilon_x A(x) = b' \;\rightarrow\; \overline{A(b)}$$

in connection with the elementary formula

$$a \neq 0 \;\rightarrow\; a = (\delta(a))'.$$

30[2] Cf. footnote 1 on p. 197 of this report.

The additional formula for the ε symbols corresponds to a part of the statement of the least number principle [31] and the added elementary formula represents the statement that for every number different than 0 there is a preceeding one.

For the formalization of analysis one has to apply the ε-axiom also to a higher type of variables. Different alternatives are possible here, depending on whether one prefers the general concept of predicate, set, or function. Hilbert chooses the type of function variables, i. e., more precisely, of the variable number theoretic function of one argument.

The introduction of higher types of variables allows for the replacement of the inference principle of complete induction by a definition of the concept of natural number following the method of Dedekind.

The essential factor in the extension of this formalism is based on the connection between the ε-axiom and the replacement rule for the function variable, whereby the "impredicative definitions" of functions, i. e., the definitions of functions in reference to the totality of functions, are incorporated into the formalism.

The task of proving consistency for the number theoretic formalism and for analysis is hereby mathematically sharply delimited. For its treatment one had Hilbert's approach at one's disposal, and at the beginning it seemed that only an insightful and extensive effort was needed to develop this approach to a complete proof.

However, this vision has been proved mistaken. In spite of intensive efforts and a multitude of contributed proof ideas the desired goal has not been achieved. The expectations that had been entertained have been disappointed step by step, and in the same process it also became apparent that the danger of mistake is particularly great in the domain of metamathematical considerations.

At first the proof of the consistency of analysis seemed to succeed, but this appearance soon revealed itself as an illusion. Thereafter it was believed that the goal had been reached at least for the number theoretic formalism. Hilbert's talk in Hamburg "The foundations of mathematics" falls in this stage, where at the end he cites a report on a consistency proof by Ackermann, as well as the talk "Problems in founding mathematics,"[32] held in

[31][1] $_{c_1}$**That is**$_{c_1}$, the principle of the existence of a least number in every nonempty set of numbers.

[32][2] *Vide* [**?**].

1928 in Bologna, where Hilbert gave an overview of the situation in proof theory at that time and put forward in part problems of consistency and in part problems of completeness.

Here Hilbert connects all problems of consistency to the ε-axiom, presenting the mathematical domains that are encompassed in place of the various formalisms.

In this presentation is expressed the view, shared at that time by all parties, that the proof for the consistency of the formalism of number theory had been given already by the investigations of Ackermann and v. Neumann.

That in fact this goal had not yet been achieved was only realized when it became dubious, based on a general theorem of K. Gödel, whether it was at all possible to provide a proof for the consistency of the number theoretic formalism with elementary combinatorial methods in the sense of the "finite standpoint."

The theorem mentioned is one of the various important results of Gödel's paper "On formally undecidable propositions of *Principia Mathematica* and related systems I,"[33] which has clarified in a fundamental way the relation between content and formalism—the investigation of which was mentioned by Hilbert in "Axiomatic thinking" as one of the aims of proof theory.

The basic message of the theorem is that a proof of the consistency of a consistent formalism encompassing the usual logical calculus and number theory cannot be represented in this formalism itself; more precisely: it is not possible to deduce the elementary arithmetical theorem which represents the claim of the consistency of the formalism—based on a certain kind of enumeration of the symbols and variables and an enumeration of the formulas and of the finite series of formulas derived from it—in the formalism itself.

To be sure, nothing is said hereby directly about the possibility of finite consistency proofs; but a criterion follows, which every proof of the consistency for a formalism of number theory or a more comprehensive formalism has to meet: a consideration must occur in the proof which can not be represented—based on the arithmetical translation—in the given formalism.

By means of this criterion one became aware that the existing consistency proofs were not yet sufficient for the full formalism of number theory.[34]

[33][1] *Vide* [**?**].

[34][2] $_{c_1}$**The proof by v. Neumann**$_{c_1}$ referred to a narrower formalism from the outset; but it appeared that the extension to the entire formalism of number theory would be without difficulties.

Moreover, the conjecture was prompted that it was in general impossible to provide a proof for the consistency of the number theoretic formalism within the framework of the elementary intuitive considerations that conformed to the "finite standpoint" upon which Hilbert had based proof theory.

This conjecture has not been disproved yet.[35] However, K. Gödel and G. Gentzen have noticed[36] that it is rather easy$_{c_1}$, **assuming the consistency of intuitionistic arithmetic as formalized by A. Heyting**[37]**,**$_{c_1}$ to prove the consistency of the usual formalism of number theory.[38]

From the standpoint of Brouwer's Intuitionism the proof of the consistency of the formalism of number theory has hereby been achieved. But this does not disprove the conjecture mentioned above, since intuitionistic arithmetic goes beyond the realm of intuitive, finite considerations by having also contentful proofs as objects besides the proper mathematical objects, and therefore needs the abstract general concept of an intelligible inference.—

A brief compilation of various finite consistency proofs for formalisms of parts of number theory that have been given will be presented here. Let the formalism which is obtained from the logical calculus (of first order) by adding axioms for equality and number theory, but where the application of complete induction is restricted to formulas without bound variables, be denoted by F_1; with F_2 we denote the formalism that results from F_1 by adding the ε-symbol and the ε-axiom,—whereby the formulas and schemata for the universal and existential quantifiers can be replaced by explicit definitions of the universal and existential quantifiers.[39] A consistency proof for F_2 immediately results

[35][1] But see postscriptum on p. 184.

[36][2] K. Gödel, "On intuitionistic arithmetic and number theory" (*vide* [**?**]). G. Gentzen has withdrawn his paper about the subject matter which was already in print because of the publication of Gödel's note.

[37][3] A. Heyting: "The formal rules of intuitionistic logic" and "The formal rules of intuitionistic mathematics" (*vide* [**?**] and [**?**]).

[38][4] Namely, it is possible to show that every formula that is deducible in the usual formalism of number theory, which does not contain any formula variable, disjunction, or existential quantifier, can be deduced also in Heyting's formalism.

[39][5] See in this paper p. 176.—With regard to the axioms of equality it is to be observed that they appear in the formalism in the more general form

$$a = a, \quad a = b \ \rightarrow \ (A(a) \rightarrow A(b))$$

so that in particular the formula

$$a = b \ \rightarrow \ \varepsilon_x A(x, a) = \varepsilon_x A(x, b)$$

in the consistency of F_1.

The consistency of F_2 is shown:

1. by a proof of W. Ackermann, which proceeds from the approach presented in Hilbert's Leipzig talk "The logical foundations of mathematics"[40];

2. by a proof by J. v. Neumann, who proceeds from the same assumptions[41];

3. using a second so far unpublished approach of Hilbert's executed by Ackermann; the idea behind this approach consists in applying a disjunctive rule of inference to eliminate the ε symbol instead of replacing the ε by number values.[42]

The consistency of F_1 is shown:

1. by a proof of J. Herbrand which rests on a general theorem$_{c_1}$—**which was stated for the first time and proved by Herbrand in his thesis "Investigations in proof theory"**[43]—$_{c_1}$about the logical calculus[44];

2. by a proof of G. Gentzen, which results from a sharpening and extension of Herbrand's theorem mentioned above found by Gentzen.[45]

For the time being one has not gone beyond these results, which are important mainly for theoretical logic and elementary axiomatics, and for the uncovering mentioned above of the relation between the usual number theoretic formalism and that of intuitionistic arithmetic.

can be deduced. In the formalism F_1 the formula

$$a = b \;\rightarrow\; (A(a) \rightarrow A(b))$$

can be replaced by the more special axioms

$$a = b \;\rightarrow\; (a = c \rightarrow b = c), \quad a = b \rightarrow a' = b'.$$

[40][1] The concluding portion of the proof is not yet carried out in detail in Ackermann's dissertation "Justification of the *tertium non datur* by Hilbert's theory of consistency" (*vide* [?]). Later Ackermann provided a complete and at the same time more simple proof. This definitive version of Ackermann's proof has not been published yet; so far only Hilbert's already mentioned report in his second talk in Hamburg "The foundations of mathematics" and the more detailed "Appendix" by P. Bernays which appeared with the talk are available (*vide* [?], [?] (this edition ch. 4, pp. 43 *seqq.*)). (The remark at the end of the appendix with regard to the inclusion of complete induction has to be abandoned.)

[41] J. v. Neumann, "Concerning Hilbert's proof theory" (*vide* [?]).

[42][3] Cf. the statement in the talk "Methods for demonstrating consistency and their limitations" by P. Bernays (*vide* [?], (this edition ch. 9, pp. 123 *seqq.*)).

[43][4] *Vide* [?].

[44][5] J. Herbrand, "On the consistency of arithmetic" (*vide* [?]).

[45][1] G. Gentzen, "■ Investigations in logical reasoning" (*vide* [?]).

But all the problems of completeness which Hilbert posed in his talk "Problems in founding mathematics" have been treated in various directions.

One of these problems deals with the proof of the completeness of the system of logical rules which are formalized in the logical calculus (of first order). This proof has been given by K. Gödel in the sense that he showed[46]: if it can be shown that a formula of the first order logical calculus is not deducible, then it is possible to give a counterexample to the universal validity of that formula in the framework of number theory (using *tertium non datur*, in particular in the form of the least number principle).

The other problem of completeness regards the axioms of number theory; it is to be shown: If a number theoretic statement can be shown to be consistent (on the basis of the axioms of number theory), then it is also provable. This claim contains also the following: "If it can be shown that a sentence[47] $\mathfrak{S}$ is consistent with the axioms of number theory, then the consistency with those axioms cannot also be shown for the sentence $\overline{\mathfrak{S}}$ (the converse of $\mathfrak{S}$)."

This problem is so far indeterminate, in that it is not specified on which formalism of logical inference it should be based. However, it was shown that the claim of completeness is justified for all logical formalisms, as long as one maintains the requirement of the rigorous formalization of the proofs.

This result stems again from K. Gödel, who proved the following general theorem in the paper mentioned above "On formally undecidable propositions of *Principia Mathematica* and related systems I:" If a formalism $\mathfrak{F}$ is consistent in the sense that it is impossible to deduce the negation of a formula $(x)\mathfrak{A}(x)$ provided that the formula $\mathfrak{A}(\mathfrak{z})$ can be deduced in $\mathfrak{F}$ for all numerals $\mathfrak{z}$, and if the formalism is sufficiently comprehensive to contain the formalism of number theory (or an equivalent formalism), then it is possible to state a formula with the property that neither it itself nor its negation is deducible.[48] Thus, under these conditions, the formalism $\mathfrak{F}$ does not have

46[2] K. Gödel, "The completeness of the axioms of the functional calculus of logic" (*vide* [?]).

47[3] A sentence is meant which can be represented in the formalism of number theory without free variables.

48[1] Moreover this formula has the special form

$$(x)(\varphi(x) \neq 0),$$

where $\varphi(x)$ is a function defined by elementary recursion, and the non-deducibility of this formula as well as the correctness and deduciblity of the formula $\varphi(\mathfrak{z}) \neq 0$ for every the property of deductive completeness (in the sense of Hilbert's claim for the case of number theory).[49]

Even before this result of Gödel was known Hilbert already had given up the original form of his problem of completeness. In his talk "The founding of elementary arithmetic"[50] he treated the problem for the special case of formulas of the form $(x)\mathfrak{A}(x)$, which do not contain any bound variables other than x. He modified the task by adding an inference rule which says that a formula $(x)\mathfrak{A}(x)$ of the kind under consideration can be always taken as a basic formula if it is possible to show that the formula $\mathfrak{A}(\mathfrak{z})$ represents a true statement (according to the elementary arithmetic interpretation) for all numerals $\mathfrak{z}$.

With the addition of this rule the result follows very easily from the fact that if a formula of the special form under consideration is consistent, then it is also true under the contentful interpretation.[51]

The method by which Hilbert enforces, so to speak, the positive solution of the completeness problem (for the special case that he considers) means a deviation from the previous program of proof theory. In fact, the requirement for a complete formalization of the rules of inference is abandoned by the introduction of the additional inference rule.

One does not have to regard this step as final. But in light of the dif-

given numeral $\mathfrak{z}$ follows already from consistency in the ordinary sense without the more restricted requirement mentioned above.

49[2] A different kind of incompleteness has been shown recently by Th. Skolem for the formalism of number theory ("On the impossibility of a complete characterization of the number series by a finite axiom system" (*vide* [?])). The formalism is not "categorical" (the term is used in analogy to O. Veblen's expression), as it is possible to state an interpretation of the relations $=, <$ and of the functions a', $a+b$, $a \cdot b$ in relation to a system of things (they are number theoretic functions)—using *tertium non datur* contentually for whole numbers—, such that on the one hand every number theoretic theorem that can be deduced in the formalism of number theory remains true also for that interpretation, but on the other hand that the system is by no means isomorphic to the number sequence (with regard to the relations under consideration), but that it contains in addition to the subset that is isomorphic to the number sequence also elements that are *greater* (in the sense of the interpretation) than all elements of that subset.

50[3] Held 1930 in Hamburg (*vide* [?]).

51[1] Hilbert had already mentioned earlier this fact in his second Hamburg talk "The foundations of mathematics" (*vide* [?], p. ■). There he used it to show that the finite consistency proof for a formalism also yields a general method for obtaining a finite proof from a proof of an elementary arithmetical theorem in the formalism, for example of the character of Fermat's theorem.

ficulties that have arisen with the problem of consistency, one will have to consider the possibility of widening the previous methodical framework of the metamathematical considerations.

This previous framework is not explicitly required by the basic ideas of Hilbert's proof theory. It will be crucial for the further development of proof theory if one succeeds in developing the finite standpoint appropriately, such that the main goal, the proof of the consistency of usual analysis, remains achievable—regardless of the restrictions of the goals of proof theory that follow from Gödel's results—.

During the printing of this report the proof for the consistency of the full number theoretic formalism has been presented by G. Gentzen,[52] using a method that conforms to the fundamental demands of the finite standpoint. Thereby the mentioned conjecture about the range of the finite methods (p. 180) is disproved.

[52]This proof will be published soon in the *Mathematische Annalen* (*vide* [**?**]).

Chapter 13

Bernays Project: Text No. 15

Theses and remarks on philosophical questions and on the situation of the logico-mathematical foundational research (1937)

Thesen und Bemerkungen zu den philosophischen Fragen und zur Situation der logisch-mathematischen Grundlagenforschung

(*Travaux IX Congrès International de Philosophie*, VI, pp. 104–110; repr. in *Abhandlungen*, pp. 79–84)

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Abstract. – I. *Scientific philosophy* and *logical syntax*. Necessity of an interpretation. – II. *Logic* and *mathematics*. The Kantian "analytic"-"synthetic" distinction is replaced by a distinction between "formal" and "objective." Concerning mathematics and logic we focus especially on the objective side: in mathematics

it consists in the existence of mathematical results independent of any formulation as a proposition and in the verificability of the arithmetical laws, in logic it consists in the hidden relation between expressions and principles and certain traits of reality. – III. *Arithmetic* and *geometry* are distinguished in respect to considerations of what is discrete and what continuous. Formal precision of intuitive mathematical concepts. – IV. *On the problematic of the foundations.* Reflections and remarks concerning the current state of research.

I. Philosophy and syntax

1. Scientific philosophy consists of fundamental considerations of the organization resp. reorganization of the language of science and considerations concerning the possible fundamental interpretations and conceptions of the scientific enterprises.

2. Syntax, as it is developed in Carnap's book *Logical Syntax of Language*[a] following Hilbert's meta-mathematics, the investigations by the Polish logicians, and those by Gödel on formalized languages, considers the mathematical properties of formalized languages of science.

3. If the syntax is to contain assertions, it must take place in an interpreted language.

If a formal definition is to serve to make a philosophical concept formation precise, then either the formal definition has to be provided with an interpretation or this more precise rendering is achieved indirectly by demanding a syntactic property of the formal definition which itself has then to be determined in a way that can be interpreted.

4. That a formal language functions as a syntax-language using, for instance, Gödel's method of arithmetization, is based on the intuitive-concrete validity of arithmetic.

II. Logic and mathematics

its general formulation suffers from fundamental problems, the introduc-

[a] *Vide* [**?**].

tion of a different kind of distinction recommends itself, a distinction between "*formally*" and "*objectively*" *motivated elements* of a theory, i. e., between elements (terms, axioms, modes of inference) that are introduced for the sake of the elegance, the simplicity, and the rounding off of the system, and those that are introduced with regard to the matters of fact of the domain in question.

Remark. This distinction admittedly does not yield a sharp classification, since formal and objective motives can overlap.

2. Systematic logic forms a domain of application for mathematical considerations. The connection between logic and mathematics in the systems of logistic corresponds to that of physics and mathematics in the systems of theoretical physics.

3. What is mathematical can not only be found in connection with the sentential formalism of logic, rather we find mathematical relations also in intuitable objects; in particular, we find mathematical relationships in all domains of the physical and the biological.—The independence of the mathematical from language has been emphasized in particular by Brouwer.

4. We must acknowledge that numerical relations express objective facts. This becomes particularly clear in syntax: e. g., if a formula A is derivable in a formalism F, then this is a fact which as such can be exhibited and verified explicitly. On the other hand, this derivability is represented in the language of syntax by a numerical relation.

We also have a way of checking arithmetical statements of general form, e. g., the statement that every whole number can be represented as the sum of four or fewer squares can be confirmed in a sense analogous to physical laws, except that in the former case one is confronted with a computational situation and in the latter case with an experimental one; in both cases a particular result to be obtained is predicted by the law.

5. In both the logic of ordinary language and symbolic logic we have formally and objectively motivated elements side by side. An objective motivation is present in so far as the logical terms and principles refer in part to certain very general characteristics of reality. In particular, Paul Hertz has pointed out this objective side of logic. F. Gonseth also speaks of logic as a general "théorie de l'objet."

On the other hand, the fact remains that the scope and the problems of logic are oriented according to certain basic features of the structure of language.

III. On the question of mathematical intuition

intuition is afflicted with various questionable additional aspects. We can leave aside all these additions, such as the claim that the intuition of space and time is required for physics and the distinction between "sensible" and "pure" intuition, and still acknowledge, however, that spacial relationships can be represented in an intuitive mathematical way, and we can, at least to a certain extent, read off the properties of configurations, as it were, from their intuitive representation. The kind of imagery involved does not have to be fundamentally different from that which a composer uses in the domain of sounds when he calls up combinations of tones in his imagination.

2. It is advisable to distinguish between "arithmetical" and "geometrical" intuition not according to spatial or temporal moments, but with regard to the distinction between discrete and continuous. Accordingly, the representation of a figure that is composed of discrete parts, in which the parts themselves are considered either only in their relation to the whole figure or according to certain coarser distinctive features that have been specially singled out, is arithmetical; furthermore, the representation of a formal process that is performed with such a figure and that is considered only with regard to the change that it causes is likewise arithmetical. By contrast, the representations of continuous change, of continously variable magnitudes, moreover topological representations, like those of the shapes of lines and plains, are geometrical.

3. The boundaries of what is intuitively representable are blurred. This is what has led to the systematical sharpening of the arithmetical and geometrical concepts that are obtained by intuition, as it has been done in part by the axiomatic method, in part by the introduction of formally motivated kinds of judgments and rules of inference. What is methodically special in this case is that the formally motivated elements that were to be introduced had already been provided largely by logic, like the principle of *tertium non datur*, which is synonymous with the assumption that every statement can be negated in the sense of a strict contradictory opposite; and in addition the objectification of the concepts (predicates, relations) and extensions of concepts.

Remark. It is noteworthy historically, that in Aristotelian logic the *tertium non datur* is nowhere required in the well-known 19 modes of inference, because the general affirmative judgment must be understood as asserting the existence of objects that fall under the concept of subject. (Note the rule *ex mere negativis nihil sequitur*[b] from this point of view.)

IV. On the problematic of the foundations

applied in analysis and set theory has been opposed by some mathematicians, as is well known, from the very beginning. In its most distinctive form this opposition has the goal to replace the usual method of introducing formally motivated elements by one that is performed completely within the framework of arithmetical evidence; geometric intuitiveness is to be eliminated and, on the other hand, all abstract concept formations and modes of inference that do not possess arithmetical intuitiveness are to be avoided.

2. The grounding of a substantial part of existing mathematics that was begun by Kronecker and has been carried out by Brouwer according to the goal (of a mathematics aiming at arithmetical evidence) mentioned in 1. has not converted the mathematicians to accept the standpoint of arithmetical evidence. The reasons for this may be the following:

a) Those who are looking for intuitiveness in mathematics will feel the complete elimination of geometrical intuition to be unsatisfying and artificial. In fact, the reduction of the continuous to the discrete succeeds only in an approximate sense. On the other hand, those who are striving for sharp concepts will prefer those methods that are most beneficial from the systematic standpoint.

b) In Brouwer's method, distinctions are introduced into the language of mathematics and play an essential role, whose importance is only apparent from the standpoint of the syntax of this language. That the *tertium non datur* is invalid, as Brouwer claims, can only be stated as a *syntactic* matter of fact, but not as one of mathematical objectiveness itself.

Comment. Brouwer's idea of characterizing the continuum as a set of choice sequences is in itself independent of the rejection of the *tertium non datur*. Certainly no *tertium non datur* can hold with regard to indefinite predicates of choice sequences. But one could nevertheless choose a standpoint such that the *tertium non datur* is retained for number theoretic properties of lawlike sequences. In this manner one would obtain an extension of Weyl's theory of the continuum of 1918.

[b]Translation: nothing follows from only negative (judgments).

3. The standpoint that Hilbert adopts in his proof theory is characterized by the fact that it meets both the requirements of formal systematic and those of arithmetical evidence. As a means to unify these goals he employs the distinction between mathematics and meta-mathematics, which is modeled after the Kantian partitioning of philosophy into "critique" and "system." As is well known, the main task that Hilbert assigns to meta-mathematics as a critique of proof is to show the consistency of the usual practice of mathematics. The problem is intended to be tackled in stages.

In the course of accomplishing this task, however, considerable difficulties arise, which are in part unexpected. An essential reason for difficulties which have not yet been overcome is that the difference between a formalism of intuitive arithmetic and that of usual mathematics is greater than Hilbert had presumed.

In the formalism of number theory the *tertium non datur* can be eliminated in a certain sense. The proofs of the consistency of the number theoretic formalism by Gödel and Gentzen are based on this fact. But as soon as one passes over to number-*functions* such an elimination is no longer possible. This follows in particular from a theorem which has been proved by S. C. Kleene after the concept of a "computable" function had been made more precise; it says that there are number-functions which are definable with the symbols of the number theoretic formalism (including a symbol for "the smallest number x that has the property $\mathfrak{P}(x)$"), but which are not computable.

Comment.—The concept of a computable function was made more precise in two independent ways: using the concept of a "generally recursive" function due to Herbrand and Gödel and by Church's concept of a "λ-definable" function; both concepts have been shown to be co-extensional by A. Church and Kleene.

4. While the task of a consistency proof for analysis is still an unsolved problem, in a different direction, namely in the domain of untyped formalisms of combinatory logic, proofs of consistency have succeeded. The theory of "combinators" which has been formulated by H. B. Curry, following Schönfinkel, is such an untyped calculus; and so is the theory of "conversions" established by Church. Both these formal theories, whose close connection has been shown by J. B. Rosser, yield a far-reaching and logically satisfying formalism for definitions. The consistency of operating with combinators (in the sense of unambiguousness) has been proved a while ago by Curry, that of the formalism of conversions recently by Church and Rosser.

The untyped combinatory formalisms also yield a new suggestion clue as to how systems of logistic may be constructed. An integration of these domains may perhaps lead to a reform of the whole of logistic. To be sure, an adequate approach to such an integration is not available yet.

Chapter 14

Bernays Project: Text No. 15b

Considerations on the Principles of Epistemology (1937)

Grundsätzliche Betrachtungen zur Erkenntnistheorie

(*Abhandlungen der Fries'schen Schule* NF 6, issue3/4 (1937), pp. 279–290.)

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In epistemological discussions, two doctrines oppose each other: that of *a priori* knowledge, and that of exclusive empiricism. The *a priori* view is characterized by the claim that we possess knowledge about nature that is originally contained in reason but comes to actuality only through sensory stimulation. This knowledge, when brought to full consciousness, can be expressed in the form of general laws in a definite way. This doctrine furthermore claims that those general laws that are knowable *a priori* include the principles of the exact natural sciences and that, in particular, the method of the construction of physical theories is determined by them in an unambiguous and definite way, so that, after having found these principles, no further development of theoretical physics occurs in any essential sense.

Thus, according to Kant, classical kinematics constitutes the necessary framework for all of physics. Kant also regards the principles of Newtonian dynamics as final principles of physics, and in this way the task of research in physics is restricted to finding mechanical models for explaining the different phenomena.

(There are even further restricting conditions which, according to Kant, can be inferred: thus, e. g., that each fundamental force has to be a central force, and also that there must be immediate action at a distance.)

In any case, in this extreme form the *a priori* doctrine cannot be brought into harmony with today's physics. To adopt it, one must either reject the ideas of today's physics in principle, or one must weaken the *a priori* standpoint by giving the principles maintained to be *a priori* valid such a liberal interpretation that they become compatible with present-day physics.

The former attitude appears to be a doubtful dogmatism. The following reasons, however, speak against the other procedure.

1. Even if the formulation of the principles can be maintained using a liberal interpretation, by doing so one will, for the most part, loose an essential element of the persuasiveness of the principle.

Thus, for instance, the principle of the conservation of substance is connected to the idea that substance is that of which a concrete thing consists. If one now interprets this principle so that it only expresses the validity of conservation laws, then the idea is surrendered, and the principle has no *a priori* persuasiveness at all.

We can illustrate this state of affairs with the law of the conservation of electric charge. As a consequence of the idea of substance this law would have to say that the positive and the negative charge are preserved individually. According to today's physics, however, the law is valid only in the sense that the algebraic sum of positive and negative charge remains constant. This is certainly a conservation law as well, but it has nothing to do with the idea of substance and has no *a priori* plausibility at all.

2. The possibility of retaining the wording of a principle in the face of changes in basic physical attitudes depends on the particular property of the (at the time) new theories, and it can hardly be assumed as certain in advance that it is always possibile to preserve the formulation.

In view of this situation, one seeks a philosophical view that releases us in a radical way from the necessity of retractions or unsatisfying defenses.

An extreme empiricism aiming at completely reducing science to the immediate data of perception presents itself as such a radical standpoint. According to this view, once one discards all the unnecessary and doubtful components, science consists of nothing else than an arrangement and combination of sense data according to the criterion of greatest possible clarity.

One should, however, point out against this position that mere classifications of sense data do not immediately result in objective states of affairs and connections. The mental process that leads from immediate sense data to the determination of objective facts is anyway not so simple. This has been emphatically asserted by Kant, and we must agree completely with him in this case.

Moreover, such extreme empirism is totally incapable of making the method of testing scientific claims by means of new experiments intelligible. Especially the fact that very small effects of observation can cause a revolutionary change in scientific theories shows how far the procedure of natural science is from a mere registering of sense perceptions.

A moderate empiricism takes these facts, which speak against extreme empiricism, into account. On the one hand, it presupposes as given the kind of objectivity with which we deal in everyday life, but also in experimenting. Furthermore, it does justice to the essential role of the assumptions by means of which statements are conjectured which, according to their form, make claim to universal validity.[1]

However, a moderate empiricism of this kind leaves open the epistemological questions concerning, on the one hand, the formation of the everyday view of nature (the "morphological world view," according to the designation by Fries and Apelt) and, on the other hand, the formation of hypotheses and theories.

In this way we are led back to our previous formulation of the problem: to look for a philosophical position concerning empirical knowledge which fundamentally excludes the conflicts with the progressive scientific conceptions to which we are led by the Kantian theory of *a priori* knowledge. We can formulate the question somewhat more precisely as follows: is a radical detachment from such restrictions, as they follow from Kant's apriorism for the methodology of science, compatible with the preservation of the essential ideas of the Kantian critique of reason?

This formulation of the problem suggests a separation between two es-

[1]Most scientifically oriented philosophers today advoate a moderate empiricism. Rudolf Carnap, who initially maintained an extreme empiricism, has recently turned towards a moderate empiricism.

sential aspects in the conception of Kant's theory of experience: the idea of considering our empirical knowledge not as a mainly receptive procedure, and also not as an immediate observation, but rather as a product of our mind stimulated by sense impressions; and on the other hand, the assumption that in this product of the mind everything essential is determined by invariable fundamental properties of the mind.

This last assumption comes from the fact that Kant's conception of his theory was guided by the following consideration: the principles of the exact sciences are knowledge *a priori*. As such, they are understandable, however, only if they express conditions of the possibility of experience. At work here is, on the one hand, the conviction of the *a priori* epistemological character of the principles of geometry and mechanics, i. e., exactly the aspect that we had considered as problematic, and furthermore, the view that there could not be knowledge *a priori* of how things that are independent of us are "in themselves," the argument that constitutes "formal idealism" as it is called by Fries in his criticism of it. This Friesian criticism is correct. Regardless of it, however, Fries upheld the essentials of the Kantian theory, and indeed almost strengthened the subjective turn in epistemology. Like Kant, he was concerned with understanding the standpoint of classical mechanics, which he also took to be the final scientific view of nature, as philosophically necessary, and at the same time tried to differentiate it, in its jurisdiction, from the religious world view. Both goals seemed to have been realized most successfully by Kant's change of perspective in his notion of the "Copernican revolution."

If we now allow the principles of Newtonian mechanics not to be *a priori* knowledge, then we give up the Kant-Friesian formulation of the problem, and we will—while keeping the idea of the productive role of mental activity in the knowledge of nature—replace the extreme position according to which "intellect prescribes nature its laws" with a more unprejudiced one.

Such an unprejudiced position seems to be given in the first place through the doctrine of mathematical knowledge and its relation to physics. It is obvious that the laws of geometry go beyond what can be determined by or inferred from observations. On the other hand, a view which ignores the essential role played by our experiences of the motion of rigid bodies for the formulation of the axioms of geometry cannot be satisfactory (as already shown in particular by von Helmholtz). We can do full justice to the special character of the intuitive formation of ideas in geometry (i. e., formation guided by intuition) without in the process excluding the very plausible thought that this formation of geometrical ideas takes place in connection with the mental processing of basic observations given by the handling of rigid bodies. Furthermore, it must absolutely be granted that the idea of space, and more so the idea of time, constitutes a form of our intuition, and that it cannot be reduced to sensations and concept formations. The recognition of this state of affairs by no means forces us to assume that physical spatiality and temporality are only derivable from our forms of intuition, and that their lawfulness is determined by these forms of intuition.

In freeing ourselves from this presupposition, physics gains a considerable freedom of speculation; the narrow mechanistic framework is replaced by the framework of the mathematical as such. Accordingly we can conceive the task of physics generally as enquiring into the facts of nature with respect to how far mathematical laws can be discovered in them, and how far through such laws a homogeneous understanding of the connections becomes possible.

In a certain sense we come back in this way to the old program of the Pythagoreans. Admittedly we have to aviod hypostatizing the mathematical in a mystical way, as they supposedly did. According to its nature the mathematical cannot be the actual itself but only something connected to the actual.

On the other hand, we are not prevented from acknowledging that this element of the mathematical can be found in reality, even independently of our cognitive constitution. Therefore we also need not understand the doctrine of a "division of truth under different worldviews" (according to an expression of Apelt) as reducing the significance of physical knowledge. Such a limitation of validity is unavoidable if mechanistic physics is taken as a basis, because of the claims of exclusiveness and completeness inherent in the mechanistic view of nature. For our view of physics, in contrast, in which only the mathematical form of concept formation and of the connection counts as a general characteristic feature, but not the carrying out of a specific view of nature taken as a basis, those claims become invalid.

As a further consequence of this way of looking at things, it turns out that the naive view—we will briefly call it our "ordinary view of nature"[2]—gains in importance. In the Kantian philosophy, and also in Fries, it appears as a simple preliminary stage of the scientific view. In dropping the assumption

[2]The expression "morphological world view" is a bit misleading because it evokes the understanding that the characteristic feature of this standpoint can be found in its restriction to shapes.

of a specific physical view of nature, our ordinary view of nature gains the role of a fixed starting point to which even theoretical research has to return again and again in experimentally motivating its concept formations and assumptions. In particular, this ordinary view of nature has the following characteristics.

1. In it the complete constitution of the idea of object is already carried out; it contains therefore also the intuitive geometrical representation and the intuitive "construction" of the spatial order of objects, as well as everything that is necessary for handling things in experiments.

2. It encompasses all those concept formations for describing and explaining the external and the internal world which are laid down the ordinary colloquial language. In particular fundamental concepts like matter, life, consciousness, cause, chance, etc. find an unproblematic application.

3. In it there are neither reductions (e. g., from the qualitative to the non-qualitative), nor isolations of domains of objects. Everything given is regarded as connected. The heterogeneity of the material and the mental does have detrimental effects because the connections are pursued only insofar as they present themselves empirically. Nor does the relation of sense qualities to perception and the resulting illusions cause fundamental problems for this view; everywhere the concept formation and the language adapt to the given circumstances. (We say, e. g., "this dress looks yellow in daylight," or "this piece of cloth feels soft.")

well into the ordinary view of nature. Some philosophers do not grant the possibility of transcending our ordinary view of nature by physics at all. In this sense, Ernst Mach, for example, was opposed to atomism.

The tendency to such a restriction to the framework of our ordinary view of nature is very understandable, since that view brings with it the advantages of intuitiveness and formal coherence. On the other hand, we have to realize that the coherence, however important it might be for our practical life and for our emotional disposition towards the world, nevertheless has a perspectival nature comparable to the unity of a landscape. And we must furthermore recognize that the procedure adopted by speculative physics, when it goes beyond the ordinary view of nature, is a consistent continuation of the methods by which we achieve our objective grasp of the world around us and our knowledge of causal connections, already within the ordinary view of nature. We shall demand of a philosophical conception of knowledge of nature that it account for the basic methodological conformity of the process of physics, both in its early stages and in the newer speculative physics.

If we look for a suitable epistemological standpoint with respect to this task, the following complementary aspects appear on the basis of the former considerations.

1. The standpoint has to be chosen in such a way that it grants research the necessary speculative freedom. The activity of research should not be regarded as a mere application of a fixed schema in advance but as a continually renewed intellectual production.

2. On the other hand, speculative freedom cannot be understood as arbitrariness; one must do justice to the rational element in research, which presents itself to us especially in the complete and fully developed parts of physics. The formation of a new physical view must be understood as an interpretation in which reason, so to speak, reacts to a given situation of experience; whereby, in each case, the interpretations obtained in earlier stages of research, in so far as they have proven to be successful and have become fixed, appear as something belonging to the situation.

According to such a conception we are admittedly not in a position to determine the contribution made by reason in the form of *a priori* principles to empirical knowledge. At best one can be successful in characterizing it by formulating regulative maxims of research; but this is doubtful as well.

In any case, however, we consider rational interpretation to be an essential element in the development of empirical science—of course, not in those specious proofs (which are in a bad sense rationalistic and which Mach justly criticizes) where, in a situaiton where experimental experience is needed, one instead tries to obtain a result by a clever deduction, but rather in the heuristic mode of thought and wherever one introduces new interpretative general concepts, thereby preparing the ground for new types of understanding. Examples of such general concepts are found in the idea of atomism; in the method of explaining regularity with the help of the concept of probability; in the modification of the concept of matter with the help of the concept of field; in the introduction and application of the concept of energy. Furthermore, examples are also found that make possible the integration of different fields to a unified theory: the integration of the phenomena of gravity and astronomical processes of motion; the integration of optics and electrodynamics; the integration of geometrical mass measurements and phenomena of inertia with gravity; and finally the latest conception of wave and corpuscular phenomena as two aspects of one and the same reality.

If we compare the view presented here with the two antagonistic opinions of pure apriorism and pure empiricism described at the outset, we find that it differs from these opinions by dropping a presupposition common to both, namely, the presupposition that reason, insofar as it is important in empirical knowledge, would have to play a role through *a priori* knowledge. We can represent this connection, following Leonard Nelson, with the help of a logical schema:

■■■

- Dogmatic assumption A: if reason is essential for physical knowledge it must play a role through principles that are recognized *a priori*.
- Fact F_1: The rational element is not dispensable in research in physics.
- Fact F_2: There are no *a priori* determined principles in physics.
- Apriorist consequence of F_1 and A: There are *a priori* recognizable principles of physics.
- Empiricist consequence of F_2 and A: The rational element is dispensable in physics.
- Solution: Reason plays a role in physical research, not through *a priori* principles, but in the progress of concept formation and explanatory methods.

On closer inspection, abandoning traditional rationalism in this way proves to be not only compatible with acknowledging the significance of the rational, but also favorable to it. Kantian philosophy resulted in a devaluation of the scientific view of nature as a consequence of its restriction of natural research with respect to its method and its validity.

Schiller facetiously sums up the Kantian view as follows: “In the theoretical field there is nothing more to find.”

We will do better justice to the significance of the rational by not treating as final a specific temporal conception of nature, but rather by accounting for the kind of development that occurs when a creature mentally confronts its environment, as well as all other living things.

Chapter 15

Bernays Project: Text No. 16

On the Current Question of Method in Hilbert's Proof Theory (1938/1941)

Über die aktuelle Methodenfrage der Hilbertschen Beweistheorie

(Manuscript from the *Nachlass*)

Translation by: *Bernd Buldt (& Gerhard Heinzmann)*
Revised by: *CMU*
Final revision by: *Bill Tait*

My report on the current situation of Hilbert's proof theory comes with principled observations. At the outset I remark that, concerning the statement on the existing situation, the views presented here cannot claim to represent without qualification the standpoint of Hilbert's school.

