JAAKKO HINTIKKA

HILBERT VINDICATED?

Hilbert's philosophy of mathematics is almost universally labeled *formalism*. I will argue in this paper that such a classification is highly misleading. It does not do justice to the leading ideas of Hilbert's thinking about the foundations of mathematics. His so-called formalism was the result of several independent ideas most of which he could have maintained even if he had given up his formalism. This holds, I will argue, of the most interesting and most characteristic of his foundational ideas. Among such leading ideas of Hilbert's that survive the elimination of formalism there is his axiomatic ideal, which in fact was for him not a mere ideal, of mathematics and science.[1] There is also, it seems to me, another leading idea which is much more elusive than the axiomatic idea. It will be the focus of this paper. It might tentatively be called the idea of mathematical reasoning as combinatorial reasoning.

A starting-point is obtained by asking: If formalism was not central to Hilbert's philosophy of mathematics, how come that he has been classified as one?

In a historical and critical perspective (in the sense of the good old German expression "historisch-kritisch") one can distinguish five different features of his approach to the foundations of mathematics that have at different times occasioned the application of the epithet "formalist" to him. I will suggest that in reality none of them alone justifies the application of the label "formalist" to Hilbert. These excuses for calling him a formalist include the following:

(a) Hilbert's use of a purely logical and axiomatic approach in the foundations of geometry as well as in the other parts of mathematics and even science.
(b) Hilbert's (less than fully articulated) early idea of a set-theoretical universe as a "structure of all structures".
(c) Hilbert's strategy of proving the consistency of various mathematical theories in a purely proof-theoretical manner, that is, by formalizing the logic used in a given mathematical theory and then showing that one cannot derive a contradiction from the axioms of the given theory by means of the formal rules of the logic in question.

Synthese **110:** 15–36, 1997.
© 1997 *Kluwer Academic Publishers. Printed in the Netherlands.*

One can consider (b)–(c) as further elaborations of, or add-ons to, the basic axiomatic idea (a).

The fourth idea goes in a direction different from the axiomatic idea.

(d) Hilbert's preference of combinatorial over set-theoretical reasoning in the foundations of mathematics.

As a fifth assumption one can mention

(e) Hilbert's finitism.

Once again this idea was perhaps not completely articulated by Hilbert.

In this paper, I will concentrate mainly on the fourth alleged reason (d) for calling Hilbert a formalist. One reason for doing so is that it has not received adequate attention in the literature. A further reason is that it is related to certain ongoing developments in the foundations of mathematics. Another reason is that this idea is the one which in conjunction with others, notable (e), actually led Hilbert to a kind of formalist position.

Of the other alleged reasons for calling Hilbert a formalist, (a) does not cut much ice.[2] What is involved in it is that according to Hilbert the derivation from the axioms of a mathematical system is a purely logical manner. Hence it is independent of the meanings of the nonlogical constants of the mathematical system in question. For instance, as far as the derivation of theorems from axioms is concerned, we might as well speak of tables, chairs and beermugs instead of points, lines and circles, as Hilbert put it in his provocative way.[3] Of course, in an actual logical deduction we are likely to replace nonlogical terms by variables of the appropriate type. This feature of Hilbert's approach is what prompted Russell (1937, vi) to call him a formalist and to object to his views.

However, such a conception of a purely logical axiom system does not imply formalism. Aristotle and Euclid used variables in their deductions of theorems from axioms without being formalists. After Hilbert's time, the resurrected idea of a purely logical axiom system has become a virtual commonplace. The reason why it prompted the charge of formalism is its short-term novelty. The idea that mathematical arguments are purely logical had been abandoned in the nineteenth century both by many mathematicians and by many philosophers.

In reality Hilbert's early conception of an axiom system was a squarely model-theoretical one, not a proof-theoretical one. From a systematical vantage point, it does not even begin to justify calling him a formalist.

The second alleged reason (b) for calling Hilbert a formalist pertains to a view which he apparently abandoned himself later. It, too, is squarely

model-theoretical or at least set-theoretical. It amounts to considering, in the same way as Cantor apparently did,[4] the set-theoretical universe as the collection of all possible structures (models).[5] Hence, as soon as an axiom system is consistent, there exists a model for it in that universe. It is in this way, and for this reason, that consistency implies existence. Once again, Hilbert's ideas are model-theoretical rather than formalistic in any strict sense. If there is something unusual about Hilbert's view here, they are his views of set theory and the set-theoretical universe, not about mathematical existence or mathematical truth.