The combination of principled observations with the exposition of the current situation of proof theory is provoked by this situation itself. As you probably know, proof theory has recently suffered from a kind of crisis, and some have already declared the Hilbertian enterprise as almost foundered.

This assessment of the situation is based on the circumstance that the program as Hilbert proposed it for proof theory in his publications from 1922–1927 is, to all appearances, in need of revision, namely, in respect to the methodical standpoint to be assumed.

Technically speaking, it concerns the following: For the metamathematical reasonings one needs stronger methods of inference than those Hilbert originally thought he could confine himself to in the sense of his "finitary attitude." This need was felt already on occasion of a problem, which was thought to be already solved: the demonstration of the consistency for the full arithmetical formalism.

In connection herewith it became also clear, that the finitary standpoint as intended by Hilbert is not—as it first had seemed—on par with Brouwer's intuitionism. Gödel could show that, within the realm of the number theoretical formalism and with help of a rather simple interpretation, all modes of inference of classical mathematics can be transformed into intuitionistically admissable modes of inference. Hence the consistency of the number theoretical formalism follows thereby directly from the standpoint of intuitionism.

Here we call the number theoretical formalism that formal deductive system, which is obtained from the logical calculus of first order (called "predicate calculus" or "restricted functional calculus"), the axioms of equality, the number theoretical axioms:

$$a' \neq 0, \quad a' = b' \;\rightarrow\; a = b$$

(a' denotes the number succeeding a)
as well as the schema of complete induction and the elementary recursive definitions. (The notion of the least number of a certain property, which occurs in number theoretical deductions, can be avoided in the investigation into consistency by the elimination procedure for the notion "that, which.")

This formalism exceeds already a little what is absolutely necessary to formalize the number theoretical proofs. In fact, as Skolem was first to show, for this purpose a more restricted formalism of "recursive number theory" suffices, which is still capable of a direct finitary interpretation.

The number theoretical formalism here considered differs from recursive number theory as well as from intuitionistic number theory by the unrestricted employment of the notions "all" and "there is."

However, in the domain of the inferences which admit of representation in the number theoretical formalism, an agreement can be established between the adherent of the usual mathematical standpoint (who regards as legitimate all these modes of inference) and the intuitionist (who does not in general acknowledge the principle of excluded middle). This can be accomplished in the following manner: The first has to declare that a proposition "there is a x such that $\mathfrak{A}(x)$ holds" should merely be another mode of expressing that in any case the opposite of $\mathfrak{A}(x)$ does not hold for all x. Likewise, a proposition "$\mathfrak{A}$ or $\mathfrak{B}$" should say nothing else than that not both, the opposite of $\mathfrak{A}$ and the opposite of $\mathfrak{B}$, hold. With this interpretation of the existential judgement and the disjunction, the intuitionist must acknowledge as legitimate all modes of inference in the mentioned domain of classical mathematics—at least, if she accepts the rules of intuitionistic inference devised by Heyting.

Now, this discovery that the intuitionistic modes of inference in number theory are so close to the "classical" ones, results, on the one hand, immediately in a demonstration of the consistency of the number theoretical formalism from the standpoint of intuitionism. On the other hand, this discovery shows that the intuitionistic standpoint differs essentially from the finitary. In particular, one will note the following difference as to general propositions (propositions of general form): While intuitionism only contests the application of the law of the excluded third to such [**general**] propositions, the finitary standpoint avoids, in principle, the negation of general propositions as well as their employment as premisses in conditional sentences.

A negation of a proposition has a finitary meaning only if it is equivalent to a claim with positive contents. Thus, e. g., the negative proposition "the numeral $\mathfrak{a}$ is not identical with the numeral $\mathfrak{b}$" denotes the same as the positive claim that the numeral $\mathfrak{a}$ is different from the numeral $\mathfrak{b}$. And a condition or an assumption is finitary only if it has as its content $_a\underline{\text{either}}_a$ an intuitively determined configuration or an intuitively determined operation (respectively the result of such operations). Thus, e. g., the assumption $_{c_1}$**that**$_{c_1}$ Fermat's $_{c_1}$**last**$_{c_1}$ theorem is true is not finitary. The assumption, however, that $\mathfrak{a}, \mathfrak{b}, \mathfrak{c}, \mathfrak{n}$ are four positive integers (numerals) such that $\mathfrak{n} > 2$ and $\mathfrak{a}^{\mathfrak{n}} + \mathfrak{b}^{\mathfrak{n}} = \mathfrak{c}^{\mathfrak{n}}$—i. e., the assumption that the four numbers $\mathfrak{a}, \mathfrak{b}, \mathfrak{c}, \mathfrak{n}$ provided a counterexample to Fermat's great theorem—is finitary. $_{c_1}$**Furthermore, the assumption that this theorem is deducible in the formalism of number theory is finitary**$_{c_1}$ in the following sense: One assumes as given a figure of formulae—with a terminal formula representing Fermat's theorem—having the properties of a deduction within the number theoretical formalism. The assumption, however, that some intuitively compelling proof of Fermat's great theorem is given, is not finitary.

Negations and hence the negations of general propositions in particular can of course be eliminated in intuitionism. By an arbitrary choice of an elementary false proposition, e. g., $0 = 1$, one is able to interpret the negation $\overline{\mathfrak{A}}$ of a proposition $\mathfrak{A}$ by $\mathfrak{A} \to 0 = 1$ ("the assumption $\mathfrak{A}$ results in $0 = 1$"). With this interpretation, the intuitionistic modes of inference which employ negation transform themselves into intuitionistically admissable inferences. But the elimination of negation thus gained is only apparent, in that we find ourselves forced to operate with unreal conditional sentences. That is, implications $\mathfrak{A} \to \mathfrak{B}$ occur, which are to be interpreted in an unreal sense: "Suppose $\mathfrak{A}$ held, $\mathfrak{B}$ would result." In fact, those indirect arguments are used not only for elementary propositions $\mathfrak{A}$—for which they are admissable in finitary reasoning as well—$_{c_1}$**but in an essential way also for general sentences and for implications with general (or logically even more complex) sentences as premisses**$_{c_1}$.

In any case, the use of the notion "absurdity" for arbitrary propositions remains an essential means for intuitionistic reflections.

Now, considering the fact that the finitary standpoint has proven to be too narrow for proof theory, the following question occurs: Is it necessary to take over all the methodic presuppositions of intuitionism?

At the moment, we can give at least a partial answer to this question. For Gentzen has delivered a consistency proof for the number theoretical formalism, whose methodical requirements constitute a kind of intermediate link between the finitary standpoint and the standpoint of intuitionism.

It recommends itself to refer to the newer version (given by Gentzen) of his proof. For, in comparison to the version first published, it has not only the advantage that here the proof idea is made perspicuous, but also the advantage, that certain methodical complications of the first proof become unnecessary.

Recently, Gentzen's newer proof has again been simplified by Kalmár, where it turned out in particular that one can dispense with Gentzen's transformation of the number theoretical formalism into a certain equivalent calculus.

Let me shortly outline the logical schema of Gentzen's consistency proof in respect to the way the finitary standpoint is transcended—(with certain insignificant deviations from Gentzen's presentation).

According to a remark already used in the previous consistency proofs, to assert the consistency of the number theoretical formalism comes to the same as to assert that in this formalism the formula $0 = 1$—which we indicate by "$\mathfrak{f}$"—is not deducible. That is to assert that each deduction within this formalism has a terminal formula different from $\mathfrak{f}$.

One can realize in a direct way that this assertion is true for those deductions in which neither complete induction nor the rules for "all" and "there is" are employed—which we call, for short, "elementary deductions."

For the general demonstration "ordinal numbers" are employed, taken from a domain of Cantor's first und second number class (they are those below Cantor's first ϵ-number). The introduction of these numbers can be made in an independent way, i. e., without recourse to Cantor's theory: The respective ordinal numbers can be characterized as certain (finite) figures, for which one can define, intuitively, a "smaller than" relation—with the properties of a well-ordering—in such a way, that for two different ordinal numbers it is always decidable which one of the two is the smaller one.

One then assigns, according to a simple calculating precept , to each deduction of the number theoretical formalism an ordinal number. Based on this assignment, one can determine for each non-elementary deduction another deduction with the same terminal formula but a smaller ordinal number. This results in the following: If each deduction with an ordinal number smaller than a certain ordinal number α has a terminal formula different from $\mathfrak{f}$, then the same is true of each deduction with the ordinal number α.

So far the proof remains within the framework of finitary reasoning. Now, to get from this consequence to the result that generally each deduction in the number theoretical formalism has a terminal formula different from $\mathfrak{f}$—which is the assertion to be proved—it is still necessary to justify the following principle of inference: "If a proposition $\mathfrak{B}(\alpha)$ about an ordinal number α holds for 0 (the least of the ordinal numbers), and if one can determine for each ordinal number α a smaller ordinal number β such that, whenever $\mathfrak{B}(\beta)$ holds, also $\mathfrak{B}(\alpha)$ holds, then $\mathfrak{B}(\alpha)$ holds for each ordinal number α." This mode of inference is in turn taken from the principle: "If a proposition $\mathfrak{B}(\alpha)$ about an ordinal number α holds for 0, and if it holds for the ordinal number α whenever it holds for each smaller ordinal , then it holds for each ordinal number."

This principle of inference is a kind of generalisation of complete induction. In set theory, a generalized induction of this kind is called "transfinite induction," because it extends to transfinite ordinal numbers. For our purposes, however, this expression is not appropriate. For we employ the word "finite" in a methodical sense and the difference between ordinary induction (inference from n to $n+1$) and transfinite induction does not at all coincide with the difference between finitary and non-finitary modes of inference. In general, an ordinary induction is finitary, only if the predicate (and whether it holds for a number) is elementary. On the other hand, there are (according to the usual terminology) transfinite inductions, which are still of a finitary character.

What matters for us here is not so much to fix the exact limit up to which inductions are finitary. Rather, it is to make clear to ourselves, from the intuitive standpoint, upon what the legitimacy of the principle of inference under consideration rests and in what way it constitutes a proper generalisation of the ordinary induction.

Let us recall how the finitary motivation for the ordinary induction proceeds: We have the assumption that $\mathfrak{A}(0)$ holds and that we can infer $\mathfrak{A}(\mathfrak{n}+1)$ from $\mathfrak{A}(\mathfrak{n})$. Because we can, by an iterated progression of 1 starting at 0, arrive at each finite number, we can likewise infer from $\mathfrak{A}(0)$ that $\mathfrak{A}(\mathfrak{n})$ holds for each finite number n.

Now, the ordering of the ordinal numbers under consideration is analogous to that of the ordinary number series. This holds insofar that also the former has the property of a well-ordering—every initial segment has an element which immediately succeeds it—and, even more, the order type of this well-ordering can be reduced, in a recursive manner, to the natural order of the number series. Thereby an intuitive kind of "running through" is made possible. With reference to this, Cantorian set theory speaks of a "counting beyond the infinite."

This counting beyond the infinite does of course not mean to operate with the representation of an actual infinite. Rather, it means the transition from a progressive process to its metamathematical consideration. This transition is of the kind which takes place already in ordinary induction, with which we go beyond getting the particular propositions $\mathfrak{A}(0)$, $\mathfrak{A}(1)$, $\mathfrak{A}(2)$, …, by means of the general metamathematical observation that we can arrive at the proposition $\mathfrak{A}(\mathfrak{n})$ for all $\mathfrak{n}$.

While running through the order type under consideration, superposed inductions occur. That is, we obtain higher inductions from the ordinary induction by employing the metamathematical consideration to the processes of iterating inductions. Now, to this superposition of inductions corresponds, as the logical form of expressing it, a superposition of conditional sentences in which general sentences enter as premisses. But these are always those general sentences which are seen to be true by means of the [**mentioned metamathematical**] consideration, so that here the conditional form has the meaning of anticipating one stage in a progressive process of inference.

Hence, the use of the principle of transfinite induction under consideration amounts to an extension of the methodical framework of proof theory, though not to a complete acceptance of the intuitionistic modes of inference. The procedure of this extension is also capable of being generalized. For it is possible to intuitively master the "running through" even for well-order types higher than those employed in Gentzen's consistency proof (the ordinal numbers below Cantor's first ϵ-number) and to intuitively justify thereby the principle of transfinite induction related to this well-order type.

At the moment, there is no way to determine whether such a higher induction principle, taken as an additional means (i. e., added to the finitary methods), suffices for a consistency proof of analysis.

According to Gödel's general theorem on formally undeducible sentences, the induction principle in question—which would in any case be expressible as a theorem about a certain well-order of the ordinary numbers—had to be such that its proof cannot be formalizable within the framework of analysis. At first, it seems impossible to satisfy this requirement; for the general theory of well-orders of the number series, including the general theorem on transfinite induction, can be developed in the formalism of analysis. However, one has to keep in mind that the general theorem of transfinite induction does not determine whether a certain defined ordering of the number series is a well-ordering; and the higher principle of induction in question could just amount to such an assertion. –

Anyway, in view of these considerations it does not seem to be expedient to fix in advance the methodical framework for proof theoretical investigations. The expectation, that the finitary standpoint (in its original sense) would suffice for the whole of proof theory, was aroused by the fact that the problems of proof theory can already be formulated from this standpoint. But there is no simple relationship between the ability to express and to prove sentences and therefore neither between the ability to formulate and to solve problems.

But now the question arises: What, then, is the characterization $_{c_1}$**of**$_{c_1}$ the method$_{c_1}$**olog**$_{c_1}$ical limitation of proof theory, $_{c_1}$**if not the demand for that elementary**$_{c_1}$ evidence which distinguishes the finitary standpoint. The answer is as follows. The tendency to limit methods remains basically the same; but, if we want to keep us open the possibility of extending the methodical framework, then we must avoid using the concepts of evidence and security in too absolute a sense. On the other hand, we thereby gain the principal advantage of not being obliged to question as unjustified or doubtful the usual methods of analysis.

One has to regard what is generally characteristic of Hilbert's methodical attitude as the following: One puts here the stress on sticking to an, in the strict sense, arithmetical mode of thinking, while the usual methods of analysis and set theory are, for an essential part, inspired by geometrical ideas (especially by that of a point set) and draw their evidential force therefrom. In fact, one can say—and this surely is the main point of the finitistic and the intuitionistic critique of the usual procedure in mathematics—that the arithmetization of geometry in analysis and set theory is not complete.

The methodical orientation of Hilbert's proof theory can contribute to a forceful development of the specifically arithmetical mode of thinking and to bringing out more clearly the stages in the formation of arithmetical concepts.

As for the rest one has to emphasize, concerning the achievements of proof theory, that the demonstrations of the consistency of the number theoretical formalism in no way represent the only progress the metamathematical investigation of recent years has to show. Especially with regard to the questions of the decidability of problems and the effective calculability of functions, remarkable results have been achieved by the investigations of Gödel, Church, Turing, Kleene, and Rosser. Already today, metamathematics is left in a such shape that its appreciation is independent of any position taken on the philosophical questions of foundational research. –

Chapter 16

Bernays Project: Text No. 17

Introduction
(1939)

Zur Einführung

(*Grundlagen der Mathematik*, Bd. 2, Berlin: Springer, pp. vi–ix)

Translation by: *Dirk Schlimm*
Revised by: *CMU*
Final revision by: *Bill Tait*

The aim of this book is to provide a thorough orientation to the current content of Hilbert's proof theory. Despite the fact that the achievements to date have been modest in comparison to the goals of the theory, there are still plenty of suggestive results, viewpoints, and proof ideas that seem worth reporting on.

The purpose of the book has resulted in two main themes for the contents of this second volume. On the one hand, to present in detail the principal proof-theoretic approaches of Hilbert that follow from the ϵ-symbol together with their implementations.

A substantial part of the investigations presented here have not yet been published at all, aside from some brief hints. Thus, not only is there interest in the subject matter, but there is also a scientific obligation on the part of the Hilbert-school to justify the various previous announcements of proofs by actually providing these proofs. This demand is all the more pressing in this case, since there was initially (until the year 1930) some error about the scope of the proofs by Ackermann and v. Neumann, which resulted from one of the approaches of Hilbert mentioned above.

These hitherto unpublished proofs are now presented in detail in §§ 1 and 2. In particular, the restriction which is here still imposed on the consistency proof for the number-theoretic formalism is made clearly apparent.

With the help of one of the methods presented here, a simple approach to a series of theorems also arises by which the proof-theoretic investigation of the predicate calculus is satisfactorily rounded off and which also allows for remarkable applications to axiomatics. A theorem of theoretical logic that was first formulated and proved by J. Herbrand, for which we obtain a natural and simple proof by the mentioned route, lies at the center of these considerations.

The discussion of the applications of this theorem also offers the opportunity for consideration of the decision problem. Following this, a proof-theoretic sharpening of Gödel's completeness theorem is proved in § 4.

The second main theme is represented by the considerations leading to the necessity of extending the limits of the contentual forms of inference allowed in proof-theory, beyond the previous demarcation of the "finite standpoint." Gödel's discovery that every sharply delineated and sufficiently expressive formalism is deductively incomplete stands at the center of these considerations. Both of Gödel's theorems which express this fact are discussed in detail with respect to their relation to the semantic paradoxes, to the conditions for their validity, and the implementation of their proofs—Gödel only hints at the proof for the second theorem—and to their applicability to the full number-theoretic formalism.

The discussion regarding the extension of the finite standpoint is followed by consideration of Gentzen's recent consistency proof for the number-theoretic formalism. Of course, only what is methodologically novel in this proof is presented in detail and discussed, namely the application of a particular kind of Cantor's "transfinite induction."

The mainly external reason for not presenting the entire proof was that the newer, first really clear version of Gentzen's proof had not been published at the time of the printing of this volume. By the way, Gentzen's proof does not relate directly to the number-theoretic formalism discussed in the book. L. Kalmár recently succeeded in modifying this proof so that it became directly applicable to the number-theoretic formalism developed in our book (in § 8 of the first volume), whereby also certain simplifications arise.

W. Ackermann is currently extending his earlier consistency proof (presented in § 2 of the present volume) by applying the kind of transfinite induction that is used by Gentzen in order to make it valid for the full number-theoretic formalism.

If this succeeds—which seems quite likely—Hilbert's original approach would be rehabilitated with respect to its effectiveness. In any case, Gentzen's proof already justifies the view that the temporary fiasco of proof theory was merely due to the fact that methodological requirements had been imposed on the theory which were too strong. To be sure, the final decision about the fate of proof theory will be based on the task of proving the consistency of analysis.

A few considerations that are distinct from to the train of thought developed in §§ 1–5 of the present volume are added as "supplements." Two of these complement the considerations in § 5: Supplement II is about making precise the notion of computable function (as has been successfully implemented recently with various methods) and presents the facts related to this circle of problems, and can be easily developed following the remaining considerations of the book. A. Church's theorem about the impossibility of a general solution to the decision problem for the predicate calculus is applied in this connection. In Supplement III some questions pertaining to the deductive propositional logic are discussed, and it also contains additional remarks to the considerations about the "positive logic" formulated in § 3 of the first volume.

Various deductive formalisms for analysis are set up in Supplement IV, and it is shown how the theory of the real numbers and also that of the numbers of the second number class are obtained from them.

Supplement I contains an overview of the rules of the predicate calculus and its application to formalized axiom systems, as well as remarks about possible modifications of the predicate calculus, and a compilation of various definitions and results from the first volume.

In view of the already enormous amount of material, various proof-theoretic themes could not be addressed in this book: in particular, the topic of multi-sorted predicate calculus, which was dealt with first in Herbrand's thesis[a]

[a] *Vide* [**?**].

and recently in more detail by Arnold Schmidt (*vide* [**?**]).

Certain considerations that could be found in Hilbert's lectures and in discussions with Hilbert, but that only remained isolated remarks or that had not been sufficiently clarified, are not presented: in particular, the approaches regarding the definitions of numbers of the second number class by common (i. e., not transfinite) recursion, and those concerning the use of type symbols, in particular those that are introduced by explicit or recursive definitions.

The the present volume follows the first volume closely; this connection is also strengthened by frequent references to page numbers. On the other hand, the compilation of terms and theorems from the first volume given in Supplement I and the recapitulation in part b), section 1 is intended to render the reading of the second volume largely independent of the first one. The reader who is already somewhat familiar with logical formalization and with the questions addressed by proof theory will be able to follow the considerations of the second volume without knowledge of the first one.

In any case, it is recommended that the reader of the present volume start with § 1 of Supplement I. Furthermore, he should make use of the page references only when he feels the need to do so in the particular passages.

In addition to the remarks given in § 2 about possible omissions in the course of study, it may also be remarked here that the rather tedious section 2 of § 4 can be left out.

Regarding references to paragraphs, the numbers from 1 to 5 refer to the present second volume if nothing else is indicated, while the numbers from 6 to 8 occur only in the first volume.

Chapter 17

Bernays Project: Text No. 18

Some perspectives on the problem of the evident (1946)

Gesichtspunkte zum Problem der Evidenz

(*Synthese* 5, pp. 321–326;
repr. in *Abhandlungen*, pp. 85–91)

Translation by: *Charles Parsons*
Revised by: *Gerhard Heinzmann*
Final revision by: *CMU*

The importance for philosophy of the question of the evident is hardly put in doubt. But it does not always happen that justice is done to the complexity of the problem. Evidence is often regarded as a quality that can simply be attributed or not to an axiom, a principle, or a mode of reasoning, and then the problem of evidence seems simply to be to decide where evidence is in fact to be found.

This simple aspect of the problem arises particularly under the influence of the idea of absolute evidence, which is substituted for the empirical concept of *de facto* evidence.

Refraining from introducing here an a priori postulate, we limit ourselves to *de facto* evidence – in other words we do not take as point of departure the idea of an absolute guarantee of truth, but we are content to observe that there are certain cases in our judgments and reasonings where we find a satisfying purchase or a point of departure given by a direct representation (which sometimes comes up spontaneously and sometimes requires some effort of the imagination). The object of evidence in this sense can be an existence or a relation. We know the distinction made in this respect by Leibniz, Hume, and others.

In taking account of the concrete character of evidence, we are led to recognize that evidence, originating from a mental situation, is relative to the implicit suppositions that such a situation includes. Please note: the expression "relative" does not mean that there is here a sort of indifference of point of view. The mental situations at issue are those that knowledge passes through in its development, and it can very well happen that in reaching a position superior to a preceding one, we discover an implicit supposition that at the same time we find ourselves obliged to give up. In this way an evidence relating to a stage of the intellectual process can be lost at a more advanced stage.

In particular, this is the case for the evidence of outer sensation, such as it is found in the position of naive realism. At a more advanced stage we discover suppositions relative to that position that we must abandon because it turns out that:

1. Sensible qualities do not apply directly to reality;
2. The information that outer sensation gives us from objects does not have a character of immediacy.

It is known that the first theory to have taken account of this discovery is that of Democritus, revived by John Locke, a theory that introduces the distinction between real and apparent qualities. At this level a large part of the evidences of naive realism are still preserved. And one can say that it is the intention of the system of Kant to give a comprehensive philosophical interpretation to the situation as it thus presented itself.

As you know, there are theories that are opposed in a more radical way to naive realism: that of the phenomenalism of Mach and Avenarius and that of the philosophers of the school of Brentano, who deny completely the evidence of outer sensation, only recognizing the evidence of inner intuition. It seems that this opposition to naive realism goes too far; in fact it is certainly not an adequate description of the facts simply to contest the evident experience of a reality surrounding us – "reality" taken here in a sense still unexplicated, and it must be observed that this primitive evidence is not shaken at all by the criticisms to which naive realism is subjected.

Looking at the matter more closely, it seems that the superiority of inner intuition over outer sensation does not in the first instance relate to the moment of existence: the existence of our Ego has originarily scarcely more certitude and evidence than that of an external world. What constitutes the superiority of inner intuition is that the categories that it produces are immediately applicable to reality, which is not the case for those that derive from outer sensation. In fact it is evident that to feel, to see, to meditate, to doubt, to be glad, to be afraid, to feel pride or jealousy are possible states of a mental being; inner intuition, in supplying us with such categories, has a character less of sensation than of reason. (That is why it is almost impossible to separate the role of interpretation from that of perception in observations by inner intuition.)

Nevertheless you know that inner intuition, besides its qualities of evidence and rationality, also has its weaknesses:

1. There are also cases in experience of the mental where we are deceived by a direct impression.

2. Here as well, there is a kind of perspective, which distorts quantitative relations and hides important constituents of mental states.

3. Finally, an essential deficiency specific to inner intuition is that the attribution of mental states to the subject (that is to say to the Ego) is not made in an intuitive way.

Let us return to the main point: it was to determine the position resulting from the critique carried out on naive realism. Although one must give up almost all the realist evidence of outer sensation, something still remains: the indication of the existence of a reality that surrounds us, manifesting the variety of its content by the forms of contact that are revealed in our sensations.

We have here an important example of loss of evidence. But in the course of the intellectual process there are also evidences gained. At the outset naive realism is also an example, since the position of even this realism represents a stage in the acquisition of knowledge. But it isn't necessary to go so far back: in fact the evidences arising in mathematics are certainly almost all acquired evidences.

At issue here is evidence of relations, and the way in which they are formed is a special case of the general process of the origin of a dialectic, in the sense that Ferdinand Gonseth gives to that term.[1]

What distinguishes this case is that the dialectic is established in our mind in such a penetrating manner that it influences our intuitive imagination, that is to say that it influences the way in which we represent intuitively certain categories of objects. Thus the intentions of the dialectic find a sort of intuitive realization lying in spontaneous interpretations. In this way one also understands that intuition can derive notions that surpass the possibilities of a complete effective control and whose conceptual analysis gives rise to infinite structures. In particular this is the case for geometric intuition, which engenders notions like that of symmetry, encompassing that of the middle, as well as the distinction between straight and curved lines. I believe that that way of looking at things is not in disagreement with the results to which Gerrit Mannoury was led by following out his distinction between choice and exclusion negation: one must concede that there are geometric notions that are not directly intuitive, such as that of straight lines that do not ever intersect - which is precisely the usual definition of parallels. In general, it seems that geometric intuition has for its object only configurations of finite extension. (As is known, in the Euclid's Elements the axioms are formulated so as always to refer to finite figures; in the axiomatic system of Pasch, the rule to limit oneself to finite figures is observed intentionally.)

Concerning the theory of parallels, it must be noted nonetheless that the characteristic properties of Euclidean geometry can be expressed without introducing the negative notion of parallelism mentioned above. For example, the possibility, which children's games have made familiar to us, of juxtaposition of cubes (in such a way as to fill a portion of space without gaps) provides us with a formulation.

Thus our point of view permits us to recognize that the dialectic of Euclidean geometry has an intuitive evidence such that it is not found again in any other metric geometry. But one must also make the following remarks:

1. It should be understood that geometric evidence can no longer be

[1] *(Note added in the German version.)* In the meantime this use of the term in Gonseth's philosophy has been pushed into the background by another use.

considered – as was the case in Locke's philosophical position – as relating immediately to physical reality (that is to say as expressing properties of physical space); rather, it is a case of phenomenological evidence, for whose genesis one can nonetheless assume external causes that lie in the structure of physical space.

2. It seems that there is a part of geometric evidence that has a more primitive and fundamental character: that is the evidence of topological relations. Let us observe in particular that in carrying out reasoning in elementary axiomatic geometry one uses in general more or less crude sketched figures; here what we represent to ourselves intuitively are only the topological properties of figures, while for the rest we proceed according to conceptual rules. It is clear that for this semi-intuitive manner of reasoning, Euclidean geometry has no privilege over the geometry of Lobachevsky.

3. It must be recognized that for the construction of mathematical theories in their present form one can do without geometrical evidence; in effect, it is eliminated from the mathematics of today as far as the foundations are concerned; the role that still devolves on it is on the one hand that of an interpretation of great value, and on the other hand, for topological evidence, that of giving directives for the conceptions of the general theory of spaces. But, so it seems, the tendencies of intuition can here only be satisfied approximately, in the sense of a compromise – and that whatever system of arithmetic one also uses.

We have envisaged geometric evidence as an example of acquired evidence. The same holds for the evidences guiding arithmetical methods; they are acquired at a certain stage of intellectual development.

It is true that there are, in the domain of purely formal relations, completely primitive statements, for example to the effect that, in applying the usual rules of elementary algebra, from the expression $(a+b)*(a-b)$ one arrives at the different expression $(a*a)-(b*b)$. (That is not a tautology; in fact the indication of an operation to be carried out does not contain the indication of the result.)

These are the purest forms of evidence that we have at our disposal. But already the elementary theory of numbers goes beyond these primitive statements. There we encounter the general intuitive concept of natural number and the procedures of reasoning by complete induction and by recursive definitions that are connected with it. We have there already a full dialectic, which certainly did not exist from the beginning for the mind but which had to be tried out and dared at a certain stage.

Surely there is still a great distance between this dialectic of natural numbers and that by which we reason in the usual infinitesimal analysis. It must be conceded to Brouwer that this last dialectic does not have as fundamental an evidence as the former; moreover it must be admitted that it is not of a purely arithmetical character. Nevertheless we can state that it has succeeded quite well, that it constitutes a satisfying solution to the problems for which it was conceived, and that it too has engendered an evidence *sui generis*. What it lacks is only, with respect to the possible extensions of its methods, a leading idea appropriate for obtaining a delimitation that can be made without a conventional element.

The philosophy of intuitionism would suggest to us that we eliminate the usual dialectic in favor of a more strictly arithmetical procedure, as geometric evidence was eliminated. But in order that this idea should be accepted, it would be necessary, according to the rules of knowledge, that the intuitionistic method should be shown to be superior in every respect to the usual method.

In any case, the possibility of eliminating an evidence in the foundation of a science is a remarkable fact. – Moreover, from our analysis of acquired evidences it follows that it is not an essential condition for the efficacy of a dialectic that it should be equipped with a specific mode of evidence.

One could conceive the idea of eliminating evidence completely from the foundations of the sciences, only keeping for it the role that it has in heuristics, analogy, and interpretation.

Nonetheless one notices soon enough that, for the foundations, one cannot do without primitive evidences concerning formal relations, because these are necessary to check the functioning of a dialectic and for noting contradictions. Moreover it is certain that for experimental sciences we need evidences from observation, that is to say some psychological evidences, but it must be pointed out that these evidences are not valid here in a direct way but are inserted in a complicated manner into the total process of empirical research.

As you know, one meets in the modern social sciences with the "behaviorist" tendency to eliminate the evidences of internal intuition as much as possible. It is pointed out that for the investigation of psychological facts indirect indication is often more reliable than the direct indication of internal intuition. Certainly one cannot reasonably dispute that, but this preference for external indications surely goes only up to a certain point, and in any case a psychology in terms of external objects and relations such as the extreme defenders of the behaviorist tendency contemplate has little chance of being sufficient.

In mathematics there was the tendency of Hilbert, with his original conception of a theory of proof, to reduce all mathematical knowledge to primitive formal evidences. It was already a compromise to make use of the full finitist dialectic (incorporating the general concept of numeral), and it is [well] known that even that basis has been shown to be insufficient. Yet it is still possible that we might succeed in establishing a dialectic of constructive mathematics that would be equal to the requirements of proof theory.

But whatever the fate of these different attempts might be, in any case we are led to discuss the possibility of kinds of dialectic that do not have a character of evidence of their own. In order to work with such a dialectic a certain *understanding* is needed; we need to be in a position to attribute a sense to certain terms and to grasp relations resulting from the sense of these terms. (And the requirements of working with the dialectic are surely not the only ones!)

In this way we recognize the necessity of something like intelligence or reason, which will not be regarded as a container of [items of] a priori knowledge, but as a mental activity consisting in reacting to given situations by the formation of categories applied on a trial basis.

That reminds us of the tendency of Leonard Nelson, who (following the example of Kant and even more that of Fries) opposed making evidence the sole authority for knowledge. It was necessary to free this tendency from its subjection to traditional apriorism. That has just been attempted in the foregoing, in accordance with the ideas of the idoneism of Gonseth. It is also by this same idoneistic philosophy that we are led to recognize that it does not suffice to have evidence, but reason in its totality is needed.

Chapter 18

Bernays Project: Text No. 19

Mathematical Existence and Consistency (1950)

Mathematische Existenz und Widerspruchsfreiheit

(*Etudes de Philosophie des sciences en hommage à Ferdinand Gonseth*,
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It is a familiar thesis in the philosophy of mathematics that existence, in the mathematical sense, means nothing but consistency; this thesis is used to describe the specific character of mathematics. The claim is that there is no philosophical question of existence for mathematics. However, this thesis is neither as simple nor as self-evident as it may seem, and reflecting on it may shed light on several issues current in philosophical discussions.

Let us begin by describing what the thesis is directed against. It opposes quite obviously the view that attributes to mathematical entities an ideal being (i. e., a manner of existence that is independent on the one hand of being thought or imagined and also on the other hand of appearing as the determination of something real); this view claims furthermore that the existential statements of mathematics are to be understood with reference to this ideal being. One fact speaks from the very outset against this view; namely, that without apparent necessity an assumption is introduced here which does not do any methodological work. To make things clear, it may be advantageous to compare this with existential claims in the natural sciences. It is well known that an extreme phenomenalistic philosophy sought to eliminate the assumption of objects that exist independently of perception even from the representation of relations in nature. However, even a rough orientation about our experience suffices to show that such an undertaking—apart from the manifold obstacles that confront its implementation—is also inappropriate from a scientific perspective. In terms of perception alone, we do not gain any perspicuous laws. The world of our experience would have to be completely different in order for a theory—founded on notions concerning the purely perceptual—to be successful. Hence, positing the objective existence of entities-in-nature is by no means solely an effect of our instinctive attitude, but is appropriate from the standpoint of scientific methodology. (This is also true for contemporary quantum physics, even though according to it states cannot be specified with complete precision.)

When comparing this case with that of mathematical entities, we find the following obvious difference. In the theoretical and concrete use of mathematical objects an independent existence of these objects plays no role (i. e., an existence independent of their respective appearance as determinations of something otherwise objective). The assumption of objective physical entities, by contrast, has explanatory value only because the entities and states in question are posited as existing at particular times and in particular locations.

What we find here concerning mathematical objects holds in general for all those entities that can be called "theoretical objects." Meant are those entities of reflection to which we cannot ascribe, at least not directly, the character of the real, or more precisely, of the independently real; e. g., species, totalities, qualities, forms, norms, relations, concepts. All mathematical entities belong to this realm.

One can hold the view—and this view has indeed been defended by some philosophers—that all statements about theoretical entities are reducible, if made precise, to statements about the real. This kind of reduction would yield, in particular, an interpretation of existence statements in mathematics. However, at this point fundamental difficulties arise. On a somewhat closer inspection, it turns out that the task of reduction is by no means uniquely determined, since several conceptions of the real can be distinguished: The "real" may refer, e. g., to what objectively real, or to what is given in experience, or to concrete things. Depending on the conception of the real, the task of reduction takes on a completely different form. Furthermore, it does not seem that in any one of these alternative ways the desired reduction can be achieved in a satisfactory way.

One has to mention in this connection especially the efforts of the school of logical empiricism towards a "unified language" for science. It is noteworthy that recently the attempt to reduce all statements to those about the concrete has been abandoned. This was prompted in particular by the requirements in the field of semantics (an analysis of meaning of the syntactic forms of language).

We will not base our discussion on any presuppositions regarding the possibility in principle of avoiding the introduction of theoretical entitites in the language of science. In any event, the existing situation is that in areas of research (and even in the approaches of everyday life) we are constantly dealing with theoretical entities, and we adopt this familiar attitude here.

As yet this attitude in no way includes an assumption about an independent existence of ideal entities. It is understandable though that such an assumption has, in fact, often been connected with theoretical entities—particularly if we agree with Ferdinand Gonseth, according to whom the more general concept of an entity arises from a primary cruder notion of an entity that is expressed in a "physics of arbitrary objects." As regards the cruder objecthood, the character of the objective is most intimately tied to existence independent of our perception and representation. Thus it is easy to understand that for entities of a general kind we are inclined to attribute their objective character to an independent existence. It is not at all necessary to do so, however. Here it is especially significant that refraining from an assumption of ideal existence does not prevent us from using existence statements about theoretical entities: such statements can be interpreted without this particular assumption. Let us bring to mind the main cases of such interpretations:

a) Existence of a theoretical entity may mean the distinct and complete representability of the object.

b) Existence of a theoretical entity of a particular kind may mean that it is realized in something that is objectively given in nature. Thus, for instance, the observation that a certain word has different meanings in a language tells us that in the use of this language, the word is employed with different meanings.

c) An existence claim concerning theoretical entities can be made with reference to a structured object of which that entity is a constituent part. Examples of this are statements about constituent parts of a figure, as when we say, "the configuration of a cube contains 12 edges," or statements about something that occurs in a particular play, or about provisions that are part of Roman law. We are going to call existence in this sense, i. e., existence within a comprehensive structure, "relative existence."

d) Existence of theoretical entities may mean that one is led to such entities in the course of certain reflections. For example, the statement that there are judgments in which relations appear as subjects expresses the fact that we are also led to such "second order" judgments (as they are called) when forming judgments.

In case *a)* the existence of the theoretical entity is nothing but the representational objectivity (in the sense of representation proper); in case *b)* existence amounts to a reality in nature; case *c)* is concerned with an immanent fact of a total structure that is under consideration.

In these three cases the interpretation of the existence statement provides a kind of immediate contentual reduction. Case *d)* is different in that "being led" to entities is not to be understood as a mere psychological fact but as something objectively appropriate. Here reference is made to the development of intellectual situations with the factors of freedom and commitment operative therein—freedom in the sense in which Gonseth speaks of a "charter of our liberties" (for example, the freedom to add in thought a further element to a totality of elements represented as surveyable) and, on the other hand, commitment which consists, e. g., in the fact that the means we use for the description and intellectual control of entities yield, on their part, new and possibly even more complex entities.

Yet even this interpretation of existence statements does not introduce an assumption of independent existence of theoretical entities. The existence statement is kept within the particular conceptual context, and no philosophical (ontological) question of modality, which goes beyond this context is entered into. Whether such a question is meaningful at all is left open.

These considerations apply to theoretical entities in general. But what of the specific case of mathematical entities, which, as has been noted, are theoretical entities? If we apply our preceding reflections to the case of mathematical entities, we notice that we already have a kind of answer to the question of what existence may mean in mathematics. However, the thesis under discussion—that existence for mathematical entities is synonymous with consistency—is intended to offer a simpler answer. For the discussion of this claim we have already gained several clarifying points. Let us now turn to this discussion.

For this purpose let us first replace the obviously somewhat abbreviated formulation of the claim by a more detailed one. What is meant is surely this: Existence of an entity (of a complex, a structure) with certain required properties means in the mathematical sense nothing but the consistency of those required properties. The following simple example may illustrate this. There is an even prime number, but there is no prime number divisible by 6. Indeed, the properties "prime number" and "even" are compatible, but the properties "prime number" and "divisible by 6" contradict each other. Examples like this give the impression that the explanation of mathematical existence in terms of consistency is entirely satisfactory. It must be noted, however, that these examples do not show what this explanation is capable of; they only demonstrate how one infers consistency from the existence of an example and, on the other hand, non-existence from inconsistency, but not how one infers existence from an already established consistency; and that, after all, would be the decisive case.

This one remark suffices to make us hesitate. It draws attention to the fact that in mathematics existential claims are usually not inferred from proofs of consistency but, conversely, that proofs of consistency are given by exhibiting models; the satisfaction of the required properties is always verified in the sense of a positive assertion. In other words, the usual proofs of consistency are proofs of the *satisfiability* of conditions, or more precisely: [**they are proofs**] of the satisfaction of conditions by a theoretical entity.

An unaccustomed innovation was brought about by Hilbert's proof theory in that it demanded consistency proofs in the sense of showing the impossibility of arriving deductively at an inconsistency. A preconditon of such a proof is that the pertinent methods of deduction to be considered can be clearly delimited. The methods of symbolic logic provide the technique for making the process of logical inference more precise. We are thus in a position to delimit the methods of inference used in mathematical theories, especially in number theory and the theory of functions, by an exactly specified system of rules. This is, however, only a delimitation of the inferences used *de facto* in the theories. In general this does not lead to making an unrestricted concept of consistency more precise, but only consistency in a certain relatively elementary domain of logico-mathematical concept-formation. In this domain the concept of mathematical proof can be delimited in such a way that one can show: each requirement that does not lead deductively to an inconsistency can (in a more precisely specified sense) be satisfied. This completeness theorem of Gödel's makes particularly clear that the claimed coincidence of consistency with satisfiability is far from obvious, but is substantially dependent on the structure of the domain of statements and inferences considered. If one goes beyond this domain, making the methods of proof precise no longer yields the coincidence of consistency and satisfiability. This coincidence—as shown again by Gödel—cannot be achieved in general (if certain natural requirements are imposed on the concept of provability).

It is, of course, possible to extend the concept of proof by means of a more general concept of "consequence," following a method developed by Carnap and Tarski, so that for the resulting concepts of logical validity and contradictoriness (leading to a contradiction) we have the alternative that every purely mathematical proposition is either logically valid or contradictory. Consequently, every requirement on a mathematical entity is either inconsistent or satisfied by an entity.

Thus the identification of existence with consistency appears to receive exact confirmation. On closer inspection, however, one notices that the decisive factor is anticipated, so to speak, by the definitions. For, on the basis of the definitions, a mathematical requirement on a mathematical entity is already contradictory if it is not satisfied by any entity. Accordingly, in the field of mathematics the coincidence of consistency of a requirement and satisfaction by an entity says no more than that an entity of species G satisfying a condition B exists if and only if not every entity of species G violates condition B.

Of course—from the standpoint of classical mathematics and logic—this is a valid equivalence. But using this equivalence to interpret existence statements is surely unsatisfactory: If the claim that there is an exception to a universal proposition is considered to be in need of a contentual explanation, since it is an existential statement, then the negation of that universal proposition certainly is not clearer as to its content. The equivalence between the negation of a universal proposition and an existential proposition serves (in classical mathematics), among other things, to explicate more clearly the sense of the negation of a universal proposition. This is also indicated by Brouwer's intuitionism, which does not recognize this equivalence. At the same time, it denies that simple negation of a universal mathematical proposition has any sense at all, and introduces a sharpened negation—absurdity—which contains an existential aspect (since "absurdity" is to be understood as an effective possibility of a refutation).

The difficulties to which we have been led here ultimately arise from the fact that the concept of consistency itself is not at all unproblematic. The common acceptance of the explanation of mathematical existence in terms of consistency is no doubt due in considerable part to the circumstance that on the basis of the simple cases one has in mind, one forms an unduly simplistic idea of what consistency (compatibility) of conditions is. One thinks of the compatibility of conditions as something the complex of conditions wears on its sleeve, as it were, such that one need only sort out the content of the conditions clearly in order to see whether they are in agreement or not. In fact, however, the role of the conditions is that they affect each other in functional use and by combination. The result obtained in this way is not contained as a constitutent part of what is given through the conditions. It is probably the erroneous idea of such inherence that gave rise to the view of the tautological character of mathematical propositions.

Leaving aside the difficulties connected with the concept of consistency and with the relation between consistency and satisfiability, there is another aspect which points to the fact that it is not always appropriate to interpret existence as consistency in mathematics. Let us consider the case of existence axioms of an axiomatic mathematical theory. Interpreting the existence statement as an assertion of consistency in this case, yields confusion insofar as in an axiomatic theory consistency relates to the system of axioms as a whole. The condition of consistency may well function as a prior postulate for the design of any axiom system. The axioms themselves, however, are intended to generate commitments, at least in the usual form of axiomatics. An existence axiom does not say that we *may* postulate an entity under certain circumstances, but that we are committed to postulate it under these circumstances.

On the basis of our initial reflections, we also have an appropriate understanding of axiomatic existence statements available. That is to say, if we consider that an axiom system as a whole may be regarded as a description of a certain structured complex—for example, an axiom system of Euclidean geometry [**may be regarded**] as describing the structure of a Euclidean manifold—then we recognize that the existence claims within an axiomatic theory can be understood as statements about *relative existence*: Just as each corner in the configuration of a cube is incident to three edges, so through any two distinct points in the manifold of Euclidean space passes a straight line; and the theorem of Euclidean geometry which states that for any two points there exists a straight line through both expresses this fact of relative existence.[1]

It must be admitted, to be sure, that the perspective of relative existence, as appropriate as it is for the practical application of the existence concept in mathematics, only shifts, as it were, the philosophical question of mathematical existence. For relative existence is scientifically significant only insofar as the particular total structure, on which the relativeness is based, is to be regarded as mathematically existent. The question thus arises: what is the status of the existence of those total structures; for example, the existence of the number series, the existence of the continuum, the existence of the Euclidean space-structure and also of other space-structures?

Here we encounter examples where the identification of existence with consistency is justified. Thus it is justified when we say that the existence of non-Euclidean (Bolyai-Lobachevsky) geometry lies in its consistency. But even in such a case, the situation surely is that the consistency proof is given by exhibiting a model and that thereby the consistency claim is strengthened to the assertion that a model satisfying the axioms exists—"exists" understood here relative to the domain of the arithmetic of real and complex numbers. In analogous ways many consistency proofs in the sense of establishing satisfiability can be given; for example, the proof of the consistency of a non-Archimedean geometry (i. e., a geometry with infinitely small segments); further, the consistency of calculating with imaginary magnitudes, taking the theory of real numbers as a basis. Most model constructions of this sort are carried out within the framework of the theory of the mathematical continuum (the theory of real numbers). The satisfiability of the axioms of the continuum itself can again be seen, starting from the number series, by essential use of set-theoretical construction processes.

But where do all these reductions lead? We finally reach the point at

[1]Bruno von Freytag-Löringhoff has emphasized what is unproblematic, so to speak, about relative existence in his $_{c_1}$**work**$_{c_1}$, *The Ontological Foundations of Mathematics* (*vide* [**?**]), to which the present investigation owes a number of suggestions. In this connection the author speaks of the "small existence problem." His point of view, however, differs from the one presented here in that he regards the identification of existence with consistency as appropriate for the small existence problem, whereas in this presentation the viewpoint of relative existence is offered as a correction of the view equating existence with consistency.

which we make reference to a theoretical framework. It is a thought-system that involves a kind of methodological attitude; in the final analysis, the mathematical existence posits relate to this thought system.

We can state descriptively that the mathematician moves with confidence in this theoretical framework and that here he has at his disposal a kind of acquired evidence (for which constructions, even of a more complicated nature, such as infinite sequences of numbers, present themselves as something objectual). The consistency of this methodology has been tested so well in the most diversely combined forms of application that there is *de facto* no doubt about it; it is, of course, the precondition for the validity of the existence posits made within the theoretical framework. But we notice here again that we cannot simply identify existence with consistency, for consistency applies to the framework as a whole, not to the individual thing being posited as existent.

Let us consider the situation more closely, using the example of the number sequence. The postulation of the number sequence is included in the framework of our operating mathematically. But what does consistency of the number sequence mean? If we are content with an answer to this question that appeals to conceiving the unbounded continuation of the process of counting in an idealizing form of representation, then we understand existence in an objectual way. We view it in this way, whether we regard the number sequence merely as a domain of theoretical entities or, in accordance with a stronger idealization, as a structured complex in itself. And only from this objectual understanding do we infer consistency. If, however, consistency is to be recognized from the point of logic, then, on the one hand, the conditions contained in the idea of the number sequence must be understood conceptually and, on the other hand, we must base our considerations on a more precise notion of logical consequence.

In this connection we also come to realize that the concept of logical consequence gives rise to an unbounded manifold similar to that of the number sequence; that is due to the possibilities of combining inference processes. Furthermore, it becomes apparent that the domain of logic can be understood in a narrower or a wider sense, and that therefore its appropriate delimitation is problematic.

At this point we come to the area of mathematical-logical research in foundations. Its controversial character contrasts sharply with the aforementioned confidence in operating mathematically within the framework of the usual methods.

The difficulties we are facing here are as follows: The usual framework for operating mathematically is adequately determined for use in the classical theories; at the same time, however, certain indeterminacies with regard to the demarcation and the method of giving a foundation remain. If one tries to eliminate these, one faces several alternatives, and in deciding between them different views emerge. The differences of opinion are reflected in particular in the effort to obtain the foundation of mathematics from a standpoint without any substantive assumptions, such that one relies solely on what is absolutely trivial or absolutely evident. It becomes apparent here that there is no unanimity concerning the question of what is to be considered obvious or completely evident.

To be sure, these differences of opinion are less irritating if one frees oneself from the idea that an assumptionless foundation, obtained from a starting point determined entirely *a priori*, is necessary. Instead, one can adopt the epistemological viewpoint of Gonseth's philosophy which does not restrict the character of a duality—due to the combination of rational and empirical factors—to knowledge in the natural sciences, but rather finds it in all areas of knowledge. For the abstract fields of mathematics and logic this means specifically that thought-formations are not determined purely *a priori*, but grow out of a kind of intellectual experimentation. This view is confirmed when we consider the foundational research in mathematics. Indeed, it becomes apparent here that one is forced to adapt the methodological framework to the requirements of the task [**at hand**] by trial and error. Such experimentation, which must be judged as an expression of failure according to the traditional view, seems entirely appropriate from the viewpoint of intellectual experience. In particular, from this standpoint experiments that turned out to be unfeasible cannot *eo ipso* be considered methodological mistakes. Instead, they can be appreciated as stages in intellectual experimentation (if they are set up sensibly and are carried out consistently). Seen in this light, the variety of competing foundational undertakings is not objectionable, but appears in analogy with the multiplicity of competing theories encountered in several stages of development of research in the natural sciences.

If we now examine more closely the—at least partial—methodological analogy between these foundational speculations and theoretical research in the natural sciences, we are led to think that with each more precise delimitation of a methodological framework for mathematics (or for an area of mathematics) a certain domain of mathematical reality is intended, and that this reality is to a certain degree independent of the particular configuration of that framework. This can be made clear by means of the geometric axiomatics. As we know, the theory of Euclidean geometry can be developed axiomatically in various ways. The resulting structural laws of Euclidean geometry, however, are independent of the particular way in which this is done. The relations in the theory of the mathematical continuum and the disciplines associated with it are in a similar sense independent of the particular way in which the real numbers are introduced, and even more so of the particular method of a theoretical foundation. In a foundational investigation those relations, which are, as it were, forced on us as soon as we settle on certain versions of the calculus and of operating mathematically, have the role of the given, and it more precise theoretical fixation is the task at hand. The method of this fixation can contain problematic elements which do not affect what is, so to speak, given.

The viewpoint gained in this way places a mathematical reality face to face with a methodological framework constructed for the fixation of this reality. This is also quite compatible with the results of the descriptive analysis to which Rolin Wavre has subjected the relationship of invention and discovery in mathematical research. He points out that two elements are interwoven, the invention of concept formations, and the discovery of lawlike relations between the conceived entities, and furthermore that the conceptual invention is aimed at discovery.

With respect to the latter, it is frequently the case that the invention is guided by a discovery already more or less clearly available and that it serves the purpose of making it conceptually definite, thereby making it also accessible to communication. The necessity of adapting the concepts to the demands of giving expression to something objective exists in this situation as much as it does in similar situations in the theoretical natural sciences. Thus the concepts of the differential quotient and of a field were introduced with a view to giving expression to something objective in the same way as the concepts of entropy and the electrical field.

In the constitution of a framework for mathematical deduction we assume to have a case of the same methodological type, when we speak of a mathematical reality that is to be explicated by that framework.

If we now apply this perspective to our question of mathematical existence, we obtain an essential addition to our earlier observation that the existence statements in our mathematical theories are, in the final analysis, relative to a system of thought that functions as a methodological framework. This relativity of the existence statements now seems compensated to a large extent by the fact that the essential properties of the reality intended by the methodological framework are invariant, so to speak, with respect to the particulars (the invented aspects) of that framework.

Furthermore, it must be noted here that the mathematical reality also stands out from any delimited methodological framework insofar as it is never fully exhausted by it. On the contrary, the conception of a deductive framework always results in further mathematical relations which go beyond that framework.

Do we not—so one may ask—return with such a view of mathematical reality to the assumption of an ideal existence of mathematical entities which we rejected as unmotivated at the outset of our reflections? To respond to this question we must recall the limits of the analogy between mathematical and physical reality. We are dealing here with something very elementary.

It is inherent in the purpose of scientific concept formation that it seeks to provide us with an orienting interpretation of the environment. In the natural sciences the modality of the factually real plays therefore a distinguished role, and in comparison with this reality all other existence one can talk about appears as mere improper existence—as when we speak of the existence of the relations given by laws of nature. This is, in fact, true even though the statements concerning the existence of laws of nature contentually go beyond what can be ascertained in the domain of the factual.

In mathematics we do not have such a marked difference in modality. ¿From the viewpoint of the mathematician, the individual mathematical entity does not present itself as something that exists in a more eminent sense than the relations given by laws. Indeed, one can say that there is no clear difference at all between something directly objectual and a system of laws to which it is subject, since a number of laws present themselves through formal developments which possess the character of the directly objectual. Even axiom systems may be considered as structured objects. In mathematics, we therefore have no reason to assume existence in a sense fundamentally different from that in which we assume the existence of relations given by laws.

This eliminates the various reservations that seem to oppose our view of the relativity of mathematical existence statements to a system of the conceptual (to a deductive framework): Irrespective of the various possibilities of constructing such a system of the conceptual, this view does not amount to relativism. On the contrary, we can form the idea of a mathematical reality that is independent in each case of the particulars of the construction of the deductive framework. The thought of such a mathematical reality, on the other hand, does not mean a return to the view of an independent existence of mathematical entities. It is not a question of being but of relational, structural connections and of the emergence (being induced) of theoretical entities from other such entities.

In order not to be one-sided, however, our reflection on mathematical existence still requires a complementary prespective. We have carried out this reflection in accord with the attitude of the mathematician who directs his attention purely toward the objectual. If we bear in mind, however, our methodological comparison between the mathematical (foundational) starting points and those of physics, then we might realize that this analogy also applies to a point we have not noted yet: Just as the theoretical language and the theoretical attitude of physics is substantially complemented by the attitude and language of the experimentalist, so is the theoretical attitude in mathematics also complemented by a manner of reflection that is directed toward the procedural aspect of mathematical activity. Here we are concerned with existence statements that do not refer to abstract entities but to arithmetical expressions, to formal developments, operations, definitions, methods for finding solutions, etc. The significance of such a constructive mode of reflection and expression—as it comes to the fore especially in Brouwer's intuitionism and for the method of Hilbert's proof theory—is also acknowledged by mathematicians who are not willing to be content with an exclusively constructive mathematics and, therefore, just as little with an action-language of mathematics as the only form of mathematical expression.

In this context it should be emphasized with respect to Hilbert's proof theoretic project which is based on an operative (constructive) standpoint: the interest of this project for the philosophy of science is not at all tied to that philosophical doctrine of "formalism" which arose from the original formulation of the aim of proof theory. In order to appreciate the methodological fruitfulness of proof theory, there is in particular no need to take the position that the theories subjected to symbolic formalization (for proof theoretic purposes) should be simply identified from then on with the schema of their symbolic formalism and thus should be considered merely as a technical apparatus.

We must also bear in mind that the motivation for the conceptual system of contemporary mathematics does not lose its significance through the proof theoretic investigation of consistency; this motivation results from the connection to the problems that gave rise (in several stages) to the conceptual system in the first place. Such a motivation is indeed assumed to have already been obtained before the proof theoretic investigation begins.[2]

Finally it should be remembered—as regards the methods of constructive proof theory and also those of Brouwer's intuitionism—that with these methods one does not remain in the domain of the representationally objectual, properly so called. The concept of the effective is idealized and extended here in the sense of an adaptation to theoretical demands—of course in a way which is in principle more elementary than it is done in ordinary mathematics. The methodological standpoint also in this case is thus not without pre-conditions, but we are concerned, once again, with a theoretical framework that includes general kinds of positing. Our preceding reflections are, therefore, also applicable to this constructive mathematics.

On the whole our considerations point out that it is not indicated either to exaggerate the methodological difference between mathematics and the sciences of the factual, which undeniably exists, or to underestimate the philosophical problems associated with mathematics.

[2][1] As regards the task of a *systematic* motivation of the concept formations of classical mathematics, we are led to the problem already mentioned for obtaining a deductive framework that is as appropriate and as satisfactory as possible. This problem constitutes a major topic of contemporary foundational research in mathematics.

Chapter 19

Bernays Project: Text No. 20

On judging the situation in proof theoretical research (1950)

Zur Beurteilung der Situation in der beweistheoretischen Forschung

(*Revue Internationale de Philosophie* 27/28, pp. 9–13)

Translation by: *Dirk Schlimm*
Revised by: *CMU*
Final revision by: *Bill Tait*

When I speak here in brief about the situation in proof theoretic research, it appears appropriate to remind ourselves of what is characteristic of this research: it is the systematic investigation of the kinds of applications and the consequences of logical reasoning in the mathematical disciplines, in which the concept formations and the assumptions are fixed in such a way that a strict formalization of the proofs is possible with the help of the means of expression of symbolic logic.

As you know, Hilbert stimulated this kind of investigation mainly with regard of the questions of consistency. But he had also envisaged from the beginning the treatment of questions regarding the completeness and decidability in the framework of these investigations, for example already in the lecture "Axiomatic thinking" (1917, *vide* [**?**]). He formulated in more detail questions regarding completeness in the lecture "Problems of the founding of mathematics" in Bologna (1928, *vide* [**?**]).

To be sure, Hilbert imagined many things regarding both the results to be obtained and the method to be simpler than they eventually turned out to be. The knowledge of these major difficulties awakened in many the idea that proof theoretic research has led to a definitive failure. But a glance at the actual state of affairs shows that there is no question of this: the methods of proof theoretic considerations find themselves in a rich state of development and considerable results have been obtained in various directions. Let me list some noteworthy successes regarding the problems Hilbert formulated:

1. Gödel's Completeness Theorem (proof of the completeness of the first order predicate calculus) together with its related extensions.

2. One succeeded in making the concept of decidability precise in such a way that systematic results could be obtained on the basis of this definition, in particular the proof of the unsolvability of the decision problem for predicate calculus by Church and, in a second way, by Turing.

3. While the aforementioned methods lead only to conclusions concerning undecidability, Tarski succeeded, on the other hand, to specify decision procedures for certain mathematically non-trivial domains. In connection with these results as well as through results supplementing Gödel's completeness theorem, there have been applications in mathematics which are also of interest to mathematicians not concerned with foundations.

4. Regarding the questions of consistency, a consistency proof for full analysis has not been achieved from the finite standpoint, but one has been obtained for restricted analysis (for example in Weyl's sense or in the sense of ramified type theory) from a constructive standpoint. Gentzen first supplied such a proof for the number theoretic formalism; but Gentzen already had in mind the extension of his method to ramified analysis. This has been carried through by Lorenzen, Schütte, and Ackermann, whereby the method of proof also became more transparent. Also to be mentioned is a new transparent consistency proof for number theory by Stenius. Furthermore, it is remarkable that the extension of the finite standpoint to the constructive standpoint in a freer sense makes it possible to consider proofs that do not have to be formalized in the full sense, but can contain parts in which metamathematical derivations can be specified which sometimes depend on a syntactical numer-

ical parameter. In this way, one transcends the domain of those systems to which Gödel's incompleteness theorem applies.

By the way, this important theorem is by no means to be judged only as a negative result; rather it plays a role for proof theory similar to that of the discovery of the irrational numbers for arithmetic.

5. Finally, efforts have been made to supplement the statement of consistency with a more general form of question: what can be extracted from the formal provability of a theorem from the constructive standpoint? Kreisel's investigations move in this direction.

Given all this it would obviously be totally inappropriate to speak of a general fiasco of proof theory. On the other hand it must be acknowledged that not only has the most essential work in this domain still to be done, but also that, regarding the methodology, there is no clear resolution and no unanimity. I would like to raise a few points in this connection.

One speaks today a bit condescendingly about the "naive set theory." We must, however, remind ourselves that it is, in any case, naive to think that, by a retreat to the axiomatic standpoint, without any contentual approach supporting it, we have at our disposal anything like what we started with. The retreat to the axiomatic in the case of non-Euclidean geometry is less problematic, because there we take arithmetic and set theory, as given knowledge, as a foundation. The discussions about possible geometries, in particular the model theoretic considerations, take place within the framework of arithmetic (analysis). By challenging this framework and assigning to set theory itself the role of an axiomatic theory, it becomes necessary to determine a different underlying framework which has to act as the arithmetic proper. Different views are possible with regard to the choice of this methodological framework.

The minimal requirement for a sharpened axiomatization is that the objects not be taken from a domain that is regarded as being antecedent, but that they be constituted by generating processes. But one could take the meaning of this to be that these generating processes determine the extensions of the objects; this point of view motivates the *tertium non datur*. In fact, the openness of a domain can be understood in two senses: on the one hand, that the processes of construction lead beyond any single element, and on the other hand, that the resulting domain does not represent a mathematically determined manifold at all. Depending on whether the number sequence is understood in the first sense or in the second, one obtains the acknowledgment of the *tertium non datur* with respect to the numbers or the intuitionistic standpoint. For the finite standpoint the requirement is added that the considerations have to be made by means of investigating finite configurations, thus in particular assumptions in the form of general statements are excluded.

The maximal requirement for the methodical framework goes beyond even that of the finite standpoint. This [standpoint] in fact contains existence assumptions, required for the possibility of systematic considerations, which are not self-evident from the standpoint of the properly concrete. For example, the application of such existence assumptions is necessary, if we want to show the eliminability of complete induction in the sense of Lorenzen. Originally, Hilbert also wanted to adopt the narrower standpoint which does not presuppose the intuitive general concept of numeral. This can be seen from his lecture in Heidelberg (1904) among others. It was already a kind of compromise that he decided in favor of adopting the finite standpoint in his publications. If we make ourselves clear on this, then the need for transition from the finite standpoint to an extended constructive standpoint does not appear to be so catastrophic.

To be sure, this requires a philosophical adjustment. Many think that one either has to accept only absolute evidence, or that evidence has to be generally abandoned as a feature of the sciences. Instead of this "all or nothing" attitude, it appears to be more appropriate to understand evidence as something that is acquired. The human being obtains evidences in the way he learns to walk, or as the birds learn to fly. Hereby one comes to the Socratic acknowledgment of our basic inability to know in advance. In the theoretical realm we can only try out points of view and standpoints and possibly have intellectual success with them.

This does not mean that, with these points of view, the problem of the foundations is already solved in principle. But at least such modesty allows that we not be completely disconcerted whenever new antinomies are discovered. Such antinomies then appear rather to be instructive clues for the right choice of our approaches and methods.

The problematic in the foundational research that has still not yet been overcome consists of different aspects: on the one hand, in respect to the choice of the methodical standpoint in the foundational research, as well as the choice of the deductive framework, and on the other hand, in respect to the understanding of mathematics. With regard to this second point a decision is maybe not to be expected by means of the foundational research, but in respect to the first questions it is not too immodest to hope that the comparison of the results of the different directions of research will yield a clear advantage to one of the ways of proceeding in the foreseeable future.

DISCUSSION

Arnold Schmidt. — My introduction of degrees of consistency, which has been mentioned by Mr. Bernays, was merely meant to emphasize the problem of the role that consistency plays epistemologically. [...]

With regard to the extensions that the finite standpoint has experienced in the course of its development I would like to remark that *tertium non datur* remains excluded at all stages of this development.

With respect to the problem of evidence one can say the following, in a certain analogy to the interpretation of the Kantian *a priori* The individual can obtain evidence through reflection, but the criteria for the evidence must be independent of such experience in order to rule out deceptive evidence which can arise by habituation. As much as I acknowledge that the matters of fact which are not evident at first sight can become evident by a thorough clarification, I want to emphasize, on the other hand, that in my opinion there can be *only one* kind of evidence, thus no relative or graduated evidence. From this point of view the task of the proof consists in reducing something that is not evident to something that is evident.

Paul Bernays. — There is no disagreement with regard to the first point. With respect to the second remark I'd like to call attention to the fact that I did not intend to write history. Had this been the case, I would have distinguished five stages of metamathematics: 1. the finite standpoint, 2. the definite standpoint ((1) with existence assumptions), 3. Intuitionism, 4. *tertium non datur*, 5. impredicative concept formation. This ordering gives more and more freedom. While it was possible to point out intimate agreements between Intuitionism (3) and the classical standpoint (4), this has not succeeded for (4) and (5) although Gentzen struggled with it. Thus the decisive point lies beyond the introduction of *tertium non datur*. Finally I wouldd like to say that one should not merely construe evidence objectively, forgetting about subjective determinations.

[... In response to Behmann, who adduced Helmholtz's argument for the "evidence" of non-Euclidean geometries, Bernays added:] Although differently constituted beings could have a different [**conception of**] evidence, it is our concern to determine what counts as evident for us. [...]

Alfred Tarski. — [...] Furthermore I should like to remark that there seems to be a tendency among mathematical logicians to overemphasize the importance of consistency problems [...]. Gentzen's proof of the consistency of arithmetic is undoubtedly a very interesting metamathematical result, which may prove very stimulating and fruitful. I cannot say, however, that the consistency of arithmetic is now much more evident to me (at any rate, perhaps, to use the terminology of the differential calculus, more $_d$**evident**$_d$than by $_a$**an**$_a$ epsilon) than it was before the proof was given.
Paul Bernays. — My thought has not been rightly interpreted. I did not wish to say that Gentzen's proof made arithmetic or truths about arithmetic more evident. But I tried to stress that some mathematical methods $_d$**allow**$_d$simultaneously $_d$**to**$_d$show $_a$**deducibility**$_a$ and validity. [...]

Chapter 20

Bernays Project: Text No. 21

Mathematics as both familiar and unknown (1954)

Die Mathematik als ein zugleich Vertrautes und Unbekanntes

(*Synthese* IX (1953–55), pp. 465–471;
repr. in *Abhandlungen*, pp. 107–112)

Translation by: *David Weberman*
Revised by: *Erich Reck*
Final revision by: *Charles Parsons*

When the mind feels weighed down or oppressed by the many mysteries of existence, by the impression of our extensive ignorance in so many areas, by the inadequacies of linguistic representation and communication, it often turns gladly to mathematics, where objects can be grasped clearly and precisely, and where gratifying insight can be attained through appropriate concepts. Here the human mind feels at home; here it experiences the triumph that the application and combination of quite elementary ideas—familiar to us from childhood play—yield significant, unexpected, and far-reaching results. Taking concrete matters as the starting-point, mathematical thinking is occupied in fixing its objects intuitively and imagining them; from there, by forming concepts and by mentally interweaving [its] findings, it goes on to results, which in turn can be applied to the concrete and show themselves impressively successful.

As a consequence, mathematical activity reveals its power and productivity in three ways. First of all, we have here a striking form of an original representation as a source for cognition, as well as for concept formation connected with it. Second, logical reasoning is here a powerful cognitive tool, indeed one that functions in a truly essential way only in this domain. But there is still a third respect: in mathematics we have not only the activity of intuition and logical reasoning, an activity which allows these powers residing in our inner nature to develop freely and productively; we also have the connection to familiar objects of everyday perception, and, beyond that, we have the remarkable confirmation which mathematics finds in the extended domain of experience where our ordinary perception no longer suffices for orientation.

These three kinds of success and satisfaction evinced by mathematics correspond roughly to the three aspects distinguished by Ferdinand Gonseth: to the intuitive, the theoretical, and the experimental aspect.

If we take a closer look at the development of mathematics and its applications, we do, of course, soon come to problematic features. If we begin with the application of mathematics to the explanation of nature, the historical development shows us a twofold disappointment in the following respect: mathematics was believed to yield a kind of familiarity with reality that it *de facto* does not provide.

This happened first in connection with the doctrine of the Pythagoreans, who discovered the reducibility of qualitative differences in perceptual objects to numerical relations as carried through in theoretical physics. The pursuit of this discovery gave rise to the hope that the concept of number might bring about an ultimate, penetrating understanding of, and thus, intellectual familiarity with, what is real. As is well known, this doctrine was fundamentally shaken by the discovery of irrational magnitudes. The Greeks soon learned to deal with irrational magnitudes in a correct deductive manner; but Eudoxus' procedure was quite abstract, and Euclidean geometry, which built on it, was in its axiomatic attitude much more restrained than the Pythagorean doctrine. It is here, too, that the purely mathematical was for the first time strictly separated from the natural sciences.

Hope for a mathematical understanding of reality arose for a second time at the beginning of the modern era. Under the influence of the powerful development of the theoretical natural sciences, and especially also of mathematics itself, that mechanistic view of nature emerged that captured many minds. Although this view of nature was paradoxical from the outset, Kantian philosophy then provided a mode, by opposing the real in itself to appearances, to carry through the mechanistic viewpoint for the domain of appearances and to view this domain as something governed by the manner of our intuitive representation. Thus nature, governed by our forms of intuition and structured mathematically, acquired the character of something familiar to us.

I need not speak about the fact, discussed so often and so much, that the contemporary development of theoretical physics has moved fundamentally away from this view. To be sure, in today's theoretical physics mathematical tools are used extensively and with great success. But we are no longer talking about a perspective of intuitive familiarity.

However, these difficulties concern the theoretical sciences, not mathematics itself. A brief survey of the development of mathematics presents us initially with the picture of an impressive triumphal march. It begins with the formal development of the infinitesimal calculus, which caused the so-called irrational in the theory of magnitudes to lose its character as an *apeiron*. The numerous beautiful and, in terms of laws, simple presentations of irrational magnitudes then moved them into the domain of the familiar. Yet initially the procedure of the infinitesimal calculus lacked sufficient methodological precision; this was achieved in the nineteenth century.

This time was also a period of massive expansion in mathematics, which deserves to be emphasized all the more since it has never come sufficiently to the consciousness of educated humanity. What developed was a freer mode of abstraction and a strengthened way of forming concepts. Consequently, new methods were developed, and a whole series of new mathematical disciplines emerged. In these disciplines the operation with mathematical concepts began to display great power, beauty, and an impressive richness of thought. A high level of rational understanding was reached here, and a new way of being intellectually familiar with enitities was gained.

Two important events in the history of ideas took place in connection with this development. The first was the discovery of non-Euclidean geometry. The second was the realization of the Leibnizian program in the domain of logic by establishing a logical calculus. This calculus might have appeared playful in its initial form, but it was later extended in such a way that it allowed for the formal representation of mathematical proofs.

While mathematics was reaching up to new forms and spheres of understanding, its character of familiarity was lost in some respects, especially since what was once the starting point and center lost this position. Not only did Euclidean geometry lose its privileged position, and thus its role as the evident theory of space, but the arithmetical theory of magnitudes, too, now seemed to be just the theory of one structure among others. The dominant point of view had become that of the general formal theory of structures. But this led to difficulties in two different ways: first, in terms of antinomies, which resulted from the fact that some totalities of possible structures, while presenting themselves as mathematical entities in a manner analogous to the number series, cannot be understood in that way on pain of contradiction; second, in terms of the strange aspect of Cantor's set theory that an immense progression of infinite cardinal numbers appeared that dwarfed both the infinite number series and the manifold of the mathematical continuum, indeed to a fundamentally greater extent than that in which the size of our earth is dwarfed by astronomical expansions. This led many to begin to doubt the justification and meaningfulness of the methods applied, and the call could be heard "Back to the concrete!" Concepts and modes of inference which had previously been recognized and used were no longer accepted. Various developments of new frameworks in mathematics were undertaken. A particular example of such a new framework is, of course, that of Brouwer's intuitionism. Hilbert, on the other hand, had the idea of connecting mathematics more strongly to concrete representation by utilizing the formalization of mathematical reasoning.

In his recent talk at the Brussels Congress, Mr. Heyting discussed the current state of research concerning the foundations of mathematics. In addressing the question of the object of mathematics, he found that it cannot be answered in a satisfying way for classical mathematics (i. e., the mathematics developed in the nineteenth century). The reason is, according to Heyting, that in classical mathematics intuitive and formal elements are combined without clear distinction. Then again, he believes that a more precise working out of these two elements, as given in Brouwer's intuitionism for the intuitive element and in Hilbert's proof theory for the formal element, is also not satisfactory for an exhaustive treatment of the epistemological problems at hand. This, he believes, indicates that the question of the object [of mathematics] is ill-phrased and has to be replaced by a more adequate formulation.

We can certainly agree with that conclusion. Indeed, it is easy to tie the question of the object [of mathematics] to a non-trivial presupposition, namely, that in scientific inquiry the object must be given to us prior to it. A study of the sciences shows, however, that an exact determination of the objects of theoretical disciplines generally grows only out of their conceptualization. We also do not need to view the combination of intuitive and formal elements in classical mathematics, noted by Mr. Heyting, as a defect. Indeed, often the role of important conceptual and methodical approaches lies exactly in the fact that they offer a kind of balance between intuitive and theoretical-formal intentions.

Such a balance is already present in the basic perspective of number theory, even in an elementary ("finitist") treatment of it. We should be clear here that even in finitist number theory we are no longer in the sphere of the genuinely concrete; large numbers cannot be exhibited in imagination or perception. From the standpoint of an approach which aims to remain in the genuinely concrete, it is then in particular not clear what a universal statement ranging over arbitrary numbers could mean. The attempt to interpret such a universal statement by appealing to the existence of a proof does not lead to its objective. Indeed, if a contentual proof, for instance in the intuitionistic sense, is intended, it consists of a certain procedure that must be exhibited. But then it must be clear that this procedure realizes the *desideratum* in each individual case; and such a claim is again a universal number-theoretic statement. If, on the other hand, a formal derivation within a deductive system is intended, then one must convince oneself that the deductive formalism functions appropriately; and this leads again to a claim that takes the form of a universal number-theoretic statement.

We can think about the intellectual steps which lead to the specifically number-theoretic point of view roughly in the following way: First we are conscious of the freedom we have to advance from one position arrived at in the process of counting to the next one. But then we take the step of a connection, through which a function that associates a successor with each and every number is posited. Hence a *progressus in infinitum* replaces the *progressus in indefinitum*. But it is not immediately obvious that this idea of the infinite number series can be realized; the intellectual experience of its successful realization is then essential for developing a feeling of familiarity, even of obviousness, as an acquired evidence.

The philosophy of mathematics generally tends not to appeal to such acquired evidence, but replaces it with a evidence *ab ovo*. Thus one is misled to make one of two mistakes: either to exaggerate the reach of this evidence by trying to include all possibly attainable levels, which leads to the antinomies; or to posit a particular level of evidence as absolute, which results in requiring a restriction of mathematics in such a way that we unnecessarily lose the freedom of making intellectual decisions.