The third allegedly formalistic ingredient (c) in Hilbert's thinking was originally little more than a ploy to prove consistency of mathematical axiom systems and thereby the existence of models for them. The idea was to formalize the logical reasoning used in deriving theorems from axioms in some mathematical axiom system and then prove by considering such formalized proofs that by their means one cannot derive an inconsistency from the axioms. If the formalized logic is complete, then from such a formal consistency one can infer consistency in the model-theoretical sense of satisfiability, that is, the existence of models of the axiom system in question. This is in a nutshell the famous Hilbertian project in the foundations of mathematics.[6]

This is again a poor excuse for calling Hilbert a formalist in his philosophy of mathematics. Hilbert's original ambition is squarely a model-theoretical one, viz., to prove the existence of models for various axiom systems. The means he used in this enterprise may include a formalistic element, but this element pertains to Hilbert's philosophy of logic rather than to his philosophy of mathematics. Hilbert's grand strategy presupposes the completeness (complete formalizability) of the logic we use in mathematics. It may justify calling Hilbert's conception of logic formalistic, but not his conception of mathematics (mathematical theorizing). Understood from the vantage point of Hilbert's original motivation, the ends of his famous foundational project are not formalistic.

In the light of such observations, Georg Kreisel was amply justified in saying that "there is no evidence in Hilbert's writings of the kind of formalist view suggested by Brouwer when he called Hilbert's approach 'formalism' ".[7] This point can be spelled out further by pointing out that the consistency proofs of the kind Hilbert envisaged had nothing to do with the epistemology of mathematics in the sense of concerning the truth, falsity, probability or reliability of any particular mathematical (or scientific) axioms.

In other words, for Hilbert the question of the consistency of an axiom system was completely different from the question of the truth or falsity

(or reliability or certainty) of its different axioms in their application to real world. The truth or falsity of any one axiom had to be established empirically. The truth of Euclid's fifth postulate in the actual universe had to be tested by such means as are used to test Einstein's general theory of relativity (which of course was in historical reality as much Hilbert's theory as it was Einstein's). The same need of empirical verification goes, Hilbert points out explicitly, even for the axioms of continuity.[8] All this illustrates the robust model-theoretical nature of Hilbert's conception of an axiomatic theory.

A consistency proof for a mathematical axiom system was not thought of by Hilbert as pertaining to the question whether its axioms are true or not, but to the question whether the axiom system has a subject matter in the first place which could make it true or false.

However, the last two ingredients (d)–(e) of Hilbert's views together pushed him to a kind of formalist position, even though neither of them does so alone. Once again, the point is about mathematical thinking and reasoning rather than about the contents of mathematical theories. The thesis (d) says that mathematical reasoning can be thought of as pertaining to the ways particular concrete individuals can be combined together into structures of different kinds. If these structures are finite, they can be thought of as being exemplified, not only by tables, chairs and beermugs, but more relevantly by formal symbols on paper or on a computer screen. I will discuss this motivation of Hilbert's formalism in the course of this paper. Clearly, neither (d) or (e) alone suffices to lead anyone to a genuinely formalist position, even though in combination they may be thought of as doing so.

A contributing factor to the confusions surrounding Hilbert's alleged formalism is that his most important statement of his formalism is relatively little known. It appears not to have been translated into English. This *locus classicus* of Hilbert's formalism deserves – and needs – a number of comments. The crucial passage is found in Hilbert (1922, 161–163). Hilbert is considering there the problem of consistency proofs as a paradigm problem in the foundations of mathematics. An abridged translation might run as follows:

> The importance of our problem of the consistency of axioms is admittedly acknowledged by philosophers; but I do not find anywhere the philosophical literature, either, any clear requirement that the problem needs a solution in the mathematical sense. In contrast, our question is essentially affected by the older attempts to find a basis for number theory and analysis in set theory and also a basis of all theories in pure logic.
>
> Frege has attempted to build number theory on pure logic and Dedekind on set theory construed as a chapter of pure logic; but neither has reached his aim. Frege did not handle carefully enough the usual concept formations of logic in their application to mathematics;

for instance, he considered the extension of a concept to be given directly (*ohne weiteres*), in such a way that he believed that these extensions could without restriction be taken themselves to be objects. He was so to speak a victim of extreme conceptual realism. Dedekind met with a similar fate. His classical mistake was to take as his starting-point the system of all objects. . . .