We can avoid these unacceptable consequences if we give up the view that mathematics is something obvious. The element of familiarity that we find in mathematics, especially in elementary mathematics, is an acquired familiarity. To be sure, mathematics is above all a grasping, not something to be grasped. But the possibility of successfully extrapolating, by way of strict mathematical laws, intuitive relations of numbers and figures is basically as non-obvious as the possibility of discovering physical laws of nature. In this respect we must return to the wisdom of Socrates, that is, we must recognize our own ignorance. Kant's belief that the structure of our own cognition must be determinable *a priori* for us is clearly based on an illusion. The structure of our mental organization is as transcendent for our consciousness as is the character of external nature.

It is also hardly true that a mathematical element enters into our investigation of nature only through the manner of our intuitive representation. Yet we can clearly accept that the world of mathematics confronts us as a phenomenal realm. Consequently one can speak in relation to mathematics of a phenomenology of the mind, in a sense different from Hegel's. However, the phenomenal in this sense goes certainly beyond what we may assume to be innate in the individual, if for no other reason than that it is structurally open. And if one speaks of "mind," "reason," or "form of intuition" in the sense of something that goes beyond concrete psychic constitution, there is no longer a clear difference between what belongs to the subject and any element of the world order.

Philosophical speculation about mathematics leads, indeed, to such lofty regions. When we consider mathematics not from the standpoint of its immediate application, where it provides us with the experience of the familiar and evident, but want to pursue its roots philosophically instead, we must avoid too simplistic a conception of mathematics.

Chapter 21

Bernays Project: Text No. 22

Considerations regarding the paradox of Thoralf Skolem (1957)

Betrachtungen zum Paradoxon von Thoralf Skolem

(*Avhandlinger utgitt av Det Norske Videnskaps-Akademie i Oslo*, pp. 3–9; repr. in *Abhandlungen*, pp. 113–118)

Translation by: *Dirk Schlimm*
Revised by: *CMU*
Final revision by: *Steve Awodey*

About 35 years ago, on the occasion of a congress in Helsingfors, Thoralf Skolem pointed out a paradoxical consequence of a theorem by Leopold Löwenheim, for which he had presented a simplified proof two years earlier using the logical normal form named after him.

This well-known theorem by Löwenheim says that for every mathematical theory axiomatized in the framework of elementary predicate logic—i. e., without bound variables for predicates—there exists a model in which the individuals are natural numbers, provided that there is a model that satisfies it at all. The theorem can be extended to the case in which one or more axiom *schemata* occur in the axiom system besides the proper axioms. In the axiom schema an arbitrary predicate that can be constructed using the formation rules of the axiom system, resp. a set or function that is arbitrary in the same sense, occurs as parameter.

Now Skolem realized that this theorem can be applied to axiomatic set theory, provided it has been sharpened by a more precise concept of definite property over the original formulation of Ernst Zermelo. That it is possible to sharpen it in such a way, whereby the axiom system can be represented as a calculus by axioms and schemata, had been realized shortly before by Skolem and, in a different way, by Abraham Fraenkel. By the way, John von Neumann even succeeded in setting up a system of finitely many axioms (without schemata) for set theory.

Thereby the possibility of such models for set theory arose in which sets are represented by natural numbers. This possibility is quite paradoxical, because the cardinal numbers of the sets that occur rise to such dizzying heights according to the theorems of set theory that the infinity of the number sequence (the countably infinite) is exceeded by far.

That this does not constitute a proper contradiction follows, as is well-known, from the fact that the enumerations which work as such in the axiomatic framework do not yet exhaust all possible enumerations. The concept of set is restricted by the axiomatic specification in such a way that one can speak of "set" only relative to a particular framework, if one generally insists on the requirement of axiomatic precision. This relativization is extended to a series of other concepts that are closely connected to the concept of set, in particular the concept of a uniquely invertible mapping between two totalities and thereby also the concept of cardinality (generalized concept of number), and especially that of denumerability.

At first the impression arises that the detected paradox shows above all that differences of magnitudes are apparent and especially that the properly uncountable is an illusion. At the same time the thought is suggested that an operative construction of mathematics, and in particular of analysis, might be preferable to an axiomatic formulation, in the light of the ascertained relativity.

An operative understanding of mathematics is championed by many. It is characteristic that it does not regard the object of mathematics as something that is given in advance and that should be made accessible to our thought by formation of concepts and axiomatic descriptions, but that the mathematical operations themselves and the objects that are brought about by them are regarded as the topic of mathematics. Mathematics should, to some extent, create its own objects. Thereby the character of arithmetic is prescribed *eo ipso*, since the structures of the operative creation are not fundamentally more general than those of the number sequence.

Herein lies a strength of this standpoint on the one hand, and a weakness on the other. It possesses a strength insofar arithmetical (constructive, combinatorial) thinking has the methodical distinction of being elementary and intuitive. However, it is doubtful whether we can get by with it for mathematics and whether a, so to speak, monistic conception of mathematics in the sense of the operative view can do full justice to its content—even as it is now.

This idea is especially reinforced when we consider the enterprises of an operative construction of analysis as they have been pursued in more recent times, following different programmatic points of view. All these kinds of constructions have in common that we are hindered by distinctions which are of no relevance for the geometrical idea of the continuum and are not necessary for the consistent functioning of the concepts. The usual procedure of classical analysis proves to be vastly superior in this respect; and if the treatment of analysis had historically begun with an operative procedure, the detection of the possibility of the so much simpler classical methods would have been an eminent discovery, hardly less as it meant a *de facto* eminent progress in a different direction, namely compared to the vagueness of the former operations in analysis.

The sense of an appropriate formation of concepts for analysis apparently lies in a suitable compromise. We can make that plausible by the following. The conflicting aspects of the concept to be determined are, on the one hand, the intended homogeneity of the idea of the continuum and, on the other hand, the requirement of conceptual distinctness of the measures of magnitudes. From an arithmetical point of view, every element of the number sequence is an individual with its very specific properties; from a geometric point of view we have here only the succession of repeating similar things. The task of formulating a theory of the continuum is not simply descriptive, but a reconciliation of two diverging tendencies. In the operative treatment one of them is given too much weight, so that homogeneity comes up short.

The investigations about the effectiveness and the fine structure in the formation of number series and sets of numbers have an unquestionable importance for their specific aspects of the general question. But the insights that have been gained here do not constitute a definite indication that the usual procedure of analysis should be replaced by the more arithmetical methods.

The method on which the procedures in classical analysis are based consists, in its logical means, in the application of a contentful "second-order" logic, in which the general concepts like "proposition," "set," "series," "function" etc. are used in an unbounded way that is not further specified. This second order logic shows its strength not only in its application to the theory of the continuum, but that it generally allows for the characterization of mathematical structures, that may even be uncountable, by explicit definitions. Namely, to what is usually called an "implicit definition" of mathematical objects there corresponds an explicit definition of a whole structure wherein those objects occur as dependent components. The model theoretic concepts of satisfiability and categoricity also find here their unproblematic application.

To be sure, second order logic is reproached for having a certain imprecision in the concepts, and it is the aim of the new sharper form of the axiomatic approach to repair this defect. Logic and axiomatic set theory have developed the methods for this. The phenomena of the relativity of the higher general concepts discussed above is evidence that this has not succeeded in a completely adequate way to make the concepts precise.

Let us again consider this with an example. The property that an ordering has no gaps is expressed in second-order logic by the condition that every proper initial segment of the ordering which has no last element possesses an immediately succeeding one. The general concept of set appears here by means of the proper initial segment. If this is now made precise by giving certain conditions on how to obtain sets, the manifold of the initial segments under consideration is narrowed, and thereby the condition is weakened. This means that some orderings are admitted as being gapless that can no longer count as being such if the concept of set is sufficiently expanded (i. e., if further processes are admitted for the formation of sets).

The difficulty considered here which, is related to the task of making a theory formally precise, not only occurs in the characterization of uncountable structures, but especially also in the characterization of the structure of the sequence of numbers. We can explain, in Dedekind's sense, that a set M has the structure of the sequence of numbers with regard to a mapping φ (from M to itself) if φ is uniquely invertible and there is an element a of M which is not in the image of φ, and which has the property that no proper subset of M exists that contains a as well as $\varphi(c)$ for every element c. Here again to stipulate a narrower concept of subset can have the consequence that the above condition is satisfied by models to which we would not attribute the structure of the number sequence based on the unrestricted condition. This state of affairs results likewise if we use an axiom system to characterize the number sequence instead of the explicit definition of structure. In the usual form of such an axiom system one has the axiom of complete induction in which the general concept of proposition (or predicate) occurs. If the axiomatics are formally sharpened this axiom is replaced by a formal inference principle in which the range of the allowed predicates is formally delimited by a substitution rule. This restriction also allows for the possibility of models for number theory that satisfy all statements provable within the formal framework, but that deviate from the structure of the number sequence when they are considered on their own. Again it was Skolem who pointed out this state of affairs of the "non-characterizability" of the number sequence by a formalized axiom system, using drastic examples.

On the whole, after what has been said so far the success of attempting to make a theory sharper and more precise using axioms might appear highly questionable. But the circumstance is not taken into consideration thereby that there are frameworks that classical mathematics has no reasons to transgress – as has been shown by the axiomatic and logical analysis of mathematical theories. The domain of sets and functions, e. g., as it is provided by the axioms of set theory, is closed in such a way that the formal axiomatic restriction is hardly palpable when forming concepts and conducting proofs.

Furthermore, the set theoretic theorems are not affected by the relativity that holds for the general concepts. This relativity of course does not mean that the continuum is shown to be uncountable in *one* framework for set theory and countable in *another*. The discrepancy consists rather only in the fact that the totality of things that are represented in a set theoretic system, e. g., the set of subsets of the number sequence, can be countable in a more comprehensive system; but then it does not act there as a representation of that set of subsets, and thus it is impossible to map the numbers uniquely to the sets of numbers. In such a way the cardinality theorems of Cantor's set theory are invariant with respect to the axiomatic framework, despite the relativity of the set concepts.

Of course, it must be conceded that this relativity brings the circumstance more forcefully into our consciousness that the higher cardinalities in set theory are only intended, so to speak, but not properly constructed. In this sense the levels of cardinalities are in a certain way unreal.

The awareness of this state of affairs is often explained by saying that everything in mathematics is countable in "actuality." But this formulation is misleading in so far as it does not take into account the fundamental fact which is expressed both in operative mathematics and in the consideration of formal axiom systems, namely that mathematical thinking in principle transcends every countable system. The framework for the mathematical formation of concepts is the open, contentual second number class both when proceeding constructively and within a theory of types, if these are not restricted in an arbitrary fashion, or also in the sequence of the ascending systems of axiomatic set theory. It represents something that is in the proper sense uncountable, and sure enough it cannot be addressed as a particular mathematical structure.

We are reminded here of the fact that also the number sequence is presented to us originally as an open domain compared to which the number sequence that we address as a structure is somehow unreal. The difference with respect to the second number class is that the openness of the number sequence is only due to the incompletedness of the iterations of a single process, whereas the openness of the second number class is due to the incompletedness of the formations of concepts.

That the unreal character of particular uncountable structures is much more noticeable than the unreal character that lies in the conception of the number sequence as a structure is due to the fact that our concept of a formal theory intends exactly the same kind of infinity as that of the number sequence.

Chapter 22

Bernays Project: Text No. 23

Comments on Ludwig Wittgenstein's *Remarks on the Foundations of Mathematics* (1959)

Betrachtungen zu Ludwig Wittgensteins *Bemerkungen über die Grundlagen der Mathematik*

(*Ratio* 2, pp. 1–18; repr. in [?], pp. ■ –■
repr. in *Abhandlungen*, pp. 119–141)

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I

The following comments are concerned with a book that is the second part of the posthumous publications of selected fragments from Wittgenstein in which he sets forth his later philosophy.[1] The necessity of making a selection

[1]The book was originally published in German, with English translation attached. All pages and numbers quoted refer to [?].

and the fragmentary character noticeable at points are not overly problematic, since in his publications Wittgenstein refrains from a systematic presentation anyway and expresses his thoughts in separate paragraphs—jumping frequently from one theme to another. In fairness to the author it has to be admitted, however, that he would doubtlessly have made extensive changes in the arrangement and selection of the material had he been able to complete the work himself. The editors of the book have, by the way, greatly facilitated an overview over the book by providing a very detailed table of contents and an index. The preface provides information about the origins of the different parts I–V.

Compared with the viewpoint of the *Tractatus*, which considerably influenced the initially rather extreme doctrine of the Vienna Circle, Wittgenstein's later philosophy represents a rectification and clarification in essential respects. In particular, the very schematic conception of the structure of scientific language—especially of the composition of statements out of atomic propositions—is here dropped. What remains, however, is the negative attitude towards speculative thinking and the constant tendency to disillusionize.

Thus Wittgenstein himself says, evidently with his own philosophy in mind (p. 63, No. 18): "Finitism and behaviorism are quite similar trends. Both say, but surely, all we have here is .... Both deny the existence of something, both with a view to escaping from a confusion. What I am doing is, not to show that calculations are wrong, but to subject the *interest* of calculations to a test." Later on he explains (p. 174, No. 16): "It is my task, not to attack Russell's logic from *within*, but from without. That is to say: not to attack it mathematically—otherwise I should be doing mathematics—but its position, its office. My task is, not to talk about (e. g.) Gödel's proof, but to pass it by."

As one can see, a certain jocularity of expression is not missing in Wittgenstein; and in the numerous parts written in dialogue form he often enjoys acting the rogue.

On the other hand, he does not lack *esprit de finesse*, and his remarks contain many implicit suggestions, in addition to what is explicitly stated.

Throughout, however, two problematic tendencies play a role. The first is to explain away the actual role of thinking—of reflective intending—along behavioristic lines. It is true that David Pole, in his interesting account and exposition of Wittgenstein's later philosophy,[2] denies that Wittgenstein is a supporter of behaviorism. And this contention is justified insofar as Wittgenstein certainly does not deny the existence of mental experiences of feeling, perceiving and imagining. Still, with regard to thinking his attitude is behavioristic after all. In this connection he tends towards a short circuit everywhere. Images and perceptions are, in each case, supposed to be followed immediately by behavior. "We do it like this," that is usually the last word of explanation—or else he appeals to some need as an anthropological fact. Thought, as such, is left out. Along these lines, it is characteristic that a "proof" is conceived of as a "picture" or "paradigm;" and although Wittgenstein is critical of the method of formalizing proofs he keeps using the formal method of proof in Russell's system as an example. Instances of mathematical proofs proper, which are neither just calculations nor result merely from exhibiting a figure or a formal procedure, do not occur at all in this book on the foundations of mathematics, a book a major part of which addresses the question as to what proofs really are; and that in spite of the fact that the author has evidently concerned himself with many mathematical proofs.

One passage may be mentioned as characteristic for Wittgenstein's behavioristic attitude, and as an illustration of what is meant here by a short circuit. Having rejected as unsatisfactory various attempts to characterize inference, he continues (p. 8, No. 17): "This is why it is necessary to look and see how we carry out inferences in the practice of language; what kind of procedure in the language-game inferring is. For example: a regulation says: 'All who are taller than five foot six are to join the ... section.' A clerk reads out the men's names and their heights. Another allots them to such-and-such sections. 'N. N. five foot nine.' 'So N. N. to the ... section.' That is inference." It is evident here that Wittgenstein is satisfied only with the characterization of an inference in which one passes directly from a linguistic specification of the premises to an action; one in which, therefore, the specifically reflective element is eliminated. Language, too, appears under the aspect of behavior ("language-game").

The other problematic tendency has its source in the program—already present in Wittgenstein's earlier philosophy—of separating strictly the linguistic and the factual, a separation also present in Carnap's $_a$**Logical** $_a$*Syntax of Language*. That this separation should have been retained in the new ver-

[2] *Vide* [**?**].

sion of Wittgenstein's doctrine does not go without saying because here the approach, compared with the earlier one, is in many respects less rigid. Some signs of change are, in fact, apparent, as for instance on p. 119, No. 18: "It is clear that mathematics as a technique for transforming signs for the purpose of prediction has nothing to do with grammar." Elsewhere (p. 125, No. 42), he even speaks of the "synthetic character of mathematical propositions." As he puts it: "It might perhaps be said that the synthetic character of propositions of mathematics appears most obviously in the unpredictable occurrence of the prime numbers. But their being synthetic (in this sense) does not make them any the less *a priori* .... The distribution of primes would be an ideal example of what could be called synthetic *a priori*, for one can say that it is at any rate not discoverable by an analysis of the concept of a prime number." As we can see, Wittgenstein turns here from the Vienna Circle concept of "analyticity" back to a conception that is more Kantian.

A certain rapprochement to Kant's conception can also be found in Wittgenstein's view that mathematics first determines the character or "creates the forms of what we call facts" (see p. 173, No. 15). Along these lines, Wittgenstein strongly opposes the view that the propositions of mathematics have the same function as empirical propositions. At the same time, he emphasizes on a number of occasions that the applicability of mathematics, in particular of arithmetic, depends on empirical conditions; e. g., on p. 14, No. 37 he says: "This is how our children learn sums; for one makes them put down three beans and then another three beans and then count what is there. If the result at one time were five, at another seven ..., then the first thing we said would be that beans were no good for teaching sums. But if the same thing happened with sticks, fingers, lines and most other things, that would be the end of all sums.—'But shouldn't we then still have $2 + 2 = 4$?'—This sentence would have become unusable."

Nevertheless, statements like the following remain important for Wittgenstein's conception (p. 160, No. 2): "If you know a mathematical proposition, that's not to say that you yet know *anything*." He repeats this twice, at short intervals, and adds: "I. e., the mathematical proposition is only to supply a framework for a description." In the manner of Wittgenstein one could ask back here: "Why is the person in question *supposed* to still know nothing? What need is expressed by this 'supposed to'?" It appears that only a philosophical preconception leads to this requirement, the view, namely, that there can exist only *one* kind of factuality: that of concrete reality. This view corresponds to a kind of nominalism that also plays a role elsewhere in discussions on the philosophy of mathematics. In order to justify such a nominalism Wittgenstein would, at the very least, have to go back further than he does in this book. In any case, he cannot appeal to our actual attitudes here. And indeed, he attacks our tendency to regard arithmetic, say, "as the natural history of the domain of numbers" (see p. 117, No. 13, and p. 116, No. 11). Then again, he is not fully definite on this point. He asks himself (p. 142, No. 16) whether it already constitutes "mathematical alchemy" to claim that mathematical propositions are regarded as statements about mathematical objects. But he also notes: "In a certain sense it is not possible to appeal to the meaning of signs in mathematics, just because it is only mathematics that gives them their meaning. What is typical of the phenomenon I am talking about is that a *mysteriousness* about some mathematical concept is not *straight away* interpreted as an erroneous conception, as a mistake of ideas; but rather as something that is at any rate not to be despised, is perhaps even rather to be respected. All that I can do, is to show an easy escape from this obscurity and this glitter of the concepts. Strangely, it can be said that there is so to speak a solid core to all these glistening concept-formations. And I should like to say that that is what makes them into mathematical productions."

One may doubt whether Wittgenstein has indeed succeeded in exhibiting "an easy escape from this obscurity;" one may even be inclined to think that the obscurity and the "mysteriousness" really have their origin in a philosophical conception, or in the philosophical language used by Wittgenstein.

His fundamental separation of the sphere of mathematics from the sphere of the factual comes up in several passages in the book. In this connection, Wittgenstein often speaks with a matter-of-factness that contrasts strangely with his readiness to doubt so much of what is generally accepted. A passage on p. 26, No. 80 is typical for this; he says: "But of course you can't get to know any property of the material by imagining." Again on p. 29, No. 98, we can read: "I can calculate in the imagination, but not experiment." From the point of view of common experience, all of this is certainly not obvious. An engineer or technician has, no doubt, just as lively a mental image of materials as a mathematician has of geometrical curves; and the mental image which any one of us may have of a thick iron rod is doubtlessly such as to make it clear that the rod could not be bent by a light pressure of the hands. Moreover, in the case of technical invention a major role is definitely played by experimenting in the imagination. It seems that Wittgenstein simply, without critical reflection, uses a philosophical schema which distinguishes the *a priori* from the empirical. To what extent and in which sense this distinction—so important particularly in the Kantian philosophy—is justified will not be discussed here; but its introduction, particularly at the present moment, should not be taken very lightly. With regard to the *a priori*, Wittgenstein's viewpoint differs from Kant's, incidentally, insofar as it includes the principles of general mechanics in the sphere of the empirical. Thus he argues, e. g. (p. II 4, No. 4): "Why are the Newtonian laws not axioms of mathematics? Because we could quite well imagine things being otherwise ... To say of a proposition: 'This could be imagined otherwise' ... ascribes the role of an empirical proposition to it." The notion of "being able to imagine otherwise," also used by Kant, has the unfortunate difficulty of being ambiguous; the impossibility of imagining something may be meant in various senses. This difficulty occurs particularly in the case of geometry, as we will discuss later.

The tendency of Wittgenstein, mentioned earlier, to recognize only one kind of factuality becomes evident not only with regard to mathematics, but also with respect to any phenomenological consideration. Thus he discusses the proposition that white is lighter than black (p. 30, No. 105), and explains it by saying that black serves us as a paradigm for what is dark, and white as a paradigm for what is light, which makes the statement one without content. In his opinion statements about differences in brightness have content only when they refer to specific visually given objects; and for the sake of clarity one should not even talk about differences in the brightness of colors. This attitude obviously precludes a descriptive theory of colors.

Actually, phenomenological considerations should be congenial to Wittgenstein, one might think. This is suggested by the fact that he likes to draw examples, for the purpose of comparison, from the field of art. It is only his philosophical program, then, that prevents the development of an explicitly phenomenological viewpoint.

This aspect is an example of how Wittgenstein's methodology is aimed at eliminating a very great deal. He sees himself in the part of the free thinker who combats superstition. However, the latter's goal is freedom of the mind, whereas it is exactly the mental that Wittgenstein restricts in many ways—by means of a mental asceticism in the service of an irrationality whose goal is quite indeterminate.

Yet this tendency is by no means as extreme in the later philosophy of Wittgenstein's as it was in the earlier form. One may already gather from the passages quoted above that he was probably on the way to giving mental contents more of their due.

A related fact may be that, in contrast to the simply assertoric form of philosophical statements in the *Tractatus*, a largely aporetical attitude prevails in the present book. With respect to philosophical pedagogics this presents a danger, however, especially as Wittgenstein's philosophy is exerting such a strong attraction on younger minds. The old Greek observation that philosophical contemplation often begins in philosophical wonder[3] misleads many philosophers today into believing that the cultivation of astonishment is in itself a philosophical achievement. One may surely have one's doubts about the soundness of a method which trains young philosophers in wondering, as it were. Wondering is heuristically fruitful only when it is the expression of an instinct for research. Clearly it cannot be demanded of any philosophy to make comprehensible everything that is astonishing. But perhaps it is characteristic for the various philosophical viewpoints what they accept as ultimate that which is astonishing. In Wittgenstein's philosophy it is, as far as epistemological questions are concerned, sociological facts. A few quotations may serve as illustrations of this point (p. 13, No. 35): "... how does it come about that all men ... accept these patterns as proofs of these propositions?—It is true, there is a great—and interesting—agreement here." (p. 20, No. 63): "... it is a peculiar procedure: I *go through* the proof and then accept its result.—I mean: this is simply what we *do*. This is use and custom among us, or a fact of our natural history." (p. 23, No. 74): "If you talk about *essence*—, you are merely noting a convention. But here one would like to retort: there is no greater difference than that between a proposition about the depth of the essence and one about—a mere convention. But what if I reply: to the *depth* that we see in the essence there corresponds the *deep* need for the convention." (p. 122, No. 30): "Do not look at the proof as a procedure that *compels* you, but as one that *guides* you ... But how does it come about that it guides *each one* of us in such a way that we agree in the influence it has on us? Well, how does it come about that we agree in *counting*? 'That is just how we are trained' one may say, 'and the agreement produced in this way is carried further by the proofs.'"

II

So much for a general characterization of Wittgenstein's observations.

[3] θαυμάζειν.

But their content is by no means exhausted by the general philosophical aspects that have been mentioned; various specific questions of a basic philosophical nature are also discussed in detail. In what follows, we shall deal with their principal aspects.

Let us begin with a question that is connected with a problem previously touched on, namely the distinction between the *a priori* and the empirical: the question of geometrical axioms. Wittgenstein does not deal specifically with geometrical axioms as such. Instead, he raises the general question as to how far the axioms of an axiomatized mathematical system should be self-evident; and he takes as his example the parallel axiom. Let us quote a few sentences from his discussion of this subject (p. 113, No. 2ff): "What do we say when we are presented with such an axiom, e. g., the parallel axiom? Has experience shown us that this is how it is? ... Experience plays a part; but not the one we would *immediately expect*. For we haven't made experiments and found that in reality only *one* straight line through a given point fails to intersect another. And yet the proposition is evident.—Suppose I now say: it is quite indifferent why it is evident. It is enough that we accept it. All that is important is how we use it .... When the words for e. g. the parallel axiom are given ... the kind of use this proposition has and hence its sense are as yet quite undetermined. And when we say that it is evident, this means that we have already chosen a definite kind of employment for the proposition without realizing it. The proposition is not a mathematical axiom if we do not employ it precisely *for this purpose*. The fact, that is, that here we do not make experiments, but accept the self-evidence, is enough to fix the employment. For we are not so naive as to make the self-evidence count in place of the experiment. It is not our finding the proposition self-evidently true, but our making the self-evidence count, that makes it into a mathematical proposition."

In discussing these remarks, it must first be realized that we need to distinguish two things: whether we recognize an axiom as geometrically valid, or whether we choose it as an axiom. The latter is, of course, not determined by the wording of the proposition. But here we are concerned merely with a technical question concerning the deductive arrangement of propositions. What interests Wittgenstein, on the other hand, is surely the recognition of the proposition as geometrically valid. It is along these lines that Wittgenstein's assertion ("that the recognition is not determined by the words") must be considered; and its correctness is at the very least not immediately evident. He says simply: "For we have not made experiments." Admittedly, there has been no experimenting in connection with the formulation of the parallel axiom considered by him; this formulation does not lend itself to this purpose. However, within the framework provided by the other geometrical axioms the parallel axiom is equivalent to any one of the following statements of metrical geometry: "In a triangle the sum of the angles is equal to two right angles. In a quadrilateral in which three angles are right angles the fourth angle is also a right angle. Six congruent equilateral triangles with a common vertex P (lying consecutively side by side) exactly fill up the neighborhood of point P." Such propositions—in which, it should be noted, there is no mention of the infinite extendibility of a straight line—can definitely be tested by experiment. And as is well known, Gauss did in fact check experimentally the proposition about the sum of the angles of a triangle, thereby making use of the assumption of the linear propagation of light, to be sure. In addition, this is not the only possibility for an experiment. Hugo Dingler, in particular, has shown that for the concepts of straight line, plane, and right angle there exists a natural and, as it were, compulsory kind of experimental realization. By means of such an experimental realization of geometrical concepts, statements like the second one above, especially, can be experimentally tested with great accuracy. Moreover, in a less accurate way they are checked by us all the time implicitly in the normal practice of drawing figures. Our instinctive estimations of lengths and of the sizes of angles, too, can be regarded as the result of manifold experiences; and propositions such as those mentioned above must, after all, agree with those instinctive estimations.

It cannot be upheld, therefore, that our experience plays no role in the acceptance of propositions as geometrically valid. But Wittgenstein does not mean that either, as becomes clear from what follows immediately after the passage quoted (p. 114, Nos. 4 and 5): "Does experience tell us that a straight line is possible between any two points? ... It might be said: *imagination* tells us. And the germ of truth is here; only one must understand it right. *Before* the proposition the concept is still pliable. But might not experience force us to reject the axiom?! Yes. And nevertheless it does not play the role of an empirical proposition. ... Why are the Newtonian laws not axioms of mathematics? Because we could quite well imagine things being otherwise. ... Something is an axiom, *not* because we accept it as extremely probable, nay certain, but because we assign it a particular function, and one that conflicts with that of an empirical proposition. ... The axiom, I should like to say, is a different part of speech." Further on (p. 124, No. 35), he says:

"What about e. g. the fundamental laws of mechanics? If you understand them you must know how experience supports them. It is otherwise with the propositions of pure mathematics."

In support of these remarks, it must certainly be conceded that experience alone does not force the theoretical acceptance of a proposition. An exact theoretical approach must always go beyond the facts of experience in its conception.

Nevertheless, the view that in this respect there exists a sharp dividing line between mathematical propositions and the principles of mechanics is by no means justified. In particular, the last quoted assertion that, in order to understand the basic laws of mechanics, the experience on which they are based must be known can hardly be upheld. Of course, when mechanics is taught at the university it is desirable that the empirical starting points be made clear. But this is not done with a view towards the theoretical and practical manipulation of the laws, but for being epistemologically alert and with an eye to the possibilities of eventually necessary modifications of the theory. An engineer or productive technician who wants to become skilled in mechanics and capable of handling its laws does not have to concern himself with how we came upon these laws. With respect to these laws applies, moreover, the same as what Wittgenstein so frequently emphasizes with respect to mathematical laws: that the facts of experience relevant for the empirical motivation of these propositions by no means make up the content of what is asserted in the laws. What is important instead for learning to handle the mechanical laws is to become familiar with the concepts involved and to make them intuitive to oneself in some way. This kind of acquisition is not only practically, but also theoretically significant: the theory is fully assimilated only in the process of rationally shaping and extending it, to which it is subsequently subjected. With regard to mechanics, most philosophers and many of us mathematicians have little to say in this connection, not having acquired mechanics in the said manner.—What distinguishes the case of geometry from that of mechanics is the (philosophically in a sense accidental) circumstance that the acquisition of the world of concepts and of corresponding intuitions is for the most part already completed in an (at least for us) unconscious stage of mental development.

Ernst Mach's opposition to a rational foundation of mechanics has its justification insofar as such a foundation endeavors to pass over the role of experience in arriving at the principles of mechanics. We must keep in mind that the concepts and principles of mechanics comprise, as it were, an extract of experience. On the other hand, it would be unjustified to simply reject all efforts at constructing mechanics rationally on the basis of this criticism.

What is special about geometry is the phenomenological character of its laws, and hence the significant role played by intuition. Wittgenstein points to this aspect only in passing: "Imagination tells us. And the germ of truth is here; only one must understand it right" (p. 8). The term "imagination" is very general, and what he says at the end of the second sentence is a qualification which shows that the author feels the topic of intuition to be rather tricky. Indeed, it is very difficult to characterize the epistemological role of intuition in a satisfactory way. The sharp opposition between intuition and concepts as it occurs in Kant's philosophy does not, on closer inspection, appear to be justified. When considering geometrical thinking in particular it is difficult to separate sharply the part played by intuition from that played by the conceptual; since we find here a formation of concepts that is in a certain sense guided by intuition—one that, in the sharpness of its intentions, goes beyond what is intuitive in the strict sense, but also cannot be understood adequately if it is considered apart from intuition. What is strange is that Wittgenstein assigns no specific epistemological role to intuition in spite of the fact that his thinking is dominated by the visual. For him a proof is always a picture. At one time he gives a mere figure as an example of a geometrical proof. It is also striking that he never talks about the intuitive evidence of topological facts, such as the fact that the surface of a sphere divides (the rest of) space into an interior and an exterior part, in such a way that a curve which connects an inside point with an outside point always passes through a point on the surface of the sphere.

Questions concerning the foundations of geometry and its axioms belong primarily to the domain of general epistemology. What is called research on the foundations of mathematics in the narrower sense today is directly mainly at the foundations of arithmetic. Here one tends to eliminate, as much as possible, what is special about geometry by separating the latter into an arithmetical and a physical side. We shall leave aside the question of whether this procedure is justified; that question is not discussed by Wittgenstein. In contrast, he deals in great detail with basic questions concerning arithmetic. Let us now take a closer look at his remarks concerning this area of inquiry.

The viewpoint from which Wittgenstein looks at arithmetic is not the usual one of a mathematician. More than with arithmetic itself, Wittgenstein is concerned with theories of the foundations of arithmetic (in particular with Russell's theory). With regard to the theory of numbers, especially, his examples seldom go beyond the numerical. An uninformed reader might well conclude that the theory of numbers consists almost entirely of numerical equations—which, actually, are normally not regarded as propositions to be proved, but as simple statements. Wittgenstein's treatment is more mathematical in the sections where he discusses questions of set theory, such as concerning denumerability and non-denumerability, as well as concerning the theory of Dedekind cuts.

Throughout, Wittgenstein advocates the standpoint of strict finitism. In so doing he considers the various types of problems concerning the infinite that there are from a finitist viewpoint, in particular the problems of the *tertium non datum* and of impredicative definitions. The quite forceful and vivid account he provides in this connection is well suited for introducing the finitist's position to those still unfamiliar with it. However, it hardly contributes anything essentially new to the debate; and those who hold the position of classical mathematics in a deliberate way will scarcely be convinced by it.

Let us discuss a few points in more detail. Wittgenstein deals with the question of whether in the infinite expansion of π a certain sequence of numbers ϕ such as, say, "777," ever occurs. Along Brouwer's lines, he draws attention to the possibility that this question may not as yet have a definite answer. Along these lines he then says (p. 138, No. 9): "However queer it sounds, the further expansion of an irrational number is a further development of mathematics." This formulation is obviously ambiguous. If it merely means that any determination of a not yet calculated decimal place of an irrational number is a contribution to the development of mathematics, then every mathematician will agree with it. But since the statement is said to be "queer sounding," something else is most likely meant. Perhaps it is that the course of the development of mathematics at a given time is undecided, and that this undecidedness can have to do with the continuation of the expansion of an irrational number given by a definition; so that the decision as to what digit is to be put at the ten-thousandth decimal place of π would be a contribution to the direction of the history of thought. But such a view is not appropriate even according to Wittgenstein's own position, for he says (p. 138, No. 9): "The question … changes its status when it becomes decidable." Now, it is a fact that the digits in the decimal expansion of π are decidable up to any chosen decimal place. Hence the suggestion about the further development of mathematics does not contribute anything to our understanding of the situation in the case of the expansion of π. One can even say the following: Suppose we maintained firmly that the question of the occurrence of the sequence of numbers ϕ is undecidable, then this would imply that the figure ϕ occurs nowhere in the expansion of π; for if it did, and if k was the decimal place that the last digit of ϕ had on its first occurrence in the decimal expansion of π, then the question whether the figure ϕ occurs before the $(k+1)$th place would be a decidable question and could be answered positively; thus the initial question would be decidable, too. (Incidentally, this argument does not require the principle of *tertium non datur*.)

Further on in the text, Wittgenstein comes back repeatedly to the example of the decimal expansion of π. At one point in particular (p. 185, No. 34) we find an assertion that is characteristic for his position: "Suppose that people go on and on calculating the expansion of π. So God, who knows everything, knows whether they will have reached a '777' by the end of the world. But can his *omniscience* decide whether they *would* have reached it after the end of the world? It cannot. ... Even for him the mere rule of expansion cannot decide anything that it does not decide for us."

That is certainly not convincing. If we concede the idea of a divine omniscience at all, then we would certainly ascribe to it the ability to survey at *one* glance a totality every single element of which is in principle accessible to us. Here we must pay special attention to the double role the recursive definition plays for the decimal expansion: as the definitory determination of decimal fractions, on the other hand; and as the means for the "effective" calculation of decimal places, on the other hand. If we here take "effective" in the usual sense, then it is true that even a divine intelligence can *effectively* calculate nothing other than what we are able to effectively calculate ourselves (no more than it world be capable of carrying out the trisection of an angle with ruler and compass, or of deriving Gödel's underivable proposition in the corresponding formal system). But it is not to be ruled out that this divine intelligence would be able to survey in some other (not humanly effective) manner all the possible calculation results of the application of a recursive definition.

In his criticism of the theory of Dedekind cuts, Wittgenstein's main argument is that in this theory an extensional approach is mixed up with an intensional approach. This criticism is, in fact, appropriate with respect to certain versions of the theory, namely those in which the goal is to create the appearance of a stronger constructive character of the procedure than is actually achieved. If one wants to introduce the cuts not as mere sets of numbers, but as defining arithmetical laws for such sets, then either one has to use a very vague concept of "law," thus gaining little; or, if one's aim is to clarify that concept, one is confronted with the difficulty which Hermann Weyl has termed the vicious circle in the foundation of analysis. This difficulty was sensed instinctively by a number of mathematicians for a while, who consequently advocated a restriction of the procedure of analysis. Such a criticism of impredicative formations of concepts plays a considerable role in discussions on the foundations of mathematics even today. However, the difficulties disappear if an extensional standpoint is maintained consistently. Moreover, Dedekind's conception can certainly be understood in that sense, and was probably meant that way by Dedekind himself. All that is required is that one accepts, besides the concept of number itself, also the concept of a set of natural numbers (and, consequently, the concept of a set of fractions) as an intuitively significant concept that is not in need of a reduction. This does bring with it a certain moderation with respect to the goal of arithmetizing analysis, and thus geometry too. But—as one could ask in a Wittgensteinian manner—must geometry be arithmetized completely anyway? Scientists are often very dogmatic in their attempts at reductions. They are often inclined to treat such an attempt as completely successful even if it does not succeed in the manner intended, but only to a certain extent or within a certain degree of approximation. Confronted with such attitudes, considerations of the kind suggested in Wittgenstein's book can be very valuable.

Wittgenstein's more detailed discussion of Dedekind's proof procedure is not satisfactory. Some of his objections can be disposed of simply by giving a clearer account of Dedekind's line of thought.

In Wittgenstein's discussion of denumerability and non-denumerability, the reader has to bear in mind that by a cardinal number he always means a finite cardinal number, and by a series always one of the order type of the natural numbers. His polemics against the theorem stating the non-denumerability of the totality of real numbers is unsatisfactory primarily insofar as the analogy between the concepts "non-denumerable" and "infinite" is not exhibited clearly. Corresponding to the way in which "infinitness of a totality G" can be defined as the property that to any finite number of things in G one can always find a further thing in it, the non-denumerability of a totality G is defined as the property that to every denumerable sub-totality one can always find an element of G not yet contained in the sub-totality. Understood in that sense, the non-denumerability of the totality of real numbers is demonstrated by means of the diagonal procedure; and there is nothing foisted in here, as would appear to be the case according to Wittgenstein's argument. The theorem of the non-denumerability of the totality of real numbers is, as such, independent of the comparison of transfinite cardinal numbers. Besides—and this is often neglected—, for that theorem there also exist other, more geometrical proofs than the one involving the diagonal procedure. In fact, from the point of view of geometry we can call this a rather gross fact. It is strange, also, to find the author raising a question like the following: "For how do we make use of the proposition: 'There is no greatest cardinal number.'? ... First and foremost, notice that we ask the question at all; this points to the fact that the answer is not ready to hand" (p. 57, No. 5). One should think that one needs not search long for an answer here. Our entire analysis, with all its applications in physics and technology, rests on the infinity of the number series. Probability theory and statistics, too, make constant implicit use of that infinity. Wittgenstein acts as if mathematics existed almost solely for the purposes of housekeeping.

The finitist and constructive attitude taken on the whole by Wittgenstein concerning the problems of the foundations of mathematics conforms to general tendencies in his philosophy. It cannot be said, however, that he finds confirmation for his position in the foundational situation in mathematics. All he shows is how this position is to be applied when dealing with the questions under dispute. In general, it is characteristic for the situation regarding the foundational problems that the results obtained so far do not favor either of the two main philosophical views opposing each other—the finitist-constructive view and the "Platonist"-existential view. Each of the two sides can advance arguments against the other. Yet, the existential conception has the advantage that it enables us to appreciate investigations aimed at the establishment of constructive methods (just as in geometry the investigation of constructions with ruler and compass has significance even for a mathematician who admits other methods of construction), while for a strict constructivist a large part of classical mathematics simply falls by the wayside.

Wittgenstein's observations concerning the foundational issues of the role of formalization, the reduction of number theory to logic, and the question of consistency are to some degree independent of partisanship in the above mentioned opposition. His views here show more independence, hence these considerations are of greater interest.

With regard to the question of consistency, in particular, he asserts what has meanwhile also been stressed by various other theorists in the field of foundational studies: that within the framework of a formal system a contradiction should not be seen so exclusively as objectionable, and that a formal system in itself can still be of interest even if it leads to a contradiction. It should be noted, on the other hand, that in the earlier systems of Frege and Russell the contradiction arises already within a few steps, as it were directly from the basic structure of the system. In addition, much of what Wittgenstein says in this connection overshoots the mark by a long way. Unsatisfactory, in particular, is his frequently used example of the derivability of contradictions by admitting division by zero. (One need only consider the justification for the rule of reduction in order to see that it is not applicable in the case of the factor zero.)

In any case, Wittgenstein acknowledges the importance of demonstrating consistency. However, it is doubtful whether he is sufficiently aware of the role played by the requirement of consistency in proof-theoretic investigations. Thus his discussion of Gödel's theorem of non-derivability and its proof, in particular, suffers from the defect that Gödel's quite explicit premise concerning the consistency of the formal system under consideration is ignored. A fitting comparison, drawn by Wittgenstein in connection with Gödel's theorem, is that between a proof of formal unprovability, on the one hand, and a proof of the impossibility of a certain construction with ruler and compass, on the other. Such a proof, says Wittgenstein, contains an element of prediction. But the remark which follows is strange (p. 52, No. 14): "A contradiction is unusable as such a prediction." As a matter of fact, such impossibility proofs usually proceed via the derivation of a contradiction.

In his remarks on the theory of numbers, Wittgenstein shows a noticeable reserve towards Frege's and Russell's foundation of number theory, such as was not present in the earlier stages of his philosophy. Thus on one occasion (p. 67, No. 4) he says: "... the logical calculus is only—frills tacked on to the arithmetical calculus." This thought has perhaps never been formulated as strikingly as here. It might be good, then, to reflect on the sense in which the claim holds true. There is no denying that the attempt to incorporate arithmetical and, in particular, numerical propositions into logistic has been successful. That is to say, it has proved possible to formulate these propositions in purely logical terms and, on the basis of this formulation, to prove them within the framework of logistic. It is open to question, however, whether this result should be regarded as yielding a proper philosophical understanding of arithmetical propositions. If we consider, e. g., the logistic proof of an equation such as $3 + 7 = 10$, we can see that within the proof we have to carry out quite the same comparative verification that occurs in our usual counting. This necessity comes to the fore particularly clearly in the formalized version logic; but it is also present if we interpret the content of the formula logically. The logical definition of three-numberedness, for example, is structurally so constituted that it contains within itself, as it were, the element of three-numberedness. For the three-numberedness of a predicate P (or of the class that forms the extension of P) is defined in terms of the condition that there exist things x, y, z having the property P and differing from each other pairwise, and further that everything having the property P is identical with x or y or z. Now, the conclusion that for a three-numbered predicate P and a seven-numbered predicate Q, in the case where these predicates do not apply to anything jointly, the disjunction $P \vee Q$ is a ten-numbered predicate requires for its justification just the kind of comparison that is used in elementary calculation—only that here an additional logical apparatus (the "frills") comes into play as well. When this is clearly realized, it appears that the proposition in predicate logic is valid because $3 + 7 = 10$ holds, not vice versa.

In spite of the possibility of incorporating it into logistic, arithmetic constitutes thus the more abstract (the "purer") schema; and this seems paradoxical only because of the traditional, but on closer examination unjustified, view according to which logical generality is the highest generality in every respect.

It might be good to look at the matter from yet another side as well. According to Frege, a cardinal number is to be defined as the property of a predicate. This view is already problematic with respect to the normal use of the number concept; for in many contexts in which a number is determined the specification of a predicate of which it is the property appears to be highly forced. It should be noted, in particular, that numbers occur not only in statements, but also in directions and commands—for example, when a housewife says to an errand-boy: "Fetch me ten apples." Moreover, the theoretical elaboration of this view is not without its complications either. In general, a definite number does not belong to a predicate as such, but only relative to a domain of objects, a universe of discourse (even apart from the many cases of extra-scientific predicates to which no determinate number can be ascribed at all). Thus it would be more appropriate to characterize a number as a relation between a predicate and a domain of individuals. To be sure, in Frege's theory this complication does not occur because he presupposes what might be called an absolute domain of individuals. But as we know now, it it is precisely this approach that leads to the contradiction noted by Russell. Apart from that, the Fregean conception of the theory of predicates, according to which the courses of values of predicates are treated as things on the same level as ordinary individuals, already constitutes a clear deviation from customary logic, understood as the theoretical construction of a general framework. The idea of such a framework has retained its methodological importance, and the question as to its most appropriate form is still one of the main problems in foundational research. However, with regard to such a framework one can speak of a "logic" only in an extended sense. Logic in its usual sense, in which it merely means the specification of the general rules for deductive reasoning, must be distinguished from it.

Yet Wittgenstein's criticism of the incorporation of arithmetic into logic is not advanced in the sense that he acknowledges arithmetical propositions as stating facts that are *sui generis*. Instead, his tendency is to deny that such propositions express facts at all. He even declares it to be the "curse of the invasion of mathematics by mathematical logic that now any proposition can be represented in a mathematical symbolism, and this makes us feel obliged to understand it. Although of course this method of writing is nothing but the translation of vague ordinary prose" (p. 155, No. 46). In fact, he recognizes calculating only as an acquired skill with practical utility. More particularly, he seeks to explain away what seems factual about arithmetic as definitional. He asks, for instance (p. 33, No. 112): "What am I calling 'the multiplication 13×13'? Only the correct pattern of multiplication, at the end of which comes 169? Or a 'wrong multiplication' too?" Elsewhere, too, he raises the question as to what it is that we "call calculating" (p. 97, No. 73). And on p. 92, No. 58 he argues: "Suppose it were said: 'By calculating we get acquainted with the properties of numbers.' But do the properties of numbers *exist* outside the calculating?" The tendency is, apparently, to take correct additions and multiplications as defining calculating, thus to characterize them as "correct" in a trivial sense. But this doesn't work out in the end, i. e., one cannot express in this way the general facts that hold in terms of the arithmetic relations of numbers. Let us take, say, the associativity of addition. It is certainly possible to fix by definition the addition of single digits. But then the strange fact remains that the addition $3+(7+8)$ gives the same result as $(3+7)+8$, and the same holds whatever numbers replace $3, 7, 8$. With respect to possible definitions the number-theoretic expressions are, so to speak, over-determined. It is actually on this kind of over-determinateness that many of the checks available in calculating are based.

Occasionally Wittgenstein raises the question as to whether the result of a calculation carried out in the decimal system also holds for the comparison of numbers by means of their direct representation as sequences of strokes. The answer to this question is to be found in the usual mathematical justification of the method of calculating with decadic figures. But Wittgenstein does touch upon something fundamental here: the proofs for the justification of the decadic rules of calculation rest, if they are given in a finitist way, upon the assumption that every number that can be formed decadically can also be produced in the direct stroke notation, and that the operations of concatenation etc., as well as of comparison can always be performed with such stroke sequences. What this shows is that even finitistic number theory is not in the full sense "concrete," but uses idealizations.

The previously mentioned statements in which Wittgenstein speaks of the synthetic character of mathematics are in apparent contrast with his tendency to regard numerical calculation as merely definitional, as well as with his denial that arithmetical propositions are factual in the first place. Note in this connection the following passage (p. 160, No. 3): "How can you say that '... 625 ...' and '... 25×25 ...' say the same thing?—Only through our arithmetic do they *become one*." What is meant here is closely related to what Kant had in mind in his argument against the view that $7 + 5 = 12$ is a mere analytical proposition. Kant contends there that the concept 12 "is by no means already thought in merely thinking this union of 7 and 5," and he adds: "That 7 should be added to 5, I have indeed already thought in the concept of a sum $= 7 + 5$, but not that this sum is equivalent to the number 12" (*Critique of Pure Reason*, B 14ff.). In modern terminology, this Kantian argument could be expressed as follows: The concept "$7 + 5$" is an individual concept (to use Carnap's terminology) expressible by means of the description ι_x $(x = 7+5)$, and this concept is different from the concept "12;" the only reason why this is not obvious is that we involuntarily carry out the addition of the small numbers 7 and 5 directly. We have here the case, in the new logic often discussed following the example of Frege, of two terms with a different "sense" but the same "meaning" (called "denotation" by A. Church); and to determine the synthetic or analytic character of a judgment one must, of course, always consider the sense, not the meaning. The Kantian thesis that mathematics is synthetic does, incidentally, not stand in conflict with what the Russellian school maintains when it declares the propositions of arithmetic to be analytic. For we have here two entirely different concepts of the analytic—a fact which, in recent times, has been pointed out especially by E. W. Beth.[4]

Another intrinsic tension is to be found in Wittgenstein's position with respect to logistic. On the one hand, he often tends towards regarding proofs as formalized. Thus we can read on p. 93, No. 64: "Suppose I were to set someone the problem: 'Find a proof of the proposition ...'—The answer would surely be to show me certain signs." The distinctive and indispensable role of everyday language relative to that of a formalized language is not given prominence in his remarks. He often speaks of "the language game," and does not restrict the use of this expression to an artificial formal language, for which alone it is really appropriate. Indeed, our natural language does not have the character of a game at all; it is part of us, almost in the same way in which our limbs are. Apparently Wittgenstein is here still under the sway of the idea of a scientific language that encompasses all scientific thought. In contrast with this stand his highly critical remarks about mathematical logic. Apart from the one already quoted concerning "the curse of the invasion of mathematics by mathematical logic," the following is especially noteworthy (p. 156, No. 48): "'Mathematical logic' has completely deformed the thinking of mathematicians and of philosophers, by setting up a superficial interpretation of the forms of our everyday language as an analysis of the structures of facts. Of course in this it has only continued to build on the Aristotelian logic."

We can get clearer about the idea that seems to underlie this criticism if we keep in mind the following: the logical calculus was intended, by various of its founders, as a realization of the Leibnizian idea of a *characteristica universalis*. With regard to Aristotle, Wittgenstein's remark, if looked at more closely, is not a criticism; since all Aristotle wanted to do with his logic was to fix the usual forms of logical argument and to test their legitimacy. The task of a *characteristica universalis*, on the other hand, was to be much more comprehensive; it was to establish a conceptual world that would make it possible to understand all real connections. With respect to an undertaking aimed at that goal it cannot be taken for granted, however, that the grammatical structures of our language are to function also as the basic framework for the theory; since the categories of that grammar have a character that is at least partially anthropomorphic. At the same time it should be emphasized that, besides our usual logic, nothing even approaching its

[4] *Vide* [**?**].

value has been devised in philosophy so far. What Hegel, in particular, put in place of Aristotelian logic when he rejected it consists of a mere comparing of universals in terms of analogies and associations, without any clear regulative procedure. This method can certainly not pass as even an approximate fulfillment of the Leibnizian idea.

Unfortunately, from Wittgenstein we do not get any guidance for how to replace conventional logic by something philosophically more efficient either. Most likely, he considered the analysis of the structures of reality to be a misguided project; his goal was, after all, not to find a procedure that is somehow determinate. The "logical compulsion," the "inexorability of logic," the "hardness of the logical must" are a constant stumbling-block for him and, again and again, a cause for consternation. Perhaps he does not always bear in mind that all these terms merely have the character of popular comparisons, and are inappropriate in many respects. The strictness of the logical and the exact does not constrain our freedom. Indeed, it is our very freedom that enables our intention to be precise in thought while confronted with a world of imprecision and inexactitude. Wittgenstein speaks of the "must of kinematics" as being "much harder that the causal must" (p. 37, No. 121). Is it not an aspect of freedom that we can conceive of virtual motions that are subject merely to kinematic laws, apart from the real, causally determined motions, and that we can compare the former with the latter?

Enlightened humanity has sought liberation in rational determination when confronted with the dominance of the merely authoritative. But at present awareness of this fact has for the most part been lost, and for many the validity of science appears to be an oppressive authority.

In Wittgenstein's case, it is certainly not this aspect that evokes his critical attitude towards scientific objectivity. Nevertheless, his tendency is to declare the intersubjective unanimity in the field of mathematics to be an heteronomous one. Our agreement, he believes, is to be explained by the fact that we are in the first place "trained" together in basic techniques, and that the agreement thus created is then continued through the proofs (cf. the quotation on p. 195). That this kind of explanation is inadequate will occur to anybody not blinded by the apparent originality of the point. Already the possibility of our calculating techniques, with their manifold possibilities of decomposing a problem into simpler parts made possible by the validity of the laws of arithmetic, cannot be regarded as a consequence of agreement (cf. the remark three pages earlier). Furthermore, when we think of the enormously rich and systematic formations of concepts in, e. g., the theory of functions—

where one can say of the theorems obtained at each stage what Wittgenstein once said: "We rest, or lean, on them" (p. 124, No. 35)—we see that the position mentioned above doesn't in any way explain why these conceptual edifices are not continually collapsing. Considering Wittgenstein's point of view, it is in fact not surprising that he does not feel the contradiction to be something strange; but what does not become clear in his account is that contradictions in mathematics are to be found only in quite peripheral extrapolations and nowhere else. In this respect one can say that Wittgenstein's philosophy does not make the fact of mathematics intelligible at all.

But what is the source of Wittgenstein's initial conviction that in the domain of mathematics there is no proper objectual knowledge, that everything consists instead of techniques, measuring devices, and customary attitudes? He must think: "There is nothing here to which knowledge could be directed." This is connected with the fact, already mentioned above, that he does not recognize any role for phenomenology. What probably provokes his opposition to it are phrases such as when one talks about the "essence" of a color; where the word "essence" suggests the idea of hidden properties of the color, whereas colors as such are nothing but what is evident in their manifest properties and relations. But this does not prevent such properties and relations from being the content of objective statements; colors are, after all, not nothing. And even if we do not adopt the pretensions of Husserl's philosophy with regard to the "intuition of essences," this does not preclude the possibility of an objective phenomenology. The fact that phenomenological investigations in the domain of colors and sounds are still in their infancy is surely connected with the fact that they have no great importance for theoretical physics; since in physics we are induced, already at an early stage, to eliminate colors and sounds as qualities. Mathematics, on the other hand, can be regarded as the theoretical phenomenology of structures. Indeed, what contrasts phenomenologically with the qualitative is not the quantitative, as traditional philosophy teaches, but the structural, which consists of the forms of juxtaposition, succession, and composition, together with all the corresponding concepts and laws.

Such a conception of mathematics leaves one's position with respect to the problems of the foundations of mathematics still largely undetermined. But it can open the door, for someone starting with Wittgenstein's views, for a viewpoint that does greater justice to the peculiar character and the significance of the mathematical.

Chapter 23

Bernays Project: Text No. 24

The multiplicity of purposes in formulations of geometric axiom systems (1959)

Die Mannigfaltigkeit der Direktiven für die Gestaltung geometrischer Axiomensysteme

(*The Axiomatic Method*, ed. by Henkin, Suppes, Tarski, Amsterdam: North-Holland, pp. 1–15;
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In considering axiomatizations of geometry we have the impression of a great multiplicity of principles according to which such axiomatizations can take place, and already have taken place. The original, simple idea, that one could just speak of *the* axioms of geometry was not only superseded by the discovery of non-Euclidean geometries but, moreover, by the insight into the possibility of different axiomatizations of one and the same geometry. But substantially different methodological principles have also arisen generally, according to which one has undertaken the axiomatization of geometry and whose purposes are in certain respects even antagonistic.

The seed for this multiplicity can already be found in Euclidean axiomatics. For its formulation was determined by the fact that one was led by geometry to the general problem of axiomatics for the first time. Here geometry is simply all of mathematics, so to speak. The methodological relation to number theory is not completely clear. In certain places a bit of number theory is developed using the intuitive idea of number. Moreover the concept of number is used contentfully in the theory of proportions, even with an implicit inclusion of the *tertium non datur*, although it seems that one attempted to avoid its unrestricted use.

While the special methodological position of the concept of number is not especially pronounced here, the concept of magnitude is explicitly put forward as a contentful tool. This is done, incidentally, in a manner that we can no longer accept today, namely by assuming as a matter of course that different objects can have the character of magnitudes. The concept of magnitude is, of course, also subjected to axiomatization; however, in this regard the axioms are explicitly separated from the remaining axioms as antecedent (*κοιναὶ ἔννοιαι*). These axioms are of a similar kind as those which are used today for Abelian groups. But what remained undone, because of the methodological standpoint at the time, was to determine axiomatically which objects were to be regarded as magnitudes.

Thus it is all the more admirable that one was then already sensitive to the peculiarity of that assumption by which the Archimedean magnitudes, as we call them today, are characterized. The Archimedean (Eudoxean) axiom is then, in the medieval tradition that followed the Greeks, used in particular in the Arabic investigations of the parallel axiom. It also occurs essentially in Saccheri's proof of the elimination of the "hypothesis of the obtuse angle." This elimination is in fact impossible without the Archimedean axiom, since a non-Archimedean, weakly-spherical (resp. weakly-elliptical) geometry is in accordance with the axioms of Euclidean geometry, except for the parallel axiom.

The second axiom of continuity, which was formulated in the late 19th century, does not yet occur in any of these investigations. It could be dispensed with in the proofs for which it came into question—like in the determination of areas and lengths—because of the already mentioned use of the concept of magnitude, according to which it was for example taken for granted that both the area of the circle and the circumference of the circle possess a definite magnitude. In place of the old theory of magnitudes at the beginning of modern times came, as a predominant and super-ordinated discipline, the theory of magnitudes of *analysis*, which developed quite prolifically both formally and contentually still before it reached methodological clarity.

Of course, analysis at first played no significant role in the discovery of non-Euclidean geometry, but it became dominant in the following investigations of Riemann and Helmholz, and later Lie, for the identification of the three special geometries by certain very general, analyticaL conditions. In particular it is characteristic for this treatment of geometry that one not only takes the particular spatial entities as objects, but also the spatial manifold itself. The enormous conceptual and formal means which mathematics had obtained in the meantime showed up in the possibility of carrying out such an investigation. And the conceptual and speculative direction which mathematics took in the course of the 19th century is expressed in the formulation of the general problem.

The differential geometrical treatment of the foundations of geometry was developed further, until very recent times, by Hermann Weyl, as well as Elie Cartan and Levi-Civitá, in connection with Einstein's general theory of relativity. Despite the impressiveness and elegance of what has been achieved in this respect, mathematicians were not content with it from a foundational standpoint. At first one tried to free oneself from the fundamental assumption of the methods of differential geometry of the differentiability of the mappings. For this the development of the methods of a general topology was needed, which began at the turn of the century and has taken such an impressive course of development since then. Moreover one strove for independence from the assumption of the Archimedean character of the geometrical magnitudes in general.

This tendency is part of that development by which analysis in some sense lost its previously predominant position. This new stage in mathematical research followed the consequences of the already mentioned conceptual and speculative direction of mathematics of the 19th century, which appeared in particular in the creation of general set theory, in the sharper foundation of analysis, in the constitution of mathematical logic, and in the new version of axiomatics.

At the same time it was characteristic for this new stage that one returned again to the methods of ancient Greek axiomatics, as happened repeatedly in those epochs in which emphasis was put on conceptual precision. In Hilbert's

Foundations of Geometry[a] we find on the one hand this return to the old elementary axiomatics, of course with a fundamentally changed methodological conception, and on the other hand the exclusion, as far as possible, of the Archimedean axiom as a principal theme: in the theory of proportions, in the concept of area, and in the foundation of the line segment calculus. For Hilbert, by the way, this kind of axiomatization was not intended to be exclusive; shortly afterwards he put a different kind of foundation along side it, in which the program of a topological foundation mentioned above was formulated and carried out for the first time.

Around the same time as Hilbert's foundation, the axiomatization of geometry was also cultivated in the school of Peano and Pieri. Shortly afterwards the axiomatic investigations of Veblen and R. L. Moore followed; and by then the directions of research were chosen along which occupation with the foundations of geometry proceeds also today. As is characteristic of it, there are numerous methodological directions.

One of them seeks to characterize the multiplicity of congruent transformations by conditions that are as general and succinct as possible. The second one puts the projective structure of space at the beginning and strives to reduce the metrical structure to the projective with the methods developed by Cayley and Klein. And the third aims at elementary axiomatization of the full geometry of congruences.

Different and fundamentally new points of view were added during the development of these directions. Firstly, the projective axiomatization gained an increased systematization through lattice theory. In addition, one became aware that the set-theoretic and function-theoretic concept formations can be deemphasized in the identification of the group of congruent transformations by identifying the transformations with structures determining them. Therewith the procedure approaches that of elementary axiomatics, since the group relations are now represented as relations between geometric structures.

But I do not want to speak further of these two directions of research in geometrical axiomatics, for which more authentic representatives are present here, and also not of the successes that have been achieved using topological methods, about which the newest essays of Freudenthal give a survey. Instead, I turn to the questions of the direction of axiomatization that was

[a] *Vide* [**?**].

mentioned in the third place.

Even within this direction we find a multiplicity of possible goals. On the one hand one can aim to manage with as few as possible basic elements, perhaps only one basic predicate and one sort of individuals. On the other hand one can especially aim to isolate natural separations of parts of the axiomatics. These viewpoints lead to different alternatives.

So on the one hand the consideration of non-Euclidean geometry suggests the preliminary investigation of an "absolute" geometry. On the other hand something is to be said for a procedure that starts off with affine vector geometry, as is done at the beginning of Weyl's *Space, Time, Matter*[b]. The demands of both these viewpoints can hardly be satisfied with a single axiomatic system. Starting with the axioms of incidence and ordering it is a possible and elegant conceptual reduction to reduce the concept of collinearity to the concept of betweenness, in the way of Veblen. On the other hand it is important for some considerations to separate the consequences of the incidence axioms which are independent of the concept of ordering. So it is desirable to realize the independence of the foundation of the line segment calculus on the incidence axioms from the ordering axioms. In the theory of ordering itself one has again realized the possibility of replacing the axioms of linear ordering by applications of the axiom of Pasch; on the other hand in some respect a formulation of the axioms is preferable in which those axioms are separated which characterize the linear ordering.

The multiplicity of the goals that are possible, and are also pursued in fact, is not exhausted in the least by these examples of alternatives. Indeed it is a possible and plausible, but not obligatory, regulative viewpoint that the axioms should be formulated in such a way that they refer only to a limited part of space respectively. This thought is implicitly at work already in Euclidean axiomatics; and it may also be that the offense that has been taken so early at the parallel axiom relies precisely on the fact that the concept of a sufficiently long extension occurs in the Euclidean formulation. The first explicit realization of the mentioned program happened with Moritz Pasch, and it was followed by the introduction of ideal elements by intersection theorems, which is a method for the foundation of projective geometry that has been successively developed since.

A different kind of possible additional task is to imitate conceptually the

[b] *Vide* [**?**].

blurriness of our pictorial imagination as it was done by Hjelmslev.[c] This results not only in a different kind of axiomatization, but in a variant relational system, which has not found much approval because of its complication. But also without moving so far from the customary manner in this direction it is possible to aim at something similar, in some respects, by avoiding the concept of point as a basic term as it is done in various interesting newer axiomatizations, in particular in Huntington's.[d]

One thus sees in a great number of ways that there is no definite optimum for the formulation of a geometric axiom system. As regards the reductions with respect to the basic concepts and the sorts of things, it must always be recalled that, regardless of the general interest any such possibility of reduction may have, a real application of such a reduction is only recommended when it leads to a clear formulation of the axiom system.

Certain directives for reductions which are generally acceptable can, however, be stated. Let us take for example the Hilbertian version of axiomatics. In it, on the one hand, lines are taken as a kind of things, on the other hand the rays are introduced as point sets and afterwards the angles are explained as ordered pairs of two rays that originate in the same point, thus as a pair of sets. Here real possibilities of simplifying reductions are given. One may be of different opinion whether one wants to start with only one sort of points instead of the different sorts "point, line, plane," whereby the the relations of collinearity and coplanarity of points replace the relations of incidence. In the lattice theoretical treatment the lines and planes are taken to be on par with points as things. Here again there is an alternative. Whereas to introduce the rays as point sets transcends in any case the scope of elementary geometry and is not necessary for it. Generally we can take as a directive that higher types should not be introduced without need. This can be avoided in the case of the definition of angle by reducing the statements about angles by statements about point triples, as was carried out by R. L. Moore.[1*] An even further reduction is achieved here by explaining the congruence of angles using congruence of line segments, but here again a certain loss takes place. Namely, the proofs rest substantially on the congruence of differently oriented triangles. Thus this kind of axiomatization is not suitable for the

[1*] *Vide* [**?**]. (Footnotes 1* to 6* were added later [to the reprint in the *Abhandlungen*].)

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[c] *Vide* [**?**].
[d] *Vide* [**?**].

kind of problems of the Hilbertian investigations, which refer to the relationship between oriented congruence and symmetry. This remark concerns also most other axiomatizations, which turn on the concept of reflection .

Besides the general viewpoints, I want to mention as something particular a special possibility of the formulation of an elementary axiom system, namely one in which the concept "the triple of points a, b, c forms a right angle at b" is taken as the only basic relation and the points are the only basic sort, a program which has recently been called attention to in a paper by Dana Scott.[2*] The mentioned relation satisfies the necessary condition ascertained by Tarski for a single sufficient basic predicate for plane geometry.[3*] In comparison with Pieri's technique,[4*] which has become exemplary for an axiomatic of this kind, and which took an axiomatization of the relation "b and c have the same distance from a" as basic predicate, it seems to permit a simplification, inasmuch the concept of the collinearity of points is closer to that of a right angle than Pieri's basic concept. As respects the concept of congruence there seems to be no simplification for the axioms of congruence from the relation considered. By the way, this axiomatization is one of those, like the one by Pieri, which do not distinguish oriented congruence.[1]

For an elementary axiomatization of geometry the special question presents itself of obtaining completeness, in the sense of categoricity. In most axiom systems this is obtained by the continuity axioms. But the introduction of these axioms involves, as is known, a transgression of the usual framework of concepts of predicates and sets. We have, however, learned from Tarski's investigations that completeness, at least in the deductive sense, can be obtained in an elementary framework, where it is noteworthy that the [Dedekind] cut axiom is preserved in a particular formalization, whereas the Archimedean axiom is omitted. The Archimedean axiom is insofar formally unusual, in that in logical formalization it has the form of an infinite disjunction, whereas the cut axiom is representable by an axiom schema, due to its general form. Thus it can be adapted in its use to the formal framework, whereby for the elementary framework of predicate logic the provability of

[2*] *Vide* [**?**].
[3*] *Vide* [**?**].
[4*] *Vide* [**?**].
[1] Some details on the definitions of the concepts of incidence, ordering, and congruence from the concept of a right angle, as well as of part of the axiom system, follow in the appendix.

the Archimedean axiom from the cut axiom is then lost. Of course, such a restriction to the framework of predicate logic has as a consequence that some considerations are possible only meta-theoretically, for example, the proof of the theorem that a simple closed polygon decomposes the plane, and also the considerations about equality of supplementation and decomposition of polygons. Here one is again faced with an alternative, namely whether to begin with the viewpoint of an elementary logical framework, or then again not to restrict oneself with respect to the logical framework, whereby incidentally different gradations can be considered.

With respect to the application of a second-order logic I only want to recall here that it can be made precise in the framework of axiomatic set theory, and that no noticable restriction of the methods of proof result. Also the Skolem paradox does not present a real inconvenience in the case of geometry, since it can be eliminated in the model theoretic considerations by equating the concept of set which occurs in one of the higher axioms with the concept of set of model theory.

Finally I want to emphasize that the fact, which I have stressed in my remarks, that there is no definite optimum for the systems of axiomatics, does not at all mean that the results of geometric axiomatics necessarily have an imperfect or fragmentary character. As you know, in this field a number of systems of great perfection and elegance have been achieved. The multiplicity of possible goals is responsible for the older systems not generally being simply outdated by newer ones, and at the same time every perfection attained still leaves room for further efforts.

Appendix. *Remarks on the task of an axiomatizing Euclidean plan geometry with a single basic relation $R(a,b,c)$: "the triple of points a,b,c forms a right angle at b."* The axiomatization succeeds as it does, in a simple way, because only the relations of collinearity and parallelism are considered. The following axioms suffice for the theory of collinearity:

A1 $\neg R(a,b,a)$

A2 $R(a,b,c) \;\rightarrow\; R(c,b,a) \,\&\, \neg R(a,c,b)$ [2]

A3 $R(a,b,c) \,\&\, R(a,b,d) \,\&\, R(e,b,c) \;\rightarrow\; R(e,b,d)$

[2] Already this axiom excludes elliptic geometry.

A4 $R(a,b,c)$ & $R(a,b,d)$ & $c \neq d$ & $R(e,c,b) \;\rightarrow\; R(e,c,d)$

A5 $a \neq b \;\rightarrow\; (Ex)R(a,b,x)$.[5*]

The definition of the relation $\mathrm{Coll}(a,b,c)$ is added: "the points a,b,c are collinear:"

Definition 1. $\mathrm{Coll}(a,b,c) \;\leftrightarrow\; (x)(R(x,a,b) \rightarrow R(x,a,c)) \vee a = c$.

Then the following theorems are provable:

(1) $\mathrm{Coll}(a,b,c) \;\leftrightarrow\; a = b \vee a = c \vee b = c \vee (Ex)(R(x,a,b)$ & $R(x,a,c))$

(2) $\mathrm{Coll}(a,b,c) \;\rightarrow\; \mathrm{Coll}(a,c,b)$ & $\mathrm{Coll}(b,a,c)$

(3) $\mathrm{Coll}(a,b,c)$ & $\mathrm{Coll}(a,b,d)$ & $a \neq b \;\rightarrow\; \mathrm{Coll}(b,c,d)$

(4) $R(a,b,c)$ & $\mathrm{Coll}(b,c,d)$ & $b \neq d \;\rightarrow\; R(a,b,d)$

(5) $R(a,b,c) \;\rightarrow\; \neg\mathrm{Coll}(a,b,c)$

(6) $R(a,b,c)$ & $R(a,b,d) \;\rightarrow\; \mathrm{Coll}(b,c,d)$

(7) $R(a,b,c)$ & $R(a,b,d) \;\rightarrow\; \neg R(a,c,d)$.
Proof: $\mathrm{Coll}(c,d,b)$ & $c \neq b \;\rightarrow\; (R(a,c,d) \rightarrow R(a,c,b))$

(8) $R(a,b,c)$ & $R(a,b,d)$ & $R(a,e,c)$ & $R(a,e,d) \;\rightarrow\; c = d \vee b = e$.
Proof: $\mathrm{Coll}(b,c,d)$ & $\mathrm{Coll}(e,c,d)$ & $c \neq d \;\rightarrow\; \mathrm{Coll}(b,c,e)$
$\mathrm{Coll}(b,c,e)$ & $b \neq e$ & $R(a,b,c) \;\rightarrow\; R(a,b,e)$
$\mathrm{Coll}(e,c,b)$ & $b \neq e$ & $R(a,e,c) \;\rightarrow\; R(a,e,b)$
$R(a,b,e) \rightarrow \neg R(a,e,b)$.

For the theory of parallelism, we add two further axioms:

A6 $a \neq b$ & $a \neq c \;\rightarrow\; (Ex)(R(x,a,b)$ & $R(x,a,c)) \vee$
$(Ex)(R(a,x,b)$ & $R(a,x,c)) \vee R(a,b,c) \vee R(a,c,b)$

In plain language, the axiom says that it is possible to draw a perpendicular to a line bc from a point a lying off from it. The unique determination of a perpendicular depending on a point a and a line bc results with the help of (4) and (8).

[5*] Paul de Witte hat darauf aufmerksam gemacht, daß A3 als Axiom entbehrlich ist, da die Formel aus A2, A4, A5 abgeleitet werden kann. (*vide* [?])

A7 $R(a,b,c)$ & $R(b,c,d)$ & $R(c,d,a) \;\rightarrow\; R(d,a,b)$

This is a form of the Euclidean parallel axiom in the narrower, angular metrical sense.

Parallelism is now defined by:

Definition 2. $_{a}$;$_{a}$ $\mathrm{Par}(a,b;c,d) \;\leftrightarrow\; a \neq b$ & $c \neq d$ & $(Ex)(Ey)(R(a,x,y)$ & $R(b,x,y)$ & $R(c,y,x)$ & $R(d,y,x))$

As provable theorems the following arise:

(9) $\mathrm{Par}(a,b;c,d) \;\rightarrow\; \mathrm{Par}(b,a;c,d)$ & $(c,d;a,b)$

(10) $\mathrm{Par}(a,b;c,d) \;\rightarrow\; a \neq c$ & $a \neq d$ & $b \neq c$ & $b \neq d$

(11) $\mathrm{Par}(a,b;c,d) \;\leftrightarrow\; a \neq b$ & $c \neq d$ & $(Ex)(Eu)($
$(R(a,x,u) \vee x = a)$ & $(R(b,x,u) \vee x = b)$ &
$(R(x,u,c) \vee u = c)$ & $(R(x,u,d) \vee u = d)\,)$

For the proof of the implication from right to left one has to show that there are at least five different points lying on the line a, b, which succeeds with the help of axioms A1–A6.

(12) $\mathrm{Par}(a,b;c,d) \;\rightarrow\; (x)((R(a,x,c) \vee x = a)$ & $(R(b,x,c) \vee x = b) \rightarrow R(x,c,d))$

(13) $\mathrm{Par}(a,b;c,d)$ & $\mathrm{Coll}(a,b,e)$ & $b \neq e \;\rightarrow\; \mathrm{Par}(b,e;c,d)$

and thus in particular:

(14) $\mathrm{Par}(a,b;c,d) \;\rightarrow\; \neg\mathrm{Coll}(a,b,c)$;

moreover

(15) $\mathrm{Par}(a,b;c,d)$ & $\mathrm{Coll}(a,b,e) \;\rightarrow\; \neg\mathrm{Coll}(c,d,e)$

(16) $\neg\mathrm{Coll}(a,b,c) \;\rightarrow\; (Ex)\mathrm{Par}(a,b;c,x)$

(17) $\mathrm{Par}(a,b;c,d)$ & $\mathrm{Par}(a,b;c,e) \;\rightarrow\; \mathrm{Coll}(c,d,e)$

(18) $\mathrm{Par}(a,b;c,d)$ & $\mathrm{Par}(a,b;e,f) \;\rightarrow$
$\mathrm{Par}(c,d;e,f) \vee (\mathrm{Coll}(e,c,d)$ & $\mathrm{Coll}(f,c,d))$.

The concept of vector equality is also tied up with the concept of parallelism: “a, b and c, d are the opposite sides of a parallelogram.”

D$_{a_2}$e$_{a_2}$finition 3. $\mathrm{Pag}(a, b; c, d) \;\leftrightarrow\; \mathrm{Par}(a, b; c, d) \;\&\; \mathrm{Par}(a, c; b, d)$

Herewith one can prove:

(19) $\mathrm{Pag}(a, b; c, d) \;\rightarrow\; \mathrm{Pag}(c, d; a, b) \;\&\; \mathrm{Pag}(a, c; b, d)$

(20) $\mathrm{Pag}(a, b; c, d) \;\&\; \mathrm{Pag}(a, b; c, e) \;\rightarrow\; d = e$

(21) $\mathrm{Pag}(a, b; c, d) \;\rightarrow\; \neg\mathrm{Coll}(a, b, c)$.