This is in my view the present state of the question as far as the foundations of mathematics are concerned. Accordingly studies of the foundations can only reach a satisfactory outcome through a solution of the problem of the consistency of the axioms of analysis. If we succeed in showing this consistency, then we can conclude that mathematical propositions are in fact indubitable and final truths – a result which is of the greatest significance to us also because of its general philosophical character. We turn now to the solution of this problem.

As we saw, abstract operating with the extensions and contents of general concepts has turned out to be insufficient and dangerous. As a precondition of the application of logical inferences and the implementation of logical operations, something must already be represented [to the mind], viz., certain nonlogical discrete objects which are present in all thinking intuitively and in immediate experience. If logical inference is to be reliable, it must be possible to review them and all their parts completely, and their structure, their individuation and their order are given to us together with the objects themselves as something that cannot be reduced to anything else. In that I adopt this point of view, the objects of number theory are – in a precise opposition to Frege and Dedekind – [number] symbols themselves. Their form can be recognized universally and with certainty independently of location and time, independently of how the symbols are produced and independently of minor variations in the execution of this production. Here is the philosophical attitude that I consider indispensable for the foundations of pure mathematics – as well as for all scientific thought, understanding and communication: *In the beginning* – so to speak – *there was the symbol.*

Certain comments are in order here.

(1) One of the most remarkable features of these statements of Hilbert's is that there is in it no mention of formalism in the strict sense, that is, of the requirement that the signs must not *mean* anything. This absent thesis, the thesis of formalism, comes in only through the fact that symbols happen to be handy "nonlogical discrete objects which are present in all thinking intuitively and in immediate experience".

(2) Hilbert's comments in the quoted passage have to be taken in the context of the rest of his ideas and in the context of his development. They also illustrate the first four excuses for calling Hilbert a formalist mentioned above. Hilbert's first and foremost emphasis in the foundations of mathematics (and in the foundations of science in general) is on the axiomatic method. His second and almost as important emphasis is on the purely logical character of satisfactory axiom systems. Furthermore, Hilbert represented (as was seen earlier) the idea that purely logical inferences must be characterizable and recognizable purely formally, on the basis of the

symbols alone that are being used. Hence Hilbert's tacit "syllogism" runs somewhat as follows: The basic clarified form of mathematical theorizing is a purely logical axiom system. All logical reasoning can be carried out purely formally. Hence in a logically clarified mathematical theory all reasoning can be carried out purely formally, as if it involved merely manipulating certain symbols. So understood, Hilbert is not saying that mathematical symbols do not mean anything. All that he is saying is that in order to be on the safe side in one's logical inferences in mathematics, one must be able to follow the rules of logical inferences blindly, as if the symbols had no meaning. As Poincaré saw, this idea is present as early as in the *Grundlagen der Geometrie*. As far as Hilbert's axiom system is concerned, "we might put the axioms into a reasoning apparatus like the logical machine of Stanley Jevons, and see all geometry come out of it" (Poincaré 1902, 150 of the English translation).

Many of the statements Hilbert makes in the quoted passage can naturally be understood as merely reiterating these points. Otherwise the explicit generality of his statements would be incomprehensible. How can "all scientific thought and communication" be purely formalistic? A purely formalistic philosophy of mathematics is possible because it is actual, if not in the writings of Hilbert, then at least in the writings of someone like Haskell B. Curry (e.g., Curry 1954). But a formalistic philosophy of physics does not make any sense.

By far the most natural way of taking Hilbert's words, at least their main thrust, is to take them to take off from the formal character of all valid *logical* inference. In other words, Hilbert is in the quoted passage expressing the idea that all *logical* reasoning can be carried out purely formally. This point, whether or not it is all that Hilbert is here saying, has nothing to do with a formalistic theory of *mathematics*, as little as it has to do with a formalistic theory of physics. Thus Hilbert may be committed to a formalistic philosophy of logic, but scarcely to anything that can plausibly be called a formalistic philosophy of mathematics. Moreover, the same idea of a purely formal character of logical laws was shared with Hilbert by the likes of Frege, Wittgenstein and Carnap. One does not even have to attribute a major historical inaccuracy to Hilbert, as might first seem to be necessary because of the contrast he sets up between his ideas and those of Frege. For even though both maintained a purely formal character of the actual logical laws (e.g., the rules of inference used to derive theorems from axioms), they reached this view from opposite directions, Frege from a belief in the universality of his *Begriffsschrift* but Hilbert from an essentially model-theoretical point of view.