For the proof of the existence theorem

(22) $\neg\mathrm{Coll}(a, b, c) \;\rightarrow\; (Ex)\mathrm{Pag}(a, b; c, x)$

one needs a further axiom:

A8 $R(a, b, c) \;\rightarrow\; (Ex)(R(a, c, x) \;\&\; R(c, b, x))$.

It is generally provable with the help of this axiom that two different, non-parallel lines have a point of intersection:

(23) $\neg\mathrm{Coll}(a, b, c) \;\&\; \neg\mathrm{Par}(a, b; c, d) \;\rightarrow$
$(Ex)(\mathrm{Coll}(a, b, x) \;\&\; \mathrm{Coll}(c, d, x))$. – [6*]

It is left open whether it is possible to achieve altogether a clear axiom system using the basic concept R. Here we content ourself with stating definitions for the fundamental further concepts. For these it is in any case possible to attain a certain clarity.

The following two different definitions of the relation “a is the center of the line segment b, c” are related to the figure of the parallelogram:

Definition $\mathbf{4_1}$. $Mp_1(a; b, c) \;\leftrightarrow\; (Ex)(Ey)(\mathrm{Pag}(b, x; y, c) \;\&$
$\mathrm{Coll}(a, b, c) \;\&\; \mathrm{Coll}(a, x, y))$

Definition $\mathbf{4_2}$. $Mp_2(a; b, c) \;\leftrightarrow\; (Ex)(Ey)(\mathrm{Pag}(x, y; a, b) \;\&\; \mathrm{Pag}(x, y; c, a))$.

According to the second definition one can prove the possibility of doubling a line segment:

[6*] Das Vorderglied $\neg\mathrm{Coll}(a, b, c)$ ist, wie man leicht einsieht, entbehrlich.

(24) $a \neq b \;\rightarrow\; (Eu)Mp_2(a; b, u)$.

The existence of the center of a line segment according to Df. 4_1, i. e.,

(25) $b \neq c \;\rightarrow\; (Eu)Mp_1(u; b, c)$,

is provable if one adds the axiom:

A9 $\mathrm{Par}(a, b; c, d)$ & $\mathrm{Par}(a, c; b, d) \;\rightarrow\; \neg\mathrm{Par}(a, d; b, c)$.
(In a parallelogram the diagonals intersect.)

By specializing the figure pertaining to the definition of Mp_1 we obtain the definition of the relation: "a, b, c form a isosceles triangle with the peak at a:"

Definition 5_1. $Ist_1(a; b, c) \;\leftrightarrow\; (Eu)(Ev)(\mathrm{Pag}(a, b; c, v)$ & $R(a, u, b)$ & $R(a, u, c)$ & $R(b, u, v))$.

With the help of Mp_1 and Ist_1 we can define Pieri's basic concept: "a has the same distance from b and c:"

Definition 6. $Is_1(a; b, c) \;\leftrightarrow\; b = c \vee Mp_1(a; b, c) \vee Ist_1(a; b, c)$.

A different kind of definition of the concept Is is based on the use of symmetry. The following auxiliary concept is used for this: "a, b, c, d, e form a 'normal' quintuple:"

Definition 7. $Qn(a, b, c, d, e) \;\leftrightarrow\; R(a, c, b)$ & $R(a, d, b)$ & $R(a, e, c)$ & $d_2 \underline{\cdot} d_2\, a_2 \underline{,} a_2$
$R(a, e, d)$ & $R(b, e, c)$ & $c \neq d$.

With the help of Qn we obtain a further way of defining Mp and Is:

Definition 4_3. $Mp_3(a; b, c) \;\leftrightarrow\; (Ex)(Ey)Qn(x, y, b, c, a)$

Definition 5_2. $Ist_2(a; b, c) \;\leftrightarrow\; (Ex)(Ey)Qn(a, x, b, c, y)$,

from which Is_2 can be defined respectively like Is_1.

Moreover also the definition of the reflection of points a, b with respect to a line c, d follows:

Definition 8. $\mathrm{Sym}(a, b; c, d) \;\leftrightarrow\; c \neq d$ & $(Ex)(Ey)(Ez)(\mathrm{Coll}(x, c, d)$ & $\mathrm{Coll}(y, c, d)$ & $Qn(x, y, a, b, z))$.–

Finally, for the definition of congruence of line segments we still need the concept of oriented congruence on a line: "the line segments ab and cd are collinear, congruent, and oriented in the same direction:"

Definition 9₁. $Lg_1(a,b;c,d) \leftrightarrow \mathrm{Coll}(a,b,c)\ \&$
$$(Ex)(Ey)(\mathrm{Pag}(a,x;b,y)\ \&\ \mathrm{Pag}(c,x;d,y)),$$

or also:

Definition 9₂. $Lg_2(a,b;c,d) \leftrightarrow$
$$\mathrm{Coll}(a,b,c)\ \&\ a \neq b\ \&\ (Ex)(Mp(x;b,c)\ \&\ Mp(x;a,d)) \vee (a=d\ \&\ Mp(a;b,c)) \vee (b=c\ \&\ Mp(b;a,d)),$$

(where any of the three definitions above can be taken for Mp.) Now the congruence of line segments can be defined altogether (with any of the two definitions of Lg):

Definition 10. $Kg(a,b;c,d) \leftrightarrow Lg(a,b;c,d) \vee Lg(a,b;d,c) \vee$
$$(a=b\ \&\ Is_1(a;b,d)) \vee (Ex)(\mathrm{Pag}(a,b;c,x)\ \&\ Is_1(c;x,d)).$$

By a definition analogous to that of Lg_2 it is possible to introduce the congruence of angles with the same vertex as a six-place relation, after one has already introduced the concept of angle bisection: "d ($\neq a$) lies on the bisection of the angle $b\,a\,c$:"

Definition 11. $\mathrm{Wh}(a,d;b,c) \leftrightarrow \neg\mathrm{Coll}(a,d,c)\ \&$
$$(Ex)(Ey)(Ez)(\mathrm{Coll}(a,c,x)\ \&\ \mathrm{Coll}(a,d,y)\ \&\ Qn(a,y,b,x,z)).$$

In consideration of the composite character of this congruence relation Kg, one will reduce the laws about Kg in the axiomatization to the concepts that occur as parts of the defining expression. Because of the variety of definitions for Mp, Ist, Is there are alternatives depending on whether one employs the relations of parallelism or of symmetry more. In any case, the axiom of vector geometry

A10. $\mathrm{Pag}(a,b;p,q)\ \&\ \mathrm{Pag}(b,c;q,r) \rightarrow$
$$\mathrm{Pag}(a,c;p,r) \vee (\mathrm{Coll}(a,c,p)\ \&\ \mathrm{Coll}(a,c,r))$$

or an equivalent one should be useful. On the whole one could set oneself as a goal to represent the interaction of parallelism and reflection that occurs in Euclidean plane geometry in a most symmetric way.

Finally, with respect to the betweenness relation, the form of the definition of the relation "a lies between b and c" is already contained as a part in that of Qn. Namely, we can define:

Definition 12. $\mathrm{Bt}(a; b, c) \;\leftrightarrow\; (Ex)(R(b, a, x) \;\&\; R(c, a, x) \;\&\; R(b, x, c))$.

For this concept, at first, is provable:

(26) $\neg\mathrm{Bt}(a; b, b)$

(27) $\mathrm{Bt}(a; b, c) \;\rightarrow\; \mathrm{Bt}(a; c, b)$

(28) $\mathrm{Bt}(a; b, c) \;\rightarrow\; \mathrm{Coll}(a, b, c)$

and also using A5, A6, and A8

(29) $a \neq b \;\rightarrow\; (Ex)\mathrm{Bt}(x; a, b) \;\&\; (Ex)\mathrm{Bt}(b; a, x)$.

To obtain further properties of the betweenness concept the following axioms can be used:

A11 $R(a, b, c) \;\&\; R(a, b, d) \;\&\; R(c, a, d) \;\&\; R(e, c, b) \;\rightarrow\; \neg R(b, e, d)$

A12 $R(a, b, d) \;\&\; R(d, b, c) \;\&\; a \neq c \;\rightarrow\; \mathrm{Bt}(a; b, c) \vee \mathrm{Bt}(b; a, c) \vee \mathrm{Bt}(c; a, b)$

A13 $\mathrm{Bt}(a; b, c) \;\&\; \mathrm{Bt}(b; a, d) \;\rightarrow\; \mathrm{Bt}(a; c, d)$

A14 $R(a, b, d) \;\&\; R(d, b, c) \;\&\; R(a, c, e) \;\&\; \mathrm{Bt}(d; a, e) \;\rightarrow\; \mathrm{Bt}(b; a, c)$

From this axiom it is possible to obtain the more general theorem in a few steps:

(30) $\mathrm{Bt}(b; a, c) \;\&\; \mathrm{Coll}(a, d, e) \;\&\; \mathrm{Par}(b, d; c, e) \;\rightarrow\; \mathrm{Bt}(d; a, e)$

This succeeds using the theorem

(31) $$R(a, b, e) \;\&\; R(e, b, c) \;\&\; R(b, a, d) \;\&\; R(b, c, f) \;\&\; R(b, e, d) \;\&\; R(b, e, f) \;\&\; \mathrm{Bt}(b; a, c) \;\rightarrow\; \mathrm{Bt}(e; d, f).$$

which can be derived from the aforementioned axiom A10.

With the help of (30) and axiom A13 one can prove:

(32) $$\neg\mathrm{Coll}(a, b, c) \;\&\; \mathrm{Bt}(b; a, d) \;\&\; \mathrm{Bt}(e; b, c) \;\rightarrow\; (Ex)(\mathrm{Coll}(e, d, x) \;\&\; \mathrm{Bt}(x; a, c)).$$

i. e., Pasch's axiom in the narrower formulation of Veblen.—

In conclusion, I want to mention the following definition of *Kg* using the concepts *Is* and *Bt*, which is based on a construction of Euclid:

Definition 13. $Kg^*(a, b; c, d) \leftrightarrow (Ex)(Ey)(Ez)(Is(x, a; c)$ & $\mathrm{Bt}(y; a, x)$ & $\mathrm{Bt}(z; c, x)$ & $Is(a; b, y)$ & $Is(c; d, z)$ & $Is(x; y, z))$.

(For Is either Is_1 or Is_2 can be taken.)

One surely can not demand from an axiomatic system like the one described here, in which the collinearity and the betweenness relation are coupled with orthogonality, that it provides a derivation of the axioms of linearity. Moreover the formulation is limited from the outset to plane geometry, since the definition of collinearity is not applicable in the multi-dimensional case. The restriction to Euclidean geometry is also introduced at an early stage. On the other hand this axiomatization may be particularly suited to showing the great simplicity and elegance of the lawfulness of Euclidean plane geometry.

Chapter 24

Bernays Project: Text No. 25

On the role of language from an epistemological point of view (1961)

Zur Rolle der Sprache in erkenntnistheoretischer Hinsicht

(*Synthese* 13, pp. 185–200;
repr. in *Abhandlungen*, pp. 155–169)

Translation by: *Dirk Schlimm*
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The book *The Logical Syntax of Language*[a] occupies a central place in the philosophy of Rudolph Carnap. The conception that is there developed of the logic of science as the investigation of the language of science together with its concepts forms, so to speak, the initial framework for Carnap's further investigations. In the course of these investigations, he has significantly revised the views expressed in the *Logical Syntax*. The framework for the

[a] *Vide* [**?**].

considerations itself and its associated concept formations have also undergone significant changes. Discussions with philosophers working in related areas have contributed substantially to this.

These stepwise revisions of Carnap's philosophy represent a successive dissociation from the exclusive and reductive tendencies of the initial program of the Vienna school. The *Logical Syntax* had already introduced significant corrections to its overly simplified theses. But there Carnap still defended the view that every epistemology, insofar as it claims to be scientific, has to be understood as being nothing other than the syntax of the language of science, resp. the language itself. Since then he has essentially extended the aim of scientific philosophy by adding semantics and pragmatics (following C. W. Morris) and, furthermore, by comparing the distinction between the logical and the descriptive with that between theoretical and observational language. In the following, the significance of the introduction of these extensions of the methodological framework for the shaping of Carnap's philosophy, and also for its partial reconciliation with more familiar philosophical views, will be elucidated from several points of view; at the same time, certain questions that naturally suggest themselves in this context will be indicated.

1

The general tendency of the *Logical Syntax* can be said to be an extension of the approach of Hilbert's proof theory. For Hilbert the method of formalization is applied only to mathematics. However, in his lecture "Axiomatic thinking" Hilbert also said: "Everything at all that can be the object of scientific thinking falls under the axiomatic method, and thereby indirectly under mathematics, when it becomes mature enough to form into theory."[b] Carnap goes a step further in this direction in the *Logical Syntax*, by considering science as a whole as an axiomatic deductive system which becomes a mathematical object through formalization: the syntax of the language of science is metamathematics that is directed towards this object.

But the idealizing scheme of science that is used here is certainly not sufficient for epistemology. First of all, it of course represents only the finished result of science, not the entire process of scientific research. For the great mathematical theories an axiomatic deductive presentation of the finished

[b] *Vide* [**?**], p. ■ .

disciplines might display sufficiently well what is significant in them. But the circumstances are already fundamentally different in theoretical physics, since here the supreme principles of the theory in their mathematically precise formulation are not the starting point of the research, but the final result.

Moreover, emphasizing the deductive form does violence to many areas of research. In these areas one does not proceed deductively at all; rather, logical reasoning is applied almost exclusively for *heuristic* considerations, which motivate the formulations of hypotheses or statements of fact.

By the addition of *pragmatics*, all of the above can be taken into account. It is clearly a task for pragmatics to consider the development of the sciences, not with regard to what is historical or biographical, to be sure, but in the sense of working out the methodologically significant ideas. Thus, here heuristic considerations find their appropriate place.

Parenthetically I want to note that heuristics play a role not only in the empirical sciences, but also in purely mathematical research, as has been pointed out lately particularly emphatically by Georg Pólya. There exists a methodical analogy between research in mathematics and in the natural sciences, in the sense that also in mathematics there is a kind of empirical approach and guessing of laws based on a series of particular cases. But such a formulation of a law is only of provisional character in mathematics, as in number theory, where the individual case never can be singled out by inessential conditions (like place and time in physics), but rather each number has its own specific properties. That it is possible even in number theory to gain convictions based on our use of numbers, however, is shown by the example of the statement of the unique factorization into prime factors, which one tends to regard as completely self-evident (when one has not yet come across number theoretic proofs) from ones experiences with calculations. Only at an advanced level is the need for a proof for this statement acknowledged, which is then satisfied accordingly.

2

It is useful for the consideration of the relation between syntax and semantics to recall that, from the usual point of view, it is fundamental for a language as such that its words and sentences are directly connected to a sense. When the structure of a language is considered independently of the meaning of its expressions, this is an intended, modifying abstraction.

In Carnap's *Logical Syntax* the exclusion of what is meaningful is compensated for in part by stating "rules of transformation" as well as "rules of formation" as rules of the language. He not only counts those rules according to which a statement is transformed into a logically equivalent one as belonging to these transformation rules of a formalized theory, but, more generally, all those that determine logical dependencies, and moreover, also the stipulations of particular statements as logically universal propositions or *formalized axioms*.

Shortly afterwards, under the influence of Alfred Tarski's investigations and in connection with the extension of his methodological program, Carnap relocated the concept of logical consequence from syntax to semantics.

The logical symbols obtain their meaning in semantics through the "rules of truth," and the semantic concept of entailment is tied to these rules of truth. The formal deductions can be introduced from there by first noting the relations of consequence partly as propositions and partly as rules of inference, and then by axiomatizing the manifold of the obtained propositions and rules. In this way, the concept of rules of transformation as primary rules of the language becomes basically dispensable, while the "rules of truth" should be seen as belonging to the characterization of the language.

The suggestive contrast between the semantic and the syntactic concepts of entailment that is hereby obtained has great advantages for the presentation of mathematical logic — insofar as this is not directed towards a constructive methodology from the outset — and Heinrich Scholz in particular has emphasized this point of view.

It is often felt to be a shortcoming of semantics that it is based on non-constructive concept formation. But being non-constructive is not specific to semantics. One can in principle also pursue semantics within an elementary framework of concept formation. On the other hand, it will hardly be possible to avoid transcending elementary concepts, with or without a semantics, if one wants to fix a concept of "validity," as Carnap intends, such that for every purely logical proposition A (i. e., a proposition without extra-logical components) not only the alternative "A or not-A" is valid (in the sense of the principle of excluded middle), but in addition also that either the logical validity of A or of not-A holds.

Semantics is also criticized with regard to a different point, namely insofar as it goes beyond the logic of extensions and addresses questions regarding sense and in particular regarding the relation between the extensional and the intensional. In particular Willard Quine claims that a scientifically inadmissible hypostasis is performed by the introduction of senses (intensions) of expressions as objects, and that even by the reduction of questions about sense to those about sameness and difference of sense, one is still in a domain of terms that are difficult to make precise. In this discussion Quine agrees with Carnap by tending to explain the sameness of the sense of two statements as their logical equivalence, and accordingly to reduce the sameness of the sense of predicates and characterizations and definitions to logical equivalences. Thereby the concept of synonymy comes into close relation with the analytic.

But such a definition of synonymy yields unwanted consequences, provided, as Carnap and many contemporary philosophers do, that the matters of fact of pure mathematics are regarded as logical laws. From this point of view, any two valid statements of pure mathematics are logically equivalent and thus, if sameness of sense was the same as logical equivalence, any correct statements of pure mathematics, for example the statements that there exits infinitely many prime numbers and that the number π is irrational, would have the same sense. Or, to take a simpler example: the statement $3 \times 7 = 21$ would have the same sense as the statement that 43 is a prime number.

For this consideration, however, we can even eliminate the dependence on the question of the purely logical character of arithmetic. Let us take an axiom system A and two totally different theorems, S and T, that are provable from these axioms. We would hardly be prepared to say that the claim "S follows logically from A" has the same sense as "T follows logically from A," even when both statements are true, thus both are logically valid, and so both are logically equivalent.

Therefore, the sameness of sense by no means always coincides with logical equivalence. On the other hand, in many cases, including mathematics, one surely would consider a logical transformation as not changing the sense. For example, one would consider the two statements "if a, b, c, n are numbers of the sequence of numbers beginning with 1 and $a^n + b^n = c^n$, then either $n = 1$ or $n = 2$" and "there do not exist numbers a, b, c, n of the sequence of numbers beginning with 1, such that $n > 2$ and $a^n + b^n = c^n$" to be formulations of the same mathematical claim (Fermat's theorem).

In these examples, we are confronted with the difficulty of determining what must be considered as having the same sense. But at the same time we notice that this difficulty is based on the distinction between the kinds of abstraction that are peculiar to different domains of inquiry. We will declare two theoretical physical assertions to have the same sense when one is obtained from the other by a conversion of a mathematical expression it contains; but this is not permissible in general with mathematical assertions. We will say of a formulation of a mathematical proposition that its sense is not changed by an elementary logical transformation; but this will no longer hold when the elementary logical relations themselves are considered. We have only considered the sameness of sense for statements; but the same can be said for predicates and definitions. Thereby the consideration of mathematical definitions yields many examples in which the contrast between extension and intension agrees with our usual scientific way of thinking. Let us take the representation of a positive real number by an expression of analysis, e. g., an infinite series or a definite integral. The extension of this definition is the real number itself, and the intension is a rule to determine this number, i. e., for it being contained in arbitrary small intervals. As is well-known, one and the same real number can be determined by very different such rules; then we have the same extension with different intensions.

To mention also a mathematical example of a predicate having the same extension with different intensions, the prime numbers among the numbers different from 1 can be characterized in two different ways: On the one hand, as those that have no proper divisor other than 1, on the other hand, as those that only divide a product if they divide at least one factor. This results in two different intensions of a predicate with the same extension: the extension is the class of prime numbers, the intensions are the two definitions of the concept "prime number" that correspond to the characterizations. Analogous examples can also be found in the empirical sciences, e. g., when it is possible to characterize an animal species in different ways, so that different definitions result in the same concept of species, and thus the name of the species has different intensions and the same extension.

On the one hand, our considerations show that there are large classes of cases in which the concept of intension has a natural scientific application. On the other hand, we have become aware of the difficulties with the concept of sameness of sense, which are related to different viewpoints in the different areas of research, whereby it does not suffice to contrast the logical with the extra-logical in order to account for the differences.

We can approach the relevant point by calling to mind the type of abstraction that is involved in the concept of intension. Here one does not start with the distinction between the form of the expressions of the language and their expressive function, but rather this latter is consciously retained. What is abstracted away are the particularities of the means of expression that are irrelevant for this function, and the variety of formulations that are based on them, which can be used for the same expressive purpose. This manifold of possibilities consists, from a conventional point of view, on the one hand, in the multiplicity of languages, and on the other hand, in conceptual and factual equivalences that can hold between definitions, properties, and relations. Such an equivalence warrants the substitution of an expression by another only if it is totally unproblematic in the framework of the exposition or investigation in which the expression is used, i. e., if it belongs to the domain that is not under discussion, but which is taken for granted. In fact, our search for knowledge, at least at the stage of developed reflections, is based on a certain supply (of which we are more or less conscious) of ideas, opinions, and beliefs, to which we, either consciously or instinctively, hold on in our questions, considerations, and methods. Following Ferdinand Gonseth's concept of "préalable," such ideas, opinions, and beliefs may be called "antecedent."

The assumption of certain antecedent ideas and premises for any scientific discipline and also for our natural attitude in day-to-day life, is not subject to the same problems as the assumption of *a priori* knowledge. It is not claimed that the antecedent ideas are irrevocable. A science that is initially based on a premise can lead us to abandon this premise in its further development, whereby we may be compelled to change the language of the science. The scientific method also requires that we make ourselves aware of the antecedent premises, and even make them the object of an investigation, resp., include them in the subject matter of an investigation.

These premises thereby lose their antecedent character for the research area in question. In the course of development of the theoretical sciences, this leads to the situation that more and more of the premises are subjected to investigation, so that the domain of what is antecedent becomes narrower and narrower. The specially formulated initial concepts and principles then take the place of the earlier, spontaneously formulated antecedents.

In contrast to the concept of the *a priori*, the concept of the antecedent is related either to a state of knowledge or to a discipline; nothing is assumed that is antecedent in an absolute sense.

Given the notion of antecedent, one can formulate the following definition of synonymy: two statements of a disciple have the same sense if the equivalence between them is antecedent to the discipline. The synonymy of predicates and definitions would have to be explained correspondingly. In the definition, the discipline can also be replaced by a state of knowledge, with respect to which one can speak of an antecedent in a sufficiently determinate way.

It appears that the observed difficulties with the determination of the sameness of sense can be removed in this way. To be sure, in this explanation one has to accept that the synonymy of sentences depends on the discipline or state of knowledge in which it is considered. But on closer inspection this turns out not to be so paradoxical.

3

Let us now turn to the extension of the methodological framework of the *Logical Syntax* that Carnap obtains by contrasting the theoretical language with the observation language.

When considering the method of natural science, we usually contrast theory and experiment. But in the initial form of logical empiricism, the theoretical aspect was not really recognized; only the course of discussions about the initial position, in which Karl Popper in particular was involved, has the preference emerged for the revised point of view of the *Logical Syntax*, in which formulations of laws of nature figure as proper sentences of the language of science.

One can understand that there was initially some resistance to this when it is realized that, with the acknowledgment of the role of statements of physical laws as proper sentences, the dualism of "relations of ideas" and "matters of fact," which was originally proposed by David Hume and which the Vienna school strived to maintain in a somewhat more precise form, turns out not to be exhaustive. On the one hand, the statements of laws of the natural sciences are not statements about "relations of ideas," i. e., not sentences of pure logic or pure mathematics; on the other hand, neither are they statements of matters of fact, since they have the form of general hypothetical sentences.

In Carnap's terminology, this result says that the domain of the descriptive (the extra-logical) does not coincide with that of the factual, but rather that the domain of the factual is narrower.

The same situation can be elucidated also from a different point of view. Carnap explains the concept of logical truth using "state description" in his book *Meaning and Necessity*.[c] He thereby follows Leibniz's idea of "possible worlds": what is necessary must hold in all possible worlds; and the "state descriptions" schematically represent the constitutions of the possible worlds. Thus, Carnap now defines: a sentence is logically true, if it holds for every "state description." In this approach, the concepts of necessity and possibility occur. But nowhere is it said that one can speak of necessity and possibility only in the logical sense. Carnap himself mentions the investigation of the non-extensional operators expressing physical and causal modalities under the open problems for semantics in the appendix to his *Introduction to Semantics* (*vide* [**?**], § 38*d*, S. 243). Physical and causal modalities concern what is possible within natural laws and what is necessary in nature. Now if the laws of nature are stipulated as valid in the framework of the language of science, and, furthermore, it is acknowledged that the laws of nature are not logically necessary, then a distinction between between what is necessary and what is actual follows which is different from that between what is logical and what is descriptive. Then we can consider "state descriptions" in a narrower sense by admitting only those that conform to the laws of nature, and we thereby obtain a narrower range of possible worlds.

Thus, not only are factual statements contrasted with the statements of logical laws, but more generally with any statements of laws. We can now express this more general contrast using the concept of the theoretical, by comparing the statements about what is actual with theoretical statements. Then the domain of what is theoretical contains what is logical as a proper subset.

What is specific to the theoretical surely consists not only in the totality of statements that are recognized as valid, but above all in a world of concepts, in the framework of which the theoretical statements occur. Within the language of science, the theoretical formation of concepts finds its place in what Carnap calls the "theoretical language."[1]

[1]By following Carnap in speaking here simply of "the theoretical language," the idea of a universal science should not be implied. Nor is this the case in Carnap's own discussions of the theoretical language. He speaks for instance of "methodological problems, that are connected to the construction of a theoretical system, like one for theoretical physics" (*vide* [**?**], pp. 241–242).

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[c] *Vide* [**?**].

Let us now take a closer look at the role that Carnap assigns to the theoretical language. In his view the theoretical language is not immediately interpreted, but rather the theoretical terms obtain their significance only in connection with the "correspondence postulates," which establish the relations between the theoretical terms and the observational terms. However, these relations are not thought to be so extensive that they define all the theoretical terms in the observational language. Instead, Carnap agrees with the view that the requirement that every theoretical term be experimentally definable and its use be bound by such a definition is too restrictive for theoretical research, and that it is not in accordance with actual practice in the theoretical sciences, as has been expressed in neo-positivist circles, in particular by Herbert Feigl and Carl Hempel.

A fundamental requirement for the freedom of the theoretical formation of ideas is hereby acknowledged. But it remains the fact that the theory is not seen as a world of ideas, but only as a linguistic apparatus, so to speak. The reduction to the purely mathematical is added as another characteristic feature to this more technical aspect that Carnap attributes to the theoretical language. Whenever possible Carnap strives to reduce theoretical entities to mathematical ones. This possibility is shown in the domain of physics in a particular way by the presentation of field theory, whereby physical events consist of a succession of states in the space-time continuum. The determination of states is given by scalars, vectors, and tensors.

For example, the description of physical state in the pure field theory of gravitation and electricity employs both the symmetric tensor of the metric field, from which the measurement of length and time and the forces of inertia and gravitation are determined, as well as the antisymmetric electro-magnetic tensor, which determines the electrical and magnetic forces. Particles of matter, either charged or uncharged, are understood here as particularly concentrated distributions of the field magnitudes in a spatially confined part of the world. The components of the tensors are functions of space-time positions, and when a coordinate system is introduced and units are chosen, the magnitudes of the components become mathematical functions of the space-time coordinates;[2] let us call them "field-functions." The physical laws of the field are formulated by the differential equations for these mathematical functions (in a way that is invariant with respect to the coordinate system),

[2][1] Initially the components of the metric field are unspecified numbers.

and the field-functions which represent the sequence of states of the system form a solution for this system of differential equations.

The connection between the theory and the world of actual experience is given through various kinds of relations:

1. those on which the introduction of space-time coordinate systems and the possibilities of the values of the field-functions are based,

2. those which concern the effects of system states partly on our direct perceptions and partly on our experimental observations,

3. those which yield the instructions for the theoretical translation of a case that is observationally given (either only schematically or in a precise experimental determination) and is to be investigated using the theory.

Carnap thinks that all these relations are axiomatizable by the correspondence postulates in which the links between the field-functions and our observations are expressed. Such a system of correspondence postulates can only be formulated if the manifold of possible applications of field theory (of the differential equations of the field) to observations is axiomatizable.

With these reservations, the possibility is therefore given to wholly restrict the theoretical language of physics to mathematical concepts, transferring everything that is specific to physics either into the observational language or into the correspondence postulates. Then the theory of physics no longer makes statements about something that exists in the physical nature, it does not even state anything at all by itself, but only yields a mathematical tool for the predictions of observations on the basis of given observations. Strictly speaking, one should not talk here of a theoretical language at all.

But a kind of theoretical language can still be regained by introducing suitable physical names for certain often recurring mathematical relations and expressions according to the meanings that they have in the interpreted theory; then the procedure is analogous to interpreting geometrically the arithmetical relations and objects of an (analytical) geometry that is constituted purely arithmetically.

What is perplexing in the described method of eliminating theoretical entities is the fact that it can be applied to any treatment of natural objects. If the assumption of natural objects is appropriate in the familiar cases of daily life and, furthermore, if we extrapolate familiar methods of orientation in place and time to the notion of the four-dimensional space-time manifold, then it does not seem appropriate to discontinue, so to speak, the treatment of natural objects at a certain point and to replace the objects there by their mathematical descriptions.

However, Carnap can reply to this consideration that the difference of the methodical treatment does not relate to the differences of positions in the space-time manifold, but rather to the different theoretical levels. What is meant by such a difference of level can be exemplified with the distinction between macro- and micro-physics. In general, a further theoretical level is present in the treatment of a domain of knowledge where the formation of concepts forces a further remove from the intuitively familiar. Such a step of increased theoretical character can be successful and prove satisfactory, and a practical confidence can arise in the course of applying the initially unfamiliar concept. But here the difference remains between what is methodologically more or less elementary, i. e., between what is closer and farther away from concrete observations.

It is obvious that quantum physics involves an increased theoretization, in the sense described above, compared with the previous "classical" physics. But the method for the elimination of entities described above cannot be applied directly to quantum physics, since here the idea of a definite sequence of states in the space-time manifold that is determined objectively and independently of experiments is lost. In a different respect quantum physics is well-suited to the aims of the method of elimination, since here the idea of objecthood is already weaker, and mathematical considerations dominate the concept formations. Quantum physics also shows us how the different methodological treatments of diverse theoretical entities can be implemented without objectionable disruptions, by giving the role of the observation language, so to speak, to the theoretical language of the previous physics.

At the same time it is hereby suggested that it is reasonable to relate the distinction between the observation and theoretical languages to the level of the concept formations, instead of taking it to be absolute. If we consider the role of the observation language in scientific practice this idea is confirmed. When physicists talk about their experiments they surely do not speak only of objects of immediate perception. One talks maybe of a piece of wood, of an iron rod, of a rubber band or of a mercury column. But the language in which physicists report their experiments goes much farther in this respect.[3] It is

[3][1] Indeed, the claim has been made that all experiments in physics turn out to be statements about coincidences. But surely this claim has to be taken only *cum grano salis*: The statement of coincidence (or non-coincidence) is in each case only the last decisive step in the overall process of an experiment. Moreover this requires that the person conducting the experiment understands the equipment and handles it correctly,

also noteworthy that many terms for the concepts of physics (like "barometric pressure," "electrical current") have entered into everyday language.

On the whole, the situation can be characterized by saying that an observational language of a science that is at a certain level refers to an antecedent world of ideas and concepts—"antecedent" in the sense introduced in our section 2. The antecedent theoretical concepts also obtain their terms in the observational language at this level. We do not need to separate the observational language from the colloquial language at all. The observational language can instead be understood as a colloquial language that has been augmented by a larger set of terms.

The relativization of the observational language to a conceptual level is also appropriate to the kind of opposition between what is empirical and what is theoretical that is intended by Ferdinand Gonseth's principle of duality. What is meant here is that there are no distinct empirical and theoretical domains, but that both aspects come into play in every domain and in every stage of knowledge. The different points of view in the above considerations: the elimination of abstract entities, the distinction between theoretical levels, and the relativization of the observational language to a conceptual level, all have their application in particular to mathematical proof theory. The latter assumes a distinction between the "classic" method of mathematics that is applied in analysis, set theory, and the newer abstract mathematical disciplines, and the more elementary methods that are characterized as "finitist," "constructive," or "predicative," depending on the restrictions at issue. In the proof-theoretic investigation of classical mathematics, an elimination of abstract entities is made possible by the method of formalizing propositions and proofs using logical symbolism. One attempts to use this elimination to prove the formal consistency of classical theories from one of the more elementary standpoints. So far formal consistency proofs using constructive methods have been obtained only for such formal systems as can be interpreted at least predicatively. Recently, it appears that a consistency proof for formalized impredicative analysis is possible from a broad view of the constructive standpoint, by a method developed by Clifford Spector.

also that this apparatus has been set up appropriately. Moreover the scientist should have sufficiently confirmed that no interferences occur, etc. It is hardly the case that everything that has to be understood and done in order to achieve this can be reduced to simple statements about coincidences. But this is surely not intended by that thesis.

The elementary "meta-language" in which such a consistency proof is carried out has the role of an observation language, as has been noted by Carnap. Originally it was Hilbert's idea that this language should remain totally within the framework of concrete considerations, i. e., should be an observation language in the absolute sense. But gradually one has been forced to include more and more theoretical terms. The "finitist standpoint" already uses strictly more than Hilbert originally wanted to allow; but even this standpoint turned out to be insufficient for the intended purpose, according to the results of Kurt Gödel. The consequences of this result appear not to be as fatal to proof theory as it initially seemed, if one accepts the idea of relating the observation language to a conceptual level. The acknowledgment of the methodological importance of proof-theoretic investigations, and in particular those concerning formal consistency, is not tied to the view that conventional, classical mathematics is dubious, or to the standpoint of "formalism" according to which classical mathematics is justified only as a purely formal technique. Hilbert essentially never thought of this, despite some remarks that point in this direction. The task of constructive consistency proofs is motivated by the high theoretical level that is present in classical mathematics.

In any case, an adherent of the constructive proof-theoretic direction of research can very well have the point of view also favored by Carnap, that the concept formations of classical mathematics have justified application when taken as interpreted. But whether it is reasonable to accept all entities that are introduced by set theory as real is open for discussion from this standpoint as well. Nor is one inclined to withhold the positive status from the theoretical concepts, awarding that privilege only to mathematical concept formation; what is just for mathematical classes and functions is equitable for the entities of the natural sciences, insofar as they are used in a way that promotes understanding.

Chapter 25

Bernays Project: Text No. 26

Remarks on the philosophy of mathematics (1969)

Bemerkungen zur Philosophie der Mathematik

(*Akten des XIV. Internationalen Kongresses für Philosophie, Wien, Band III: Logik, Erkenntnis- und Wissenschaftstheorie, Sprachphilosophie, Ontologie und Metaphysik*, Wien: Herder, pp. 192–198;
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When we compare mathematics with logic in regard to the role assigned to these two domains of knowledge within philosophical thinking, we find a disagreement among philosophers.

For some logic is singled out; for them, logic in the broader sense is the $\lambda\acute{o}\gamma o\varsigma$, that what is rational, and logic in the narrower sense is the inventory of elementary insights which should lie beneath all considerations, i. e., the inventory of those truths that hold independently of any particular factual content. Thus, logic in the narrower sense ("pure logic") has a primary epistemological status.

A different starting point takes the method of mathematics as exemplary for all scientific thinking. While for the first point of view the logical is what is the obvious and unproblematic, for the second point of view the mathematical is what is epistemologically unproblematic. Accordingly, understanding is ultimately mathematical understanding. The idea that all rational insight must be of a mathematical kind plays a fundamental role particularly also in the arguments of David Hume.

For this point of view Euclid's Elements counted for a long time as a paradigm of the mathematical method. But often it was not sufficiently clear that from the standpoint of axiomatics the Euclidean axiom system is special (the fact that early commentators had already come up with suggestions for replacing axioms by equivalent ones was an indication of this). Obviously many were of the opinion—although probably not the authors of the Greek work—that the possibility of a strict and successful proof in geometry is based on the evidence of the axioms.

Those who philosophized according to the axiomatic method, in particular in the school of Christian Wolff, at times understood evidence as conceptual evidence, so that they did not distinguish between the logical and the mathematical. The principle of contradiction (which mostly included the principle of excluded middle was regarded as a magic wand so to speak, from which all scientific and metaphysical knowledge could be obtained with the help of suitable concept formations.

As you know, Kant has emphasized in opposition to this philosophy the moment of the intuitive in mathematics in his theory of pure intuition. But also for Kant the possibility of geometry as a successful deductive science is based on the evidence of the axioms, that is in his case, on the intuitive clarity and certainty of the postulates of existence. That the discovery of non-Euclidean, Boyai-Lobatschefskian geometry had such a revolutionary effect on philosophical doctrines is explained by the unclarity in epistemological judgment about Euclid's geometry.