(3) This suggestion can be strengthened by considering the immediate context of Hilbert's utterances. Hilbert is in the quoted passage obviously thinking primarily in terms of the problem of proving the consistency of arithmetic and analysis. Hence his statement of his "formalistic" program in the quoted passage is merely a description of the strategy of his attempted consistency proofs, not a philosophical manifesto. In brief, the abstraction from the meanings of symbols and the exclusive concentration on their formal properties is (in part at least) merely one possible way of carrying out a consistency proof

The same point can be placed in a wider context. Hilbert's so-called formalism was not calculated to eliminate nonconstructive existence proofs, but to vindicate them.

What philosophers have failed to see is that Hilbert's so-called formalism was like Russell's "reduction to acquaintance". He did not deny the role of nonformal reasoning in mathematics or its validity. He wanted to show the acceptability of such reasoning by means of formalistic reasoning.

This is not the whole story, however.

(4) Hilbert's remarks are addressed in so many words to Frege's and Dedekind's attempts to base arithmetic and set theory on "reine Logik" and even reduce them to logic. He is hence talking about the basic logic that is needed in the foundations of mathematics, not about what is going on in real working mathematics. Insofar as Hilbert really represents a formalistic position, *he is a formalist in the philosophy of logic, not in the philosophy of mathematics*. In the quoted passage Hilbert is not offering formalism of any sort as an alternative to the usual reduction of the more advanced parts of mathematics like analysis to arithmetic and/or set theory. He is talking about the reduction of arithmetic and set theory to logic.

(5) The contrast which Hilbert sets up between his own approach and those of Frege and Dedekind is clear. According to Hilbert, what was insufficient and insecure in their procedure was that they operated with abstract concepts, either with their extensions (*Umfänge*) or with their intensions (*Inhalte*). Hilbert wants to resort instead to operating with directly given discrete nonlogical objects.

As an example of such concrete, given objects Hilbert chooses certain symbols. The reason for this choice is their concrete givenness and manipulability. For these properties, the question as to whether the symbols in question represent something or not is totally immaterial. Symbols are not drafted to Hilbert's service because they are purely formal, i.e., nonrepresentational. Rather, they are preferred by Hilbert because they are

intuitively given to us in direct experience and hence admit of a complete overview.

Thus Hilbert's emphasis on the role of "nonlogical discrete objects" in logic and the foundations of mathematics is directed squarely against the likes of Frege and Dedekind with their use of abstract concepts. Hilbert was favoring first-order reasoning over set theory or higher-order logic, albeit not necessarily in the form of what now goes by the name of first-order logic. Hence the term "formalism" is once again seriously misleading when applied to Hilbert's views. Even though he does not use the term himself and even though it is subject to misunderstanding, it seems to me that the term *combinatorial* is a much happier one than "formalistic".

The same point can be put differently. It is clear that Hilbert viewed analysis, set theory, and parts of logic as involving "ideal elements" which are not self-explanatory and whose role in mathematics ought to be explained by reference to concrete combinatorial operations. As was mentioned above, it is for instance very interesting to see that all the mistakes Hilbert attributes to Frege and Dedekind concern set-theoretical or higher-order matters, not their views on ordinary first-order logic.

(6) As an epistemological program, Hilbert's suggestions may be compared with the program of such fellow reductionists as Russell or Husserl. Hilbert is envisaging a kind of reduction of all mathematical reasoning to something like Russellian acquaintance. The crucial question in Hilbert is when the kind of reduction we are dealing with cannot be continued any further. His view is that we have reached a rock bottom with objects which are directly given of us and which are under our control. We have to realize that the Hilbertian reduction is merely a structural one. He is dealing with logical inferences that are independent of the subject matter to which it is applied, be it points, lines and circles or tables, chairs and beermugs. Therefore Hilbert does not need acquaintance with any particular objects. All that is needed are *some* objects that are so to speak clear and distinct. Symbols are Hilbert's candidates for this role merely because they are directly given to us and completely in our control. Their being merely formal in the sense of not having a meaning is irrelevant to Hilbert.

In Russell, the inverse of his famous reduction to acquaintance, that is, the logical construction of objects of description out of objects of acquaintance