But a fundamental change of aspect resulted also for the first of the two mentioned points of view from the development of mathematical logic. It became clear that logic as a discipline (which it was already with Aristotle) does not consist directly in establishing singular logical facts, but rather in investigating the possibilities of proofs in formally delimited domains of deduction, and should better be called metalogic. Furthermore, the method of such a metalogic is typically mathematical.

Thus it might seem appropriate to classify logic under mathematics. The fact that this has mostly not been done is explained by the lack of a satisfactory epistemological view of mathematics. The term "mathematical" was not, so to speak, a sufficiently familiar philosophical term. One tried to understand mathematics itself by classifying it under logic. This is particularly true of Gottlob Frege. You surely know Frege's definition of cardinal number in the framework of his theory of predicates. The method employed here is still [redacted] important today for the classification of number theory under set theory. Various objections (which might be discussed in a group of those interested) can be raised against the view that hereby an *epistemological* reduction to pure logic has been achieved.

A different way of approaching the question of the relation between mathematics and logic is—as is done in particular by R. Carnap—to regard both as being analytic. Thereby the Kantian concept of analyticity is fundamentally extended, which has been pointed out especially by E. W. Beth. For the most part the same character of obviousness that is ascribed to analytic sentences in the Kantian sense is attributed to analyticity in this extended sense.

As you know, W. V. Quine has fundamentally opposed the distinction between the analytic and the synthetic. Although his arguments contain much that is correct, they do not do justice to the circumstance that by the distinction between the analytic in the wide sense and the synthetic, a fundamental distinction is hit upon, namely the distinction between mathematical facts and facts about natural reality. Just to mention something in this regard: Mathematical statements are justified in a different sense from statements in physics. The mathematical magnitudes of analysis are relevant for physics only approximately. For example, the question whether the speed of light is measured in the centimeter-second-system by a rational or an irrational number has hardly any physical sense.

To be sure, the fundamental difference between what is mathematical and what is natural reality is not a sufficient reason to equate mathematics with logic. It appears natural to count as logic only what results from the general conditions and forms of discourse (concept and judgment). But mathematics is about possible structures, in particular about idealized structures.

Herewith, on the one hand the methodical importance of logic becomes apparent, but on the other hand also that its role is in some sense anthropomorphic. This does not hold in the same way for mathematics, where we are prompted to transcend the domain of what is surveyable in intuition in various directions. The importance of mathematics for science results already from the fact that we are concerned with structures in all areas of research (structures in society, structures in the economy, structure of the earth, structures of plants, of processes of life, etc.). The methodical importance of mathematics is also due to the fact that a kind of idealization of objects is applied in most sciences, in particular the theoretical ones. In this sense F. Gonseth speaks of the schematic character of the scientific description. What differentiates the theoretically exact from the concrete is emphasized especially also by Stephan Körner. As you know, science has succeeded in understanding the connections in nature largely structurally, and the applicability of mathematics to the characterization and explanation of the processes in nature reaches much further than humanity had once anticipated.

But the success and scope of mathematics is something entirely different from its so-called obviousness. The concept of obviousness is philosophically questionable in general. We can speak of something being relatively obvious in the sense in which, for example, the mathematical facts are obvious for the physicist, the physical laws for the geologist, and the general psychological properties of man for the historian. It may be clearer to speak here of the procedurally prior (according to Gonseth's expression "préalable") instead of the obvious.

At all events mathematics is not obvious in the sense that it has no problems, or at least no fundamental problems. But consider for instance, that there was no clear methodology for analysis for a long time despite its great formal success, but the researchers had to rely more or less upon their instinct. Only in the 19th century precise and clear methods have been achieved here. Considered from a philosophical point of view the theory of the continuum of Dedekind and Cantor, which brought the justifications of these methods to an end, is not at all simple. It is not a matter of the bringing to consciousness of an *a priori* cognition. One might rather say that here a compromise between the intuitive and the demands of precise concepts has been achieved which succeeded very well. You also know that not all mathematicians agree with this theory of the continuum and that the Brouwerian Intuitionism advocates a different description of the continuum—of which one can surely find that it overemphasizes the viewpoint of strict arithmetization at the expense of the geometrically satisfying.

The problems connected with the antinomies of set theory are especially well known and often discussed. As you know, different suggestions have been brought forward to repair the antinomies. In particular, axiomatic set theory should be mentioned, which shows that such a small restriction of the set theoretic procedure suffices to avoid the antinomies that all of Cantor's proofs can be maintained. Zermelo's original axiom system for set theory has been, as you surely know, on the one hand extended, on the other hand made formally sharper. The procedure of solving the antinomies using axiomatic set theory can be interpreted philosophically as meaning that the antinomies are taken as an indication that mathematics as a whole is not a mathematical object and therefore mathematics can only be understood as an open manifold.

The application of the methods of making formally precise to set theory resulted in a split of the set theoretic considerations into the formulation and deductive development of formal systems, and a model theory. As a result of this split the semantic paradoxes, which could be disregarded at first for the resolution of the purely set theoretic paradoxes, received new formulation and importance. So today we face a new set of fundamental problems, which, to be sure, does not bother mathematics in its proper research, just as the set-theoretic antinomies did not earlier. Rather, [mathematics] unfolds in the different disciplines with great success.

The above remarks suggest the following viewpoints for philosophy of mathematics, which are also relevant for epistemology in general:

1. It appears appropriate to ascribe to mathematics factual content, which is different from that of natural reality. That other kinds of objectivity are possible than the objectivity of natural reality is already obvious from the objectivity in the phenomenological regions. Mathematics is not phenomenological insofar, as has been said before, it is about idealized structures on the one hand, and on the other hand it is governed by the method of deduction. With idealization, intuitiveness comes into contact with conceptualization. (Therefore, it is not appropriate to oppose intuitiveness and conceptualization so heavily as it is done in Kantian philosophy).

The significance of mathematics for theoretical physics consists in the fact that therein the processes of nature are represented approximatively by mathematical entities.

2. It does not follow from the difference between mathematics and empirical research that we have knowledge in mathematics that is secured at the outset (*a priori*). It seems necessary to concede that we also have to learn in the fields of mathematics and that we here, too, have an experience sui generis (we might call it "intellectual experience"). This does not diminish

the rationality of mathematics. Rather, the assumption that rationality is necessarily connected with certainty appears to be a prejudice. We almost nowhere have certain knowledge in the simple, full sense. This is the old Socratic insight which is emphasized today especially also in the philosophies of F. Gonseth and K. Popper.

We have certainly to admit that in mathematical considerations, in particular in those of elementary mathematics, we possess a particular kind of security, because on the one hand the objects are intuitively clear and, on the other hand, almost everything is stripped off by the idealization of the objects that could lead to subjectivity.—But when we talk about the certainty of $2 \cdot 2 = 4$ in the popular sense, we think at the concrete applications of this statement. But the application of arithmetical statements to the concrete is based on empirical conditions, and for their compliance we only have an empirical, even if practically sufficient certainty.

By dropping the coupling of rationality and certainty we gain, among other things, the possibility to appreciate the *heuristic rationality*, which plays an essential role for scientific inquiry.

The acknowledgment of heuristic rationality provides in particular the solution to the epistemological difficulty that has been made a problem by David Hume: we can acknowledge the rational character of the assumption of necessary connections in nature, without having to claim that the basic approach of [assuming] such connections guarantees success; with regard to this success we depend in fact on experience.

Chapter 26

Bernays Project: Text No. 27

Schematic correspondence and idealized structures (1970)

Die schematische Korrespondenz und die idealisierten Strukturen

(*Dialectica* 24, pp. 53–66;
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1.

Among the theses that are characteristic of Ferdinand Gonseth's philosophy there is one which, at first glance, seems less specific than the others, but which, on closer inspection, reveals itself to be especially important. It is the claim that in our theoretical description of nature we do not arrive at an adequate representation of reality, but only at a schematic correspondence.

Initially this statement is perhaps open to misunderstanding, and a detailed discussion of its content may not be superfluous. What is definitely not meant is that any kind of representation of an object or process in nature as provided by the natural sciences is merely a schematic representation. In fact, theoretical inquiry in science provides us with a variety of possibilities for letting nature, as it were, work for us, and the depictions obtained along these lines have a high degree of perfection and are far from schematic; e. g., the depiction of objects by means of a good photographic image or the rendering of a sound by means of a good radio reproduction.

What is declared to be schematic is, instead, the representation of a situation or process in a theoretical "description." Here the schematic aspect comes into play from the very beginning insofar as the description is always fitted to a certain scale of the investigation. It is, in particular, characteristic that physics, in its continuing exploration of smaller and smaller phenomena, is led successively to new kinds of objects and laws.

In this development the old idea of atomism has been confirmed in an impressive way, but not in the sense that with the atoms we have found something so to speak final, indivisible, and unchangeable. The study of aggregate states leads to the composition of matter out of molecules; the study of chemical processes leads to the composition of molecules out of atoms; and in microphysical research the atoms themselves reveal themselves to be structured in a complex way, as complexes of even smaller parts which can be separated if subjected to strong enough forces.

One consequence of discovering smaller and smaller components of physical entities is that the majority of natural processes are to be conceived as mass phenomena and, hence, to take many of the usual laws to have a schematic character insofar as they are based on the explanation of processes as involving averages.

Another schematic aspect of the laws of physics consists in the following: During the development of our theories many of the initially formulated and empirically confirmed laws come to be seen as mere approximations of more complex, but also more comprehensive laws. Thus even Newton's law of gravitation, long regarded as a fundamental law of physics, is now derived from Einstein's theory of gravitation as an approximate consequence.

In all these cases the schematic character of the representation does not by any means signify a deficiency; rather, the realization that a certain more complex structure can be replaced, to a degree that is perfectly adequate for the given purposes, by a certain much simpler structure constitutes an additional insight. The corresponding approximate representation is also completely adequate with respect to the given realm of applications; it is just not adequate absolutely, i. e., for every kind of application.

Let us look at this situation a bit more closely. Most scientific investigations concern only a limited space-time region, for which the effects of its [further] environment are taken into account only as general boundary conditions, so schematically, or they are neglected altogether. Cosmological theories, on the other hand, aim at a mathematical-physical description of nature as a whole; as such they are forced to schematize even more, since here we are only dealing with global relations.

A kind of neglect that is, as it were, unintentional derives from the fact that at every level of research only certain kinds of structures, processes, and dependencies are known to science. Thus for the characterization of, say, a certain condition only the known aspects can be taken into account.

In spite of the tremendous expansion of human knowledge concerning law-governed structures and various forms of theoretically comprehensible connections during the previous and the current century, there is no reason to assume that we will soon come to an end in that regard.

The principle of the schematic limitation of theoretical description can be applied especially to the idea of *determinism*, i. e., the idea that the totality of natural processes within a sufficiently closed domain is determined in its development, uniquely and exhaustively, by mathematical laws, if we start from any fixed momentary state. Organic processes are meant to be included here as well, as are human life and human actions.

This view depends on the assumption that natural processes can be represented adequately by the solution to a system of differential equations. As is well known, this assumption is dropped in contemporary quantum mechanics; according to that theory microscopic processes are not determined uniquely by means of differential laws, these laws provide only determinations of probabilities. But even if one argues that such indeterminacy only concerns microscopic processes and that for macroscopic processes we nevertheless get deterministic laws as a result, those resulting laws still have the feature of being schematic; and that feature already constitutes a sufficient counter-argument against a strict form of determinism.

It should be emphasized here that the rejection of strict determinism does not at all mean the abandonment of our usual causal thinking. After all, the principle of causal investigation—which states that if we observe a deviation from a steady state or from the normal development of a process we can expect to find an explanatory cause for that deviation—does not in itself include determinism.

Furthermore, the deterministic form of physical laws still remains crucial for the application of these laws in deriving predictions. A rejection of determinism is, thus, only justified with respect to determinism in the sense mentioned above, i. e., determinism as an extreme philosophical doctrine.

This doctrine plays a role, in particular, in the longstanding debate about human freedom of the will.[1]

One can look at this question from many different perspectives. On the one hand, from the point of view of experience one can point out that especially with respect to important human decisions, those with respect to which one is more strongly engaged, emotional drives are usually so dominant that there is no question of an arbitrary choice. The role of the will is here comparable with that of the executive of a state who is given the more discretion the less important the decision in question is.

On the other hand, if freedom of the will is called into question from the point of view of determinism the case is quite different. In that case human actions are viewed either in terms of physical or physiological laws, in the sense of psycho-physics, or one imagines psychological research into the mind to have been brought to such precision that it is capable of exact prediction. However, an appeal to the principles and methods of psycho-physics or psychology cannot ground a strict form of determinism with respect to human freedom of the will; this becomes clear as soon as we remember the fundamental schematic limitation that is characteristic of the scientific description of processes and states. Let us assume, e. g., that biology succeeds in determining the gene structure, thus also the hereditary disposition, of a human being experimentally; then this determination can still hardly be of a kind in which the observing researcher or, say, the registering apparatus has access to all the abilities contained in the hereditary disposition of the corresponding human being. In other words, the registered data can hardly be equivalent to the potentialities contained in the hereditary disposition. But that would be required if one wanted, just on the basis of determining the hereditary disposition of each human being as well as the influence of environmental factors, to give a detailed prognosis for someone's attainments.

[1/1*] Gonseth has presented his thoughts on this issue in *Determinism and Free Will* (*vide* [**?**]). (Footnotes 1* to 3* were added later [to the reprint in the *Abhandlungen*].)

2.

So far we have considered the schematic only in the sense of a limitation, as the merely schematic and abstract as opposed to the richer concrete and the living. But this is only one side of the story; and it would be an inadequate interpretation of schematic correspondence in the sense of Gonseth, too, if one thought of the schematic *eo ipso* as a coarsening. Among the schemata used in scientific description belong, after all, in particular, the geometric figures, and they have a kind of perfection that can be attained only approximately by concrete things. A concrete spatial object can only roughly, but never precisely, have the form of a sphere; similarly, a concrete length can only approximately be the middle proportional between two different lengths. Thus, there is a kind of reciprocity between the concrete and the schemata: on the one hand, the schemata do in general represent the concrete only approximately; on the other hand, the schemata can in general be realized only approximately by concrete objects.

What reveals itself in this reciprocity is the fact that in the schemata we are confronted with a kind of objectivity that is *sui generis*; it is the objectivity of the *mathematical*.

Overall, mathematics can be understood as the science of schemata with respect to their internal constitution. Seen as such, the essential role played by mathematics in the theoretical sciences has been acknowledged in terms of the idea of schematic correspondence, while the fundamental difference between mathematical objectivity and the objectivity of nature has also been taken into account.

Mathematical objectivity arises, by means of processes of idealization and abstraction, out of the phenomenal objectivity of the *structural*.

Recently the topic of structure has been discussed by Mr. Gonseth with regard to "structuralism," namely in connection with the methodological issues of axiomatization and formalization.[2*] Let me add a few remarks about these topics.

a) To begin with, as far as the role of structure in general is concerned structure can be regarded as that in the phenomena which goes beyond their qualities. The common opposition between quality and quantity may be adequate for some purposes in ordinary life, but that between the qualitative

[2*] *Vide* [**?**].

and the structural is certainly more fundamental. Assessing the quantitative comes down to processes of joining together and of observations [such] as that one object extends beyond another; both of these have a structural character. In contrast, a general reduction of the structural to the quantitative can hardly succeed in a phenomenological sense, i. e., by way of direct description, but at best in a theoretical sense, say that of Pythagoreanism, whereby qualitative differences are, however, also reduced to quantitative ones.

In mathematics we are usually not dealing with structures that are given directly in a phenomenal sense; rather, we are dealing with idealized structures, where the idealization consists in an adaptation to the conceptual, a compromise between the intuitive and the conceptual, as it were.

We should note here that in the enterprise of constructive mathematics the goal is to restrict idealization as much as possible. But this does not succeed completely; in particular, even constructive mathematics cannot do without the idea of the unlimited applicability of arithmetic operations (sum, product, exponentiation, etc.).

b) Mathematical idealization becomes especially pertinent through the *axiomatic* treatment of theories. As is well known, there are two different kinds of axiomatics. Mr. Gonseth, in his book *Le Problème du Temps*,[3*] calls them *axiomatisation schématisante* and *axiomatisation structurante*. With respect to the first, one relies on an already given language, a language in which the objects and relations under consideration have names; here the axiomatic aspect consists, on the one hand, in sharpening this language in the sense of a schematization of the relevant objects and, on the other hand, in adopting certain claims about these objects that are assumed to hold as the starting points for logical deductions. With respect to the second kind of axiomatization, the original objects and relations do not occur independently any more, but only as links in an overall structure—they occur merely in their grammatical role, as it were—, and the axiomatic system makes assertions about this overall structure.

For a number of axiomatic systems of this second kind, a definitional formulation is the most common, e. g., for the axiomatic system for groups. Thus one says: a domain of objects for which a composition $ab = c$ is defined is called a group with respect to this composition if 1. the composition is associative and 2. the composition is invertible on both sides, i. e., for any

[3*] *Vide* [**?**].

two objects a, b (in the domain of objects) there exists an object x in the domain such that $ax = b$, as well as an object x such that $xa = b$.

These conditions can also be formulated as "the group axioms." It is clear, then, that we are confronted with a definition, and not an implicit, but an *explicit* definition. Of course, what is defined is neither the domain of objects nor the composition. Those two occur only implicitly in the definition. What is defined, instead, is what a group is, or better, the condition under which a domain of objects together with a composition operation defined on it forms a group.

There are, however, groups with very different structures. Thus what is characterized by the group axioms is not a determinate structure, but a *kind of structures*. The case of an axiomatic system that characterizes a structure uniquely is only a special case. Such an axiomatic system, one for which any two realizations ("models") are structurally identical ("isomorphic"), is called "categorical."

On the other hand, one and the same species of structures can in general be defined by means of several different axiomatic systems: which of the theorems holding in the structure are adopted as axioms is not determined by the structure itself; also, the choice of the basic predicates or basic operations, respectively, is not determined by the structure: what is a basic predicate with respect to one axiomatic system can be a (definitionally) derived predicate for another system that defines the same kind of structures.

In this way there exist equivalence relations between axiomatic systems. A different relation between such systems that is important methodologically is that in which one axiomatic system forms an extension of another. Here we have to distinguish two possibilities: One is that the basic domain remains the same, but new axioms are added; in this case the characterized kind of structure is (in general) restricted. The other consists in adding new basic predicates or operations, in addition to corresponding new axioms; in that case one moves over to a richer structure. The linear continuum, e. g., if assumed to be endowed with a measure, is a richer structure than the linear continuum considered only as an ordered manifold.

c) It is by means of logical inference that axiomatic systems are intended to be used. The methods of proving things logically have been analyzed by mathematical logic. The result of this analysis is that for proofs in elementary theories the predicate logic of "first-order" is sufficient. It consists of sentential logic, i. e., the rules concerning the sentential connectives "and," "or," "not," "if, then," as well as the rules for the universal form and the existential form, and the rules for equality. Logical inference within this framework can be schematized so precisely that, by using symbols for the sentential connectives and for "all" and "there exists," all contentual proofs can be translated into the combined application of a few schematic rules.

This leads to a new kind of structures: the structures of formal deductions. Between the theorems of an axiomatic theory that can be formulated within the logical framework mentioned and the sentence-formulas that are deducible according to the rules of the theory formalized as a calculus there is a complete correspondence. This harmony between the "semantics" and the "syntax" of the theory is established by Gödel's Completeness Theorem, which says: A sentence of the theory is deducible by means of the formal rules if and only if it cannot be refuted by means of a "model."

We are already led beyond the framework of logical deduction described so far wherever the general concept of a *finite number* is used. This happens—just to mention a few elementary examples—in geometry when statements about arbitrary polygons or arbitrary polyhedra are made, furthermore in connection with general theorems in formal algebra and in the theory of finite groups. In all these cases the principle of mathematical induction is used.

More far reaching than such an arithmetic extension of first-order logic is the logic of "second-order." In it general concepts such as those of (one- or many-place) predicate, function (operation, mapping, sequence), and set are used, and the rules for universal and existential forms are applied to such concepts. The inference rules for second-order logic include, among others, the axiom of choice.

This logic of second order is first used in classical analysis, more extensively in set theory, and then in every domain where the set-theoretic way of thinking is employed, thus in particular in semantics, i. e., in those investigations that concern the satisfiability of axiomatic systems by models. In fact, the concept of satisfiability of an axiomatic system belongs already to second-order logic, as does the notion of semantic consequence (the semantic notion of implication). One says that a sentence is implied by an axiomatic system (in the semantic sense) if it is satisfied in every model of the axioms. The definition of the concept of "categoricity" requires second-order logic as well.

d) Second-order logic, i. e., the concepts of set, function, etc. that are essential for it, has in turn been subjected to analysis; and for a while it may have looked as if second-order logic could be reduced to first-order logic, by treating sets as mathematical objects and the element relation ("a is element of m") as a basic axiomatic relation, analogously to the incidence relation in geometry.

To be sure, in the corresponding axiomatic system formulated by Zermelo an axiomatic rule (the "Axiom of Separation") occurs in which, as in the case of the principle of mathematical induction, there is reference to an arbitrary predicate ("definite property"). But the employment of this concept of predicate can again be made more precise axiomatically, so that one arrives at an axiomatization within the framework of first-order logic.

In fact, by means of such an axiomatization all the proofs of classical analysis and of Cantorian set theory can be carried through, as well as formalized in logical symbolism. However, there is no longer a harmony between syntax and semantics here. The concept of predicate has been restricted by giving it a precise axiomatic formalization. This does not affect the usual proofs in number theory, analysis, or set theory; these proofs can, as indicated, be carried out within the framework of the axiomatic system; also, every sentence that is deducible within the axiomatic framework is true in the usual (classical) sense. But with respect to applying the Completeness Theorem we now have the complication that the concepts of "satisfiability" and "refutability" have a different sense depending on whether they are used in accordance with the axiomatic system or from the viewpoint of the semantics.

Any model of axiomatic set theory or, more generally, any model of a theory axiomatized first within the framework of second-order logic, but then reduced to first-order logic by axiomatically restricting the notion of predicate or, respectively, the notion of set or function, is called a "non-standard" model, at least if the restriction of the concept of predicate (or, respectively, of the concepts set or function) makes a difference; otherwise it is called a standard model.

One obtains a corresponding non-standard model for axiomatic set theory or analysis by means of Löwenheim's Theorem, which says that any axiomatic system that is formalizable in first-order logic and that is consistent has a model whose elements (individuals) are the natural numbers. Such a model can certainly not be a standard model, since it is provable in set theory, as already in analysis, that the number-theoretic functions (i. e., functions with numbers as arguments and as values)—which here count as individuals—are not enumerable (by the natural numbers). Thus one obtains a model that contradicts a theorem provable in the theory as interpreted externally.

In may seem now that such difficulties arise only in the case where we are dealing with uncountable manifolds; but in fact such difficulties can already be found in connection with number theory. Here, too, the restriction of the principle of mathematical induction to sentences of a certain form has non-standard models as a result. Here, once more, a number theoretic sentence that holds in terms of its content can contradict a sentence that holds in a model (externally). In any case, a non-standard model of number theory contains, besides the numbers $0, 1, 2, 3, \ldots$, also infinitely many other elements that function as "natural numbers" in it.

Then again, these remarks do not refute the view that the presence of non-standard models has to do with the uncountable: to be sure, the set of natural numbers is countable, in fact it is the prototype of the countable; but the properties (sets) of numbers that play a role in the principle of mathematical induction form an uncountable totality.

It should be noted, by the way, that the number-theoretic non-standard models cannot be eliminated by integrating number theory into a wider formalizable and axiomatic framework. Rather, because of Gödel's Incompleteness Theorems the following is the case: for any axiomatization of analysis or set theory that is consistent and can be formalized faithfully, there exist non-standard models which are already non-standard with respect to number theory.

e) The difficulties just considered attach to axiomatic mathematics, more precisely to any axiomatization within the framework of first-order logic that is of a stronger kind and allows for formalization. By making the deductive structure of a formalized theory one's object of study, as suggested by Hilbert, that theory is, as it were, projected into number theory. The resulting number-theoretic structure is, in general, essentially different from the structure intended by the theory; nevertheless, it can serve to recognize the consistency of the theory, from a viewpoint that is more elementary than the assumption of the intended structure.

Hilbert's idea was to obtain, along such lines, an elementary consistency proof for all of classical mathematics, thus to resolve the problem of the foundations of mathematics once and for all.

This program had to be revised in two respects. On the one hand, expectations with respect to how elementary the proof-theoretic considerations could be had to be lowered. The "finitist stance" envisioned by Hilbert proved to be insufficient for the purpose at hand; at the same time, it also became clear that this stance is more restrictive than that of Brouwer's intuitionism.[2]

The other way in which Hilbert's program needed to be revised concerns the idea of a definitive resolution of the foundational problems of mathematics. So far consistency proofs for formal systems of number theory and for fragments of analysis have been provided by a variety of methods, methods that all lie within the scope of intuitionist mathematics. Let us assume we succeeded in giving, within a suitably extended framework of constructive mathematics, consistency proofs for formal systems of classical analysis and for formalized axiomatic set theory; then this would still not provide final closure. Since, as mentioned above, the semantics of set theory goes essentially beyond set theory as made precise axiomatically. Moreover, the totality of mathematics can certainly not be represented exhaustively by one formally restricted theory. Mathematics as a whole—this is the lesson of the set theoretic antinomies—is not a structure in itself, i. e., an object of mathematical investigation, nor is it isomorphic to one.

Proof-theoretic considerations can, consequently, not encompass mathematics as a whole, but only particular restricted mathematical theories.

Even if the original goals of Hilbert's proof theory require modification in the two respects mentioned, this Hilbertian project has still proved very fruitful. Proof-theoretic investigations form a vibrant area of mathematical research today. In connection with these investigations, too, we are dealing with idealized structures, although people like to talk about the "concrete" in this connection in order to emphasize the difference to those considerations that lead further away from the concrete.

f) The task here cannot be to discuss and to evaluate all the different foundational programs that play a role today. Some of them, in particular Brouwer's intuitionism, tend towards replacing ordinary mathematics by a more restricted methodology, one that, compared to analysis, amounts to a stricter arithmetization. The fruitfulness of such investigations consists

[2]With regard to evaluating methodological stances in terms of their evidence it is important to realize the following: We cannot talk about "evident" or "not evident" *simpliciter*—even if we disregard individual conditions of evidence. There are, after all, both degrees and different kinds of evidence. Thus a gain in terms of being more elementary can be offset by a cost in terms of the degree of evidence; there is no dearth of examples here. It is hardly adequate, then, to declare any one methodological stance to be the stance of mathematical evidence absolutely. Of course, the possibility of justifying the methods of classical analysis (in the sense of establishing consistency) by elementary considerations is still important.

mostly in the fact that in them a number of new, mathematically valuable concepts and methods have been developed. Results obtained along those lines are to be valued even if one does not share the opinion that the usual methods of classical analysis should be replaced by others. It also has to be granted that the classical foundations of the theory of real numbers by Cantor and Dedekind do not constitute a *complete* arithmetization. But it is very doubtful whether a complete arithmetization can do justice to the idea of the continuum. The idea of the continuum is, originally at least, a geometric idea.

The monism of arithmetization in mathematics is an arbitrary thesis. It is by no means clear that mathematical objectivity grows only out of the idea of number. Instead, concepts such as those of a continuous curve and of a surface, as developed especially in topology, can probably not be reduced to the idea of number. This does not mean that we shouldn't try to make the idea of number as fruitful as possible for the study of geometric figures, as is of course already done in analysis.

Based on the conception laid out above, according to which mathematics is the science of idealized structures, we have a viewpoint on the foundations of mathematics that saves us from exaggerated perplexities and from forced constructions, one that will also not be undermined if foundational research comes up with various new, surprising results.

This conception requires, however, that we accept another kind of objectivity besides the objectivity of natural reality. For Gonseth's philosophy this presents no difficulty. In this philosophy it is acknowledged from the beginning that the totality of what is objective divides up into different "horizons," which, at the same time, enter into relations to each other, relations such as those between concrete and idealized structures.

On the other hand, this philosophy provides us with an alternative to the apriorist view of mathematics, a view for which the following paradox presents itself: mathematical facts reveal themselves to us only gradually, in the process of doing research; and concepts appropriate for them are also only found gradually in that process, in such a way that completely new constellations occur again and again.

Gonseth proclaims *ouverture à l'expérience* as a general method; and as a requirement it is not restricted to research into nature, but is equally important in the field of *intellectual experience.*

Chapter 27

Bernays Project: Text No. 28

On a Symposium on the Foundations of Mathematics (1971)

Zum Symposium über die Grundlagen der Mathematik[1]

(*Dialectica* 25, pp. 171–195;
repr. in *Abhandlungen*, pp. 189–213)

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The following remarks on the problems in the foundations of mathematics should serve to situate the Symposium on the Foundations of Mathematics begun in volume 23 of *Dialectica*,[2] which was initiated by Erwin Engeler

[1]Eine Ankündigung des Symposiums brachte Herr Gonseths Editorial zu $_{a_2}$***Dialectica***$_{a_2}$ Band 22 $_{a_2}$**(1968) (siehe S. 91–95, insbesondere**$_{a_2}$ S. 95)

[2]$_{d_2}$***Dialectica* 23, 1,2 , 3/4 (1969); 24, 4 (1970)**$_{d_2 a_2}$**Verzeichnis der Abhandlungen** $_d[_{da}(_a$**nachträgliche Hinzufügung [in den *Abhandlungen*]**$]_d]_{da})_a$**: *Dialectica*, Vol. 23 (1969)**

and me at the suggestion of Mr. Gonseth, who briefly introduced it in the editorial of that volume.

Since the discussions in the twenties, in which the problems in the foundations of mathematics received particularl attention in philosophy, the problems has changed considerably. At that time, the situation was characterized by the opposition of three positions, which—following L. E. J. Brouwer—were called Logicism, Intuitionism, and Formalism. The impression was created that these positions formed a complete disjunction of the possible ones on the question of the foundations of mathematics. In truth these were three particular schools, advocating three different approaches with respect to the foundations of mathematics.

The ideas underlying these approaches have retained their relevance to the contemporary discussion of questions of foundations of mathematics. Many of the methods and results that were achieved by these approaches also have lasting significance, in particular:

1. the realization that it is possible to formalize mathematical theories using logical symbolism; the use of this symbolism is already familiar to the present-day mathematician.

2. the application of formalization in metatheoretical considerations. The Aristotelian theory of the syllogism already contains the beginnings of this; the investigation of possible mathematical proofs goes far beyond this very limited framework.

3. the contrast between the constructive way of doing mathematics, with its emphasis on procedures, as opposed to the classical way, which is more focussed on relations among objects.

issue 1: Abraham Robinson, "From a formalist's point of view" (*vide* [?]). – R. L. Goodstein, "Empiricism in mathematics" (*vide* [?]). – Erwin Engeler/Helmut Röhrl, "On the problem of foundations of category theory" (*vide* [?]). – Paul Finsler, "On the independence of the continuum hypothesis" (*vide* [?]).

issue 2: Haskell B. Curry, "Modified basic functionality in combinatory logic" (*vide* [?]). – Georg Kreisel, "Two notes on the foundations of set-theory" (*vide* [?]).

issue 3–4: F. William Lawvere, "Adjointness in foundations" (*vide* [?]).
***Dialectica*, Vol. 24 (1970)**

issue 4: Marian Boykan Pour-El, "A recursion-theoretic view of axiomatizable theories" (*vide* [?]). – Richard Montague, "Pragmatics and intensional logic" (*vide* [?]). – Eduard Wette, "From infinite to finite" (*vide* [?]).$_{a_2}$

With respect to the problems in the foundations of mathematics, however, none of the original approaches achieved exclusive success. Instead, the development was toward a combination of the different points of view, while some of the positions associated with the different approaches had to be abandoned.

The initial framework for the formalization of mathematics presented in *Principia Mathematica* included unnecessary complications, and it was, on the suggestion of F. P. Ramsey, replaced by the simple theory of types. A different, type-free, framework had already been given, of course not formally at first, in the axiomatic set theory of Zermelo. This was also given a formal presentation. For that it was necessary to give a restricted, precise specification of the notion of a "definite property" used by Zermelo in one of the axioms, as was done simultaneously by A. A. Fraenkel and Thoralf Skolem. The systems presented by W. V. Quine mediated, as it were, between type theory and the systems of axiomatic set theory.

It turned out, however, by the results of Kurt Gödel and Skolem, that all such strictly formal frameworks for mathematics are unable to represent mathematics in its entirety. In this connection, even for number theory treated axiomatically one has that any strictly formalized axiom system possesses "non-standard models," which do not have the intended structure of the series of numbers. In these facts we see that certain concepts do *not* admit a fully adequate formalization, such as the concept of finiteness (of a finite number) and the general notion of a predicate.

With respect to the metatheoretical perspective (as permitted by formalization), furthermore, one such for mathematics was intended by Hilbert ("metamathematics") as an method of demonstrating the consistency of classical mathematics by elementary combinatorial methods, thereby answering the various criticisms, initially mainly from Kronecker, toward the methods of classical mathematics. In this connection it turned out, again on the basis on Gödel's results, that the goals needed to be weakened considerably, in that one could not limit oneself for the consistency proofs to elementary combinatorial methods, but rather required the use of stronger methods of constructive mathematics. Just how much stronger they must be is still not fully clear.

This difficulty, however, only concerns one particular kind of metatheoretical investigation. Such investigations are, however, not only useful for consistency proofs, but are also important for treating questions of decidability and completeness, as Hilbert also intended. Thus a metatheoretical investigation of mathematical theories does not necessarily involve a reduction of the usual methods of proof.

The first use of conventional (classical) methods in metamathematics was in the Gödel completeness theorem, which gives the completeness of the rules of the usual (first-order) predicate logic, and which led to numerous mathematically fruitful discoveries.

A large field of metamathematical research without any methodological restrictions was opened up by model theory, cultivated in A. Tarski's school, which has been extended to a theory of relational systems. This research makes strong use of set-theoretic concepts like the transfinite ordinal and cardinal numbers, not only for the methods of proof, but also in determining the objects studied. One considers for instance formal languages with names for infinitely many individuals, and indeed, for totalities of individuals of arbitrarily high transfinite cardinality. Infinitely long formulas are also considered.

This sort of set-theoretical metamathematics shows, among other things, that the claim of Brouwer's intuitionism to be the only correct way of doing mathematics has generally not been accepted. Intuitionistic mathematics is usually regarded instead as a possible methodological alternative, alongside of the usual, classical mathematics. As such it has been investigated metamathematically, after A. Heyting satisfied the need for a more precise description of intuitionistic methods by giving a formal system of intuitionistic logic and arithmetic.

In connection with this formalization given by Heyting it also become clear that the methodological standpoint of intuitionism did not coincide with the finitist standpoint intended by Hilbert for the purposes of proof theory, but actually went beyond it, contrary to what had been thought. This was in particular evident in that it proved possible to establish the consistency, using intuitionistically acceptable methods, of various formal systems that had been shown to be out of reach of finite methods. This difference in the scope of methods rests on the fact that the evidences used by intuitionism are not only elementary and intuitive ones, but also include abstract conceptualization. Brouwer uses for instance the general notion of a proof; these are not, however, proofs according to fixed rules of deduction, but meaningful proofs, and thus not something delimited intuitively. This general notion of proof is then used in particular to interpret the Heyting formalism, with which (by reinterpreting some of the usual logical operations) a very simple consistency proof can be given for the formalism of (classical) number theory. Of course, in using this concept of proof, one not only goes beyond the finitist standpoint, but also beyond conventional mathematics, which surely uses the notion heuristically, but not in any systematic way. One can, of course, replace applications of the general notion of proof in establishing consistency by other concepts of intuitionistic mathematics. Such concepts include, on the one hand, Brouwer's notion of a choice sequence, i. e., an unending sequence of successive choices of values, and on the other, as Gödel pointed out, the concept of a functional, i. e., a function taking functions as arguments.

The constructive use of transfinite ordinal numbers, for example, can be justified with the help of the concept of a choice sequence, since for certain ranges of such ordinal numbers, which can be described by elementary means, it can be shown that any decreasing sequence stops after finitely many steps. — In applying the notion of a "functional" one ascends to higher levels ("types") of functionals: functionals can in turn be arguments to functionals of a higher type.

Thus there are various ways of extending the finitist standpoint for the purposes of proof theory.

One direction in research into mathematical foundations has not yet been mentioned. It was only remarked in passing that the system of *Principia Mathematica* included unnecessary complications, which were then overcome by the system of simple types. The "lack of necessity" resulted, however, only from repudiating one of the aims of the formulation of the system, expressed in the form of the "ramified types," but not enforced, and indeed in effect rendered superfluous by the later addition of the "axiom of reducibility."

This aim goes back to a critique of the method of founding analysis (by Dedekind, Cantor, Weierstraß), as expressed by some French mathematicians. This critique, while not going as far as that of Kronecker and later Brouwer, has in common with those sorts of views that it aims for a stricter arithmetization of the continuum. It is objected that in existence proofs in analysis, such definitions ("impredicative") are often used as make reference to the totality of all real numbers, say, for instance when a decision is made to depend on whether or not there is a real number with a certain property. According to the arithmetization, however, the totality of real numbers is supposed to arise from the possible arithmetic definitions.

A more precise formulation of the associated requirement for predicativity was indeed first given by Bertrand Russell, although, as mentioned, he did not consistently maintain it. Hermann Weyl returned to it later in his work *The Continuum*.[a] Since then various ways have been attempted to give a predicative formulation of analysis and set theory, in particular by Leon Chwistek, Frederic B. Fitch, Paul Lorenzen, and Hao Wang.[b]

With respect to the requirement of predicativity there is little unanimity among mathematicians and researchers in foundations. Here, too, one can assume a compromise position, similar to that assumed with respect to the requirements of intuitionism in what has already been said. One can respect the possibility of a predicative treatment of analysis, i. e., the theory of real numbers and continuous functions of one or more variables, while at the same time recognizing that mathematics goes beyond these methods, and that for research in some areas it is appropriate to use the concepts of general set theory. In particular, only by using such concepts does the idea of the continuum receive an adequate theoretical treatment.

A new aspect of the question of mathematical foundations has recently appeared in the results concerning Cantor's problem of the continuum. This is the question whether the cardinal number of the set of subsets of the sequence of numbers, which is also the cardinal number of the continuum, is the next larger one after the cardinal number of the set of natural numbers. The assumption that this is so is called the continuum hypothesis.

After Gödel had proved that the continuum hypothesis is consistent in the framework of axiomatic set theory (assuming that the latter is itself consistent), Paul Cohen has recently shown that axiomatic set theory leaves the cardinal number of the continuum fully undetermined, except for certain known restrictions, within the range of uncountable cardinal numbers. This of course holds for the restricted formulation of axiomatic set theory already mentioned, which makes possible its strict formalization. However, in the case of the continuum problem it is not apparent, at least up to now, how we can, by giving up the more precise axiomatic formulation, gain the possibility of a decision about the power of the continuum.

The situation encountered here is similar to the one resulting from the imperfection of formalized axiom systems already mentioned, as expressed

[a] *Vide* [**?**].
[b] *Vide* ■■■ .

by the existence of non-standard models.

The description just given of the current situation in research in mathematical foundations is certainly not complete in all aspects. It may serve nonetheless as an introduction to the remarks that now follow, referring to the various contributions to the symposium, and which are intended in part as elucidation, and in part concern the philosophical discussion. Some further elaborations on the forgoing remarks will also be included.

The series of contributions to the symposium is opened by the one from Abraham Robinson "From a formalist's point of view,"[c] in which a position is presented that the author does not definitely hold, but at least states for discussion. The position holds that there are *no* infinite totalities and that it is strictly speaking nonsense to refer to them, but that, on the other hand, this should not hinder us from doing mathematics in the classical way, making free use of the various concepts of the infinite.

This view is obviously problematic in and of itself; it has more the character of a problem to be solved than an explanation of something. In searching for a clarification we should first recognize that the claim that there are no infinite totalities surely only applies to natural reality, and that accepting it in no way implies that the idea of an infinite totality — say that of the lattice points in the plane equipped with coordinate axes and origin — is nonsensical.

We can admit that we have no real visual representations of infinite totalities; but neither do we have any of totalities with very large numbers, although our experience tells us that such do exist. Full visual representability is thus not so critical for our claims and assumptions about existence, even in the natural world. On the other hand, there is a kind of representative imagination, in which conceptualization and intuition are combined, and the scope of which is difficult to determine. The claim that there is a strict separation of concepts and perceptions in our mental lives — as held by Kantian philosophy in particular — certainly needs revision.[d]

Representative imagination makes it in particular possible to go beyond the finite. The mathematician who considers the infinite is surely not thinking only in words. In analysis, in particular, mathematical procedures have a kind of intuitiveness and certainty that cannot be attained by mere verbal

[c] *Vide* [**?**].

[d] The term *Vorstellung* is not translated uniformly in this passage.

operations.

If we want to do justice to our mental experiences, we must not be satisfied with an oversimplified scheme of what is conceivable. Nor will it suffice to recognize only one kind of objectivity. The sensory qualities, for instance, are of course merely subjective from the standpoint of physics, while a color as such is something objective, and the relations among colors are objective states of affairs (to which it is not initially determined how or to what extent there must correspond physical or physiological states of affairs). Works of literature and music are also objective entities, whereby this objectivity does not coincide with that of a particular presentation, which may fail to do justice to the work.

In light of all this, there is no obstacle in principle to recognizing the objectivity *sui generis* of mathematical objects. The objectivity is that of idealized structures, whereby the idealization consists in mediating between concepts and intuition. That such idealized structures should have their own laws is certainly something quite remarkable, but hardly more remarkable than the fact that mathematics finds such immediate application in the natural sciences.

What probably bothers many philosophers and mathematicians about admitting a special objectivity for mathematical objects, about this kind of "platonism," is mainly that the objectivity is taken to be to too great an extent analogous to natural reality.

That mathematical objects and states of affairs have a fundamentally different character than those of the natural sciences is emphasized and elucidated in the article by R. L. Goodstein "Empiricism in mathematics."[e]

On the basis of the difference between the objects of mathematics and those of the natural world, Goodstein explains mathematics as a mere game, pointing in particular to the similarities with chess. This clearly Wittgensteinian characterization is hardly adequate. The similarities of chess, and board games in general, with mathematics rest, not on the fact that these are all games, but on the fact that the games have geometric and arithmetical properties, resulting from the board configurations and rules for moving the pieces. Statements and conjectures of a thoroughly mathematical kind are often made concerning games, particularly chess. To be sure, one can do mathematics in a playful way, but that is not particular to mathematics;

[e] *Vide* [**?**].

one can conduct other sciences playfully. (Aspects of play occur in many intellectual activities in which people have a certain freedom.) The rules of doing mathematics are not determined with an eye to having fun, but for the characterization and study of distinguished (usually idealized) structures. The statement of these rules is itself a part of mathematics.

The contributions by Paul Finsler and Georg Kreisel are concerned specifically with questions of set theory.

In his study "On the independence of the continuum hypothesis"[f] Finsler emphasizes that the independence of the continuum hypothesis from the framework of axiomatic set theory, established by Paul Cohen and meanwhile also by Dana Scott using other methods, depends on the axiomatic restriction of the notion of a subset, and thus does not apply to the continuum (respectively, the set of all subsets of the number series) in the *original* sense. In this sense, Finsler speaks of a *formal continuum*, which does not posess all of the properties of the real continuum. He gives the example of a *hypercontinuum* that he arrives at from Cantor's first and second number classes, and which has many properties of the continuum, but can fairly easily be shown not to have the next cardinality after the countable, in contradiction to the continuum hypothesis. This example is supposed to show that the possibility of defining a *formal continuum* that doesn't satisfy the continuum hypothesis is nothing surprising.

The example is of course not very convincing, since the constructed hypercontinuum exhibits quite clear deviations from the continuum of analysis, whereas the continuum of axiomatic set theory can be shown to have all the usual properties (independently of set theoretic axiomatics).

In his "Two notes on the foundations of set$_{d\text{-}da_a}$theory"[g] Kreisel provides detailed arguments against those who either would reject set theory entirely or would advocate restricting the rules of set formation, particularly in response to the difficulties regarding the continuum hypothesis. His openmindedness is not connected with a lack of appreciation for the constructive point of view, as is the case with some set theorists. Indeed, Kreisel himself has been deeply involved with research into finitist and intuitionistic mathematics and its exact characterization. This makes his thoughts all the more relevant for those who consider awareness of constructive methods important.

[f] *Vide* [**?**].
[g] *Vide* [**?**].

The brief introductory summary of the beginnings of set theory provides an opportunity for further discussion. Kreisel believes that Cantor's contemporaries found the concept of set to be a "mixture of notions," as a result of its very different kinds of applications (to concrete objects, to numbers, to geometric points). According to this view, the desire to arrive at a more precise general concept of set resulted after several unsatisfactory attempts in the discovery of the *cumulative type structure*, with which the decisive clarification was achieved.

To be sure, Kreisel himself remarks in a footnote that he does not feel competent to judge how things were seen at the time. Some further historical remarks may not be out of place here, of course, with equally imperfect knowledge of the full development.

The discovery of the cumulative type structure — today one also speaks of *natural models of the axioms of set theory* in this connection — was closely related to the formulation of the Axiom of Foundation, which does indeed enforce a sort of type structure on the system of [all] sets. The idea of the Axiom of Foundation was probably first due to Dmitri Mirimanoff,[3] who in connection with it found the "independent"[4] theory of ordinal numbers (at about the same time as Zermelo and von Neuman.[h]

Mirimanoff was led to his observations by consideration of the set theoretic paradoxes. These were also responsible for Zermelo's formulation of his axiom system for set theory and Russell's formulation of the theory of types.

The idea of restricting to those sets that can be built from an initial set (say, the natural numbers) by forming power sets, unions, and separation, was at one point considered — as was related to me by Hilbert. It led directly into paradox, however, since the process of forming unions was not sufficiently regulated, and instead the collection into a set of all the sets arrived at by the processes mentioned was itself regarded as a legitimate union.

There is really no reason why anyone should object to the different applications of the concept of set. The elementary concept of cardinality is not

[3]The condition expressed by this axiom is of course already implicitly contained in the intuitive notion of set.

[4]so-called because the ordinal numbers do not need to be introduced as abstraction classes of similar well-ordered sets.

[h] *Vide* ■ ■ ■ .

considered a mixture of concepts, even though it can be applied at once to both concrete things and also to arithmetical and geometric objects.

For theoretical treatment of number theory one has of course isolated a structure that can be studied purely purely for itself, prior to its application for determinations of cardinality, to which it is applied. It should be noted, however, that the axiomatic formulation of number theory by Dedekind and Peano, which provided the characterization of the structure of the number sequence, was only achieved when number theory was in an advanced stage of development.

Set theory, for its part, had already been developed by Cantor to a considerable degree when it was axiomatized by Zermelo. And even in that axiomatization, the cumulative type structure mentioned by Kreisel was not yet worked out. Almost a decade passed until the formulation of the Axiom of Foundation, and it was in that period that Hausdorff's classic work *Grundzüge der Mengenlehre*[i] appeared. It was path-breaking for the more recent development of set theory and set theoretic topology. In it, however, set theory is developed without using axiomatics. Hausdorff himself explained that in his treatment of set theory he wanted to "permit the naive concept of set, while retaining the restrictions" that block the way to the set theoretic paradoxes.

It is noteworthy that this approach is still taken today in methodologically unrestricted metamathematics, i. e., in model theory, and was also used by Zermelo in his model-theoretic investigation "Über Grenzzahlen und Mengenbereiche."[j]

It is understandable that set theory needs to be treated intuitively in this way, since it does not merely represent a certain structure, but should provide our way of thinking about structures in general.

Remark on the "Discussion" (pp. 101–102):[k]

In light of the examples of restricted methods mentioned here by Kreisel, some readers might think the proposed concentration on a "relatively restricted system" also refers to one of the restrictions Kreisel mentioned. In fact, what is intended instead is a contrast with certain approaches using enormous infinities, going well beyond those usually encountered in math-

[i] *Vide* [**?**].
[j] *Vide* [**?**].
[k] ■ Zitat/Inhaltsangabe geben?■

ematical theories. The restricted theories mentioned by Kreisel are surely presented only for the purpose of a methodological comparison.

Two of the essays in the symposium deal with the theory of "categories:" that by Erwin Engeler and Helmut Röhrl "On the problem of foundations of category theory" and that by F. William Lawvere "Adjointness in foundations."[1]

Lawvere is one of those responsible for working out the theory of "categories," invented by Eilenberg and Mac Lane. This is a general theory of mathematical mappings. Its basic concept is the relation $A \xrightarrow{f} B$ meaning "f maps A to B." Here A is called the "domain," and B the "codomain," of f.[5]

The basic operation is the composition of mappings $fg = h$, which is always possible when the codomain of f agrees with the domain of g. The domain and codomain are to be thought of as structured sets, although the elementhood relation is not taken as a basic relation. One refers simply to "objects" between which there is a mapping.

The concept of a *category* is tied to these concepts, which concern the mappings. A category is thereby understood to be a kind of structure, as is determined by the conditions of an abstract axiom system (i. e., the structures of such a kind are the models of a system of axioms). The mappings from one object of a category to another such object are called "morphisms." The intention is to characterize structures (categories) through the morphisms occurring in them.

Not all mappings are morphisms in a category, however. There are also mappings that lead from one category to another. This is accommodated by considering "functors," in addition to morphisms. A functor is regarded as a mapping from one category into another category. The categories thereby play the role of objects; the are regarded as the objects of a "category of categories."

This leads to problems, however; one is led into the same kind of concept formation that gave rise to the set theoretic paradoxes. Nor is that surprising. A category, after all, is a totality only in the sense of being the extension

[5]The mapping need not be *onto* B. Thus the codomain B need not coincide with the set of values of f.

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[1] *Vide* [**?**], resp. [**?**]

of a concept, not a mathematical structure. For that reason, it is hardly legitimate to treat categories as "objects."

The essay by Engeler and Röhrl discusses these difficulties and makes a proposal for their solution, which, however, only partly satisfies the authors themselves.

I would like to make a simpler proposal here (if only in outline):

1. Mappings occurring between objects of different categories are treated on a par with morphisms within a category.

2. Functors are replaced by mappings, the domain of which are *variable* within a category, and which (with respect to this variability) take the objects of the given category to ones of some other particular category.

A simple example of such a mapping of a variable object is given by the transition from an arbitrary Boolean algebra to a ring according to the method of M. H. Stone.

The intensive study of mappings and the various ways of composing them, as is done in the theory of categories, has proved to be extremely fruitful and successful in various fields of mathematical research. In recognizing this success, however, we need not subscribe to the tendency, often associated with presentations of the theory, to eliminate the usual notion of elementhood from mathematics. This tendency is evident in the practice of intentionally avoiding the explicit mention of elements of objects, as well as of the arguments and values of morphisms, even if only in the form of bound variables.

To be sure, an element relation of a sort is introduced by postulating a special object "1" with the property that for every object A there is one and only one mapping $A \to 1$; the elements of an object C are then taken to be the mappings from 1 into C. Accordingly, the value of a function (morphism) f with the domain C and codomain D for an element a of C (as argument) is given as the composite of f with a, which is indeed a mapping from 1 to D, and thus an element of D. In this respect, the representation of elements as mappings seems to be satisfactory. In other respects, however, it turns out to be inadequate; since an element a of an object C is taken to be a mapping into C, the object C is uniquely determined by the element a as its codomain. Two different objects can therefore never have an element in common. Thus the common boolean algebra operations cannot even arise here.

The motivation for avoiding the usual elementhood relation is sometimes said to be that mathematics has nothing to do with substance, but only with structure. As a statement about mathematics, this is surely correct. However, the relation of an element belonging to an objective whole, as, e. g., a point belongs to a point-set, is indeed a structural relation.

To be sure, there can be no fundamental objection to developing a theory of functionality in which the elements of the *objects* represented occur only implicitly. The general method that is being applied in such a case consists in taking the objects occurring initially at second order (sets, functions) to be the immediate objects of the theory, as is already done in boolean algebra.

A theory of this sort will hardly make the usual set theoretic point of view in mathematics superfluous, however.

A theory of functionality of a very different kind than that of categories is the object of Haskell B. Curry's contribution "Modified basic functionality in combinatory logic."[m] The type-free combinatory logic that he formulates is closely related to the λ-calculus, developed by Alonzo Church, and provides a formalism for representing constructive functionality by means of combinations of certain *atomic combinators*. Since the range of values of the variables to which the combinators are applied is completely unrestricted initially, the expressions built up out of combinators do not necessarily always represent a meaningful function. In general, one is led to the further question, whether a given combinatorial expression is of functional character.

The functionality properties of combinatorial expressions are studied in depth in the textbook on combinatorial logic by Curry and Feys. The present article adds to the discussion in the book certain simplifying modifications and additions regarding the new applications of functionals in research in foundations of mathematics. In particular Curry proves here a theorem to the effect that, if for certain "atomic" combinators the functional character is given, then for every combinatorial expression built from these, it can be decided whether it has a functional character, and if so, this character can be determined.

Curry has laid out his general views about mathematics in various other places: in his monograph *Outlines of A Formalist Philosophy of Mathematics*, in his Notre Dame Mathematical Lectures, and in the already mentioned textbook *Combinatory Logic*.[n]

[m] *Vide* [**?**].
[n] *Vide* [**?**], [**?**].

Curry describes mathematics as the science of formal systems. He thereby understands a formal system to be formalized theory in which it is determined by stipulations how the sentences are built from predicates and terms, and the terms from primitive terms, by means of operations, and moreover, which sentences count as the "elementary theorems." Just as the terms are recursively generated from the primitive terms by applications of operations, so the elementary theorems are recursively generated from "axioms" by applications of rules of deduction, whereby the axioms are certain sentences that are simply postulated as valid. Curry regards the determination of the elementary theorems as part of the formal systems, in contrast to further considerations about the system, the "$\epsilon\pi\iota$-theorems."

Taken together, these definitions do not differ essentially from those of Hilbert's proof theory. While proof theory is thought to be a further enterprise, beyond existing mathematics, however, Curry's position takes formal systems themselves to be the actual topic of mathematics. The view is thereby not that one could arrive at a single formal system for the whole of mathematics. Rather, Curry emphasizes that what is essential to mathematics is not to be found in the particular kinds of formal systems, but in the formal structure as such.

One can also agree in general with this formulation from a non-formalist standpoint, as long as one takes formal structure to mean, not only the structure of formal systems of the kind just described, but also idealized structures in general. It is indeed structures that are the objects of mathematical investigations.

The structures of formal systems are, however, of interest for us only as a means to an end. These systems merely serve to put mathematical theories into a form that is suitable for the application of proof theoretic considerations.[6] Mathematics is most certainly not only present where theories of this kind are have [already] been produced.

From a proof theoretic point of view, to be sure, most of the mathematical theories can be regarded and presented as the development of a particular formal system.

The formal systems all have in common a number theoretic character, which derives from their recursive construction. This is particularly evident

[6]In some situations—as Curry also mentions—proof theoretic considerations seem to require one to give up the framework of formal systems and move to "semi-formal" systems.

in the application of the method of Gödel numbering, by which the formulas of a formal system, as well as the sequences of formulas, are assigned specific natural numbers.[7] This method involves many arbitrary choices, so that various different numberings of one and the same system are possible. Moreover, one and the same mathematical theory will have different formalizations, which can be translated into each other by sometimes simple, sometimes complicated transitions.

One can ask in this connection, given any two formal systems, to what extent is it possible to determine whether they formally represent the same theory, just on the basis of their Gödel numberings (determined by the same method). An investigation of just this sort was conducted by John Myhill. It proceeds from the characterization of a formal system by the set of Gödel numbers of the provable formulas. Myhill shows that for any two formal systems, both satisfying a certain condition amounting to a minimum of expressiveness, the characteristic sets of numbers can be mapped isomorphically onto each other by means of an effective algorithm which induces a permutation of the number sequence.

If one then regards any two formal systems satisfying the appropriate condition as equivalent, then something paradoxical results: Among the systems satisfying the condition are, on the one hand, very elementary number-theoretic ones for which consistency can be shown by finitist means, and, on the other hand, also much stronger formal systems of analysis and set theory, assuming these are consistent.

This result is surely to be interpreted to say that the existence of an effective isomorphism between the sets of Gödel numbers of provable formulas of two formal systems cannot be considered a sufficient condition for their equivalence. One is led to ask what further restriction on the isomorphism would lead to a suitable criterion of equivalence of formal systems.

[7]It may be remarked by the way that the more general method of Gödel numbering, as applied in *Foundations of Mathematics* (*vide* [**?**], pp. ■), corresponds to Curry's more general kind of formal systems; while the more usual, special method which makes use of the order of symbols in a formula fits the kind of formal systems that Curry calls "logistic systems."

This question is taken up by Marian Boykan Pour-El in her symposium contribution "A recursion-theoretic view of axiomatizable theories,"[o] in which she reports on recent research on the relation between "recursively enumerable" sets (i. e., those generated by combinations of elementary processes) and formalized theories. In a joint paper with Saul Kripke, the attempt was made to characterize those effective mappings between formal systems that preserve the proof structure. One condition for such preservation that they consider is that the mapping preserves not only provability, but also implication and negation. It turns out that this requirement is, however, not yet sufficient to provide a suitable characterization of equivalence by means of such mappings. Some difficulties therefore still remain in this connection, about which Pour-El formulates a number of more precise questions.[p]

The contribution of Richard Montague leads us into the field of semantics. In research in the foundations of mathematics, and in the school of logical empiricism, semantics is understood to be the investigation of interpretations of formal languages, such as are used for the formalization of axiomatic deductive systems. Such interpretations make use of set theoretic concept formation.

In the case of a formal language of first order with only one sort of variables and constants for individuals and without symbols for mathematical functions, the interpretation proceeds by taking as basic a domain (a set) of individuals, and assigning to each individual constant an element of the domain of individuals, and to each predicate symbol with k many arguments, a set of ordered k-tuples of elements of the domain of individuals. A k-place predicate symbol is then interpreted as a k-place predicate which applies to a k-tuple of elements of the domain of individuals if and only if that k-tuple belongs to the set assigned to the predicate symbol.

On this basis the notions of satisfaction and satisfiability of a formula, with the usual meanings of the logical symbols &, $\vee$, $\neg$, $\bigwedge x$, $\bigvee x$ (and, or, not, for all x, there is an x), can now be defined recursively:

A prime formula, i. e., a formula without logical symbols, which therefore consists of a predicate symbol with k arguments, themselves either variables or constants, is said to be *satisfied* by a substitution of each of the free variables (occurring as arguments) by [<u>names for</u>] elements of the domain of

[o] *Vide* [**?**].

[p] The title "*Frau*" has been omitted.

individuals, in such a way that, taken together with the elements assigned to the individual constants (occurring as arguments), the result is a k-tuple that is in the set assigned to the predicate symbol. One also says that a prime formula in which all k arguments are constants is satisfied if the elements assigned to the constants determine a k-tuple that is in the set assigned to the predicate symbol.

The conjunction A & B is satisfied when A is satisfied and B is satisfied, whereby the individual variables occurring in both A and B receive the same substitution.

The negation $\neg A$ of a formula A is satisfied by a substitution for the free variables, resp. (if there are none) without any substitution, if and only if the formula A is not satisfied by the substitution, resp. without any substitution.

The formula $\bigvee xA(x)$ (there is an x such that $A(x)$) is satisfied when the formula $A(b)$ is, whereby b is any free individual variable not occurring in $A(x)$.

With this, satisfaction is already defined for arbitrary formulas, since the logical connectives $\vee, \rightarrow, \leftrightarrow$, as well as universality, can be expressed in terms of conjunction, negation, and the existence operator $\bigvee$.

A formula is called *satisfiable* if it can be satisfied.

For a formula without free variables, given an interpretation of the formal language, there are only two possibilities: that it is (simply) satisfied or that its negation is satisfied. Accordingly, it is said to be "true" (correct) or "false" (incorrect).

All these notions are relative to the interpretation used, i. e., the basic domain of individuals and the chosen assignments.

Semantics also makes use of a wider notion of satisfiability, according to which the predicate symbols are also treated as variables. The interpretation itself is then regarded as variable, and one defines a formula to be satisfiable if it is satisfiable, in the previous sense, for a suitable interpretation.

Numerous applications can now be made of these concepts. First of all it can be shown that applications of the rules of the logical calculus to correct formulas always results in formulas that are again true, and that is the case for any interpretation. In particular it follows from this that every provable formula of the logical calculus (of first order) is true under every possible interpretation. The converse statement, that in the framework of first-order logic every formula with this property is provable in the logical calculus, is asserted by the Gödel completeness theorem.

One can furthermore study the axiom systems formalized in the first-order framework. The interpretations of such a system under which all the axioms (resp. the formulas built according the axiom schemata, if used) are true are called the "models." The valid sentences of the formalized theory are those represented by formulas that are true in every model of the axiom system. That every formula representing a valid sentence can be derived by the logical calculus again follows from the completeness theorem. This says, in other words, that any sentence which can be represented by a formula of the formal theory and cannot be derived from the axioms can be refuted by a model which does not make the corresponding formula true.

These facts can be formulated precisely only using the semantic concepts just introduced. Although these concepts are concerned with first-order logic, their definitions and applications go beyond it; in particular the general notion of satisfaction of a formula is a concept of second-order logic.

The semantic investigations under discussion are all concerned with extensional logic, which is of course sufficient for conducting mathematical proofs. In his essay "Pragmatics and intensional logic"[q] Richard Montague attempts to show that the methods of semantics can be applied to formal languages that go beyond extensional logic, and in particular that the concepts of satisfaction of a formula and that of truth can be defined for such formal languages.

Montague considers various formal languages, some of which he calls pragmatic, and some intensional.

The pragmatic languages are characterized by the occurrence of means of expression that require knowledge of *points of reference* (in the terminology of Dana Scott) for their interpretation.

Such means of expression are, in particular, temporal references, e. g., the word "now," and verb tenses. Intensional languages are understood to be those in which occur expressions for modalities or attitudes of persons to propositions, such as "B believes that ... ."

The method of interpretation is one and the same for pragmatic and intensional languages: a parameter set is fixed, the value of the parameter determines a point of reference or a possible world (according to the sort of language in question) and the interpretation then proceeds with a dependence

[q] *Vide* [**?**].

on the value of the parameter. For this interpretation and each value of the parameter, a set of individuals is assigned.

The execution of this approach to the definition of semantic concepts is given in detail for the formal languages considered.

It seems that the scientific application of this method is problematic. Pragmatic languages are indeed used in science, but mainly for empirical research, to which a treatment in the form of a formal axiomatic theory is generally not well suited. In particular, it seems very contrived to assign each point in time a domain of individuals. Even more questionable is the assignment relative to possible worlds; we don't even know in the real world whether one can speak of the totality of individuals in certain ways. Thus the methods of extended semantics described by Montague will likely be applicable only in situations where a very schematic treatment suffices.

Unfortunately, these questions can not be discussed with the author himself, since he is no longer living. He did, however, name in his article various colleagues with whom he has discussed such questions.

In his essay "From infinite to finite"[r] Eduard Wette takes an extreme position. He there claims that it is possible to demonstrate the inconsistency of formalisms for classical mathematics and even for number theory. He has pursued this idea, and reported on his research in various papers and lectures.

The contradictions he arrives at are of course not of the same sort as the familiar set theoretic paradoxes, which can be presented relatively easily, and indeed can even given a popular formulation in some cases. The proofs involved in Wette's work are extremely complicated, and are only described by him, but not actually given. This description provides too little to go on for an accurate verification.[1*] Moreover, although Wette's deliberations make a strong impression of intense intellectual effort, facility with foundational techniques, and attention to detail, the possibility of an error cannot be excluded in such extensive investigations. In this connection it seems suspicious that the contradiction does not rest with axiomatic set theory, but

[1*] Such a verification could more plausibly begin with the recently published treatise "Contradiction within pure number theory because of a system-internal 'consistency'-deduction" [**?**].

[r] *Vide* [**?**].

that Wette pursues his arguments to the conclusion that analysis, and even arithmetic, as already mentioned, are inconsistent.

With analysis and number theory we come to fields in which we have acquired sufficient trust through our intellectual experience.

Of course, a case can be made that only a small part of the requirements implicit in the formalization of analysis are applied in the actual proofs, and that more narrow formal restrictions can be made which suffice to conduct all the proofs in the theory as it actually stands. Thus the trust we have acquired through intellectual experience does not really apply to the entire formalization of analysis. Proposals for such more narrow restrictions have been made by Paul Lorenzen and Georg Kreisel. According to Wette's claims, however, even such restrictions would not suffice to eliminate the possibility of contradictions.

That these extreme consequences of his investigations do not make Wette himself suspicious of them is to be explained by the fact that he sees the results as confirming his philosophical views, on which he bases his opposition to the usual methods in mathematics, and in particular to indirect proofs and the use of the infinite.

The opposition to indirect proof, which Wette pursues in the name of strict finitism, goes beyond Hilbert's finitist standpoint and well beyond Brouwer's intuitionism. According to the finitist standpoint, as well as to Brouwer, indirect proofs are only to be prohibited when establishing positive (existence) claims, not when used as a method for proving impossibility, and certainly not in refuting assumptions. Wette considers even such applications as these of indirect proof to be "problematic," as he makes clear in criticizing the Gödel incompleteness theorem at the very beginning of his paper "From infinite to finite." At some points it sounds as though he believes his results are in opposition with that theorem.[8]

In fact, Gödel's proof of the incompleteness theorem is not indirect.[9] A procedure is specified, for any formal system F satisfying certain preconditions, which derives a contradiction from a given proof in F itself of the consistency of F. The preconditions on F that must be satisfied concern its expressiveness, deductive strength, and strictly formal character. The requirement that the system F is consistent, on the other hand, is initially not

[8] Cf. the remarks in *Dialectica* 24, 4 p. 315, l. 14–16 and p. 316, l. 22–24.

[9] A proof is said to be indirect if an assumption is made, which in the course of the proof is shown to be incorrect.

even used. Only by a subsequent contraposition does one infer that, if the system F is consistent, then no proof of the consistency of F can be given within F itself.

Even without the contraposition, however, one has the following result: if a formal system F satisfies the mentioned preconditions, then from a proof in F of the consistency of F one can derive a contradiction in F.

It is on this very principle, however, that the proofs rest with which Wette claims to establish the inconsistency of the formal systems of classical mathematics. These proofs can therefore lead to no incompatibility with the incompleteness theorem. This line of reasoning also hardly seems likely to lead to a rejection of indirect methods of proof.

Turning to Wette's critique of using the infinite, his arguments here make use of the formalizability of mathematical theories and the finiteness of symbolic formulas. In a recent lecture "On new paradoxes in formalized mathematics" (Madison, Wisconsin, 1970) he says "Is it not a magnificent joke of history that our symbolism tries bona fide to express each theorem on mathematical infinities and logical generalizations in the form of a string which is finite as well as particular?"

To this question, one can respond that the single sentence which formalizes the statement of a theorem does not achieve this feat in isolation, but rather in the framework of a formal system, i. e., in connection with the basic formulas of the system and its rules of deduction. In this way, every formula of the system corresponds to its "consequence set," i. e., the unlimited set of formulas that can be derived from it. Here we see the number-theoretic structure of formal systems entering in, as is made plain by the application of the method of Gödel numbering.

The basic question may be asked: why in mathematics, or at least arithmetic, do we not restrict ourselves to a finite framework? The answer to this is that, in an essential respect, the infinite is more simple than the numerous finite. A circle is much easier to characterize than an inscribed many-sided polygon which approximates it. Number theory in a restricted number field involves various complications. Moreover, how would the restriction of the numbers be determined? If we left it arbitrary, then we would again already be using a general numerical variable as a parameter for the possible restrictions.

If on the other hand we take a particular restriction, then a certain arbitrariness is associated with that particular choice. This approach is problematic from the point of view of applications as well; to be sure, in physics there are size restrictions, but no sharp boundary. And do we even know that our current theoretical physics includes all possible applications of mathematics? Science is far from having reached its end! As an aside, finite geometries (geometries with only finitely many points) have been constructed which can be approximated by euclidean geometry.[10,s]

A question in the other direction, so to speak, is whether in formalizing analysis and set theory, the number-theoretic structure of formal systems is in tension with the uncountable sets occurring in these theories, and whether this tension might be at fault in the difficulties that have turned up in connection with the existence of non-standard models of formal theories and the formal undecidability of the continuum hypothesis, as were already mentioned.

In recent foundational research, one tries to counter this tension by allowing, on the one hand, infinite expressions (in analogy with infinite sums and products in analysis) and, on the other, uncountably many constants. It seems doubtful, however, that this approach will lead to a solution of the difficulties. These difficulties may well be an expression of the fact that the potential means of concept formation in mathematics cannot be exhausted by a formal system—just as was already remarked that axiomatic set theory cannot completely replace the intuitive use of set theoretic ideas as a way for us to think about structures.

[10]Such geometries have been considered by G. Järnefelt, P. Kustaanheimo, and B. Qvist.

[s] *Vide* [redacted]).

Chapter 28

Bernays Project: Text No. 29

Preface
(1974)

Vorwort

(*Abhandlungen*, pp. vii–x)

Translation by: *Wilfried Sieg*
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The *Wissenschaftliche Buchgesellschaft* has kindly offered to publish a collection of my essays on the philosophy of mathematics, which have appeared in various journals. I accept this offer gladly, in particular since several of these essays are not easily accessible.

The present volume can also serve as a temporary substitute for a comprehensive treatment of the philosophy of mathematics.[1] This is possible because, during the period in which these articles were published, my views on the relevant questions have changed almost exclusively in response to new insights gained from research in the foundations of mathematics.

[1]A monograph on this topic, to be published by Duncker & Humblot, has long been planned (but is still not done).

The collection of these various essays should provide the reader with an adequate characterization of my views on mathematics.

Especially with regard to what has been called the "foundational crisis" of mathematics it will become clear, I hope, that according to my view we cannot justifiably speak of a crisis, at least not in the sense that classical mathematics has been shown to be questionable. Problematic aspects have, of course, presented themselves in various respects.

First of all, we have become conscious of the fact that the idea of the obviousness of mathematics is not justified unless we consider as obvious simply what has become familiar to us through use and practice. Even ideas that are not really trivial can become familiar to us in this sense, and in their use we can acquire practical certainty. The very idea of an absolute certainty is presumably illusory for human reason in any case.

Going beyond the trivial is involved especially in all those idealizations which are characteristic for mathematical concept formation. It has become clear that even the general concept of natural number and the related notion of the number series are based on an idealization.

Already here, we also meet with an opposition that calls for a restriction of methods of proof. For instance, the restricted methodology of Brouwer's "intuitionism" as well as that of the "finitist" standpoint—as Hilbert called it—avoid the inference according to which a numerical predicate either applies to all numbers or else there exists at least one number to which it does not apply. This kind of application of the *tertium non datur* is avoided here, all the more, for predicates of sets and of functions. However, even if only such a restricted methodology is accepted, the recursive generation of functions leads beyond what is concretely, computationally feasible.

Avoidance of the above-mentioned application of the *tertium non datur* has, incidentally, no essential impact on elementary number theory. For analysis, however, it amounts to a considerable restriction.

As far as classical analysis is concerned, it was initially believed that its foundation according to the methods of Dedekind and Cantor provided a complete arithmetization. However, viewed from the standpoint of the requirement of a strict arithmetization, classical analysis was soon criticized. And this critique grew under the influence of the demands of finitist and intuitionistic methodology. Various programs for a more strictly arithmetical treatment of analysis have since been developed within research in the foundations of mathematics.

It would be unjust not to recognize that these various kinds of a more strongly arithmetized analysis are of definite mathematical interest. Yet, it should also be admitted that it is a prejudice to believe that it is absolutely necessary to arithmetize analysis completely. In analysis, geometrical ideas are made conceptually precise. The methods of Dedekind and Cantor, referred to above, succeed in connecting analysis to number theory, but not without the addition of *set-theoretical* concepts. If one understands clearly that these concepts are not completely arithmetical, then the procedure involves nothing objectionable.

To be sure, the methods of classical analysis contain strong idealizations. But these do not detract from practical certainty. Indeed, a kind of intuitiveness is gained here that confers great certainty on our considerations.—

Problems have also arisen in connection with the discovery of the formalizability of mathematical proofs by means of symbolic logic. This formalization can certainly be viewed as a sharpening of the axiomatic method. Indeed, formal systems have been successfully set up for number theory, analysis, and set theory. These formal systems consist of a symbolism and rules of deduction, and are set up in such a way that, within the framework of such a system, the known proofs of the respective theory can be formally represented.

Formal-deductive systems can also be set up independently of already existing theories, and then we have the reverse situation, namely that we can try to find contentual interpretations (models) for them. That is a topic for "semantics" which is generally concerned with the relations between theories and formal systems.

A different kind of research tied to the formalization of mathematical theories takes formalized proofs as the object of mathematical investigation. This is the aim of Hilbert's proof theory. It is above all a matter of investigating the internal consistency of formalized theories. For this purpose there arises the possibility of a methodological reduction in the case of the theories of the infinite, because formalized proofs are, after all, finite objects and because consistency can be formally characterized. Consistency proofs of this kind have actually been given successfully for formalized number theory and formalized analysis, but they do not use means as elementary as Hilbert had sought. He wanted to restrict the methods of such proofs to combinatorial ones in accord with the finitist standpoint. Stronger methods of giving constructive proofs had to be used.

This necessity of going beyond the elementary "finitist" methods in consistency proofs is related to another difficulty. Both came to light on the basis of results obtained by Kurt Gödel and Thoralf Skolem: It was shown that a formal system, if it is to satisfy the conditions of strict controllability, could not completely express its intended theory. This is particularly shown by the fact that the formal system, apart from its normal interpretation by the intended theory, also permits deviant interpretations, the so-called "non-standard" models.

Within foundational research this fact has been dealt with in different ways, either by studying non-standard models more closely or by considering possibilities of excluding non-standard models by an extended kind of formalization. For number theory two ways of extending the procedure of formalization have been considered: on the one hand "infinite induction" and on the other hand the admission of infinite conjunctions and disjunctions. In either case, the finitist character of proof figures is lost.

As regards fundamental reflections, it emerges that the role of formalization is not so simple as was originally intended and, at the same time, that we do not have to demand formalization so unconditionally. In any case, semantics, in keeping with its purpose, uses set-theoretic thinking that is not bound to a formal system.—

As we see, there is no dearth of problems for the philosophy of mathematics. Nevertheless, what I said in one of my essays[2] still holds: "If we ... take as basis the view according to which mathematics is the science of idealized structures, then we have an attitude for research in the foundations of mathematics that will save us from exaggerated aporiae and forced constructions and that is also not contested, even if foundational research brings to light many astonishing facts."

[2] "Schematic correspondence and idealized structures" (*vide* ch. 26, p. 324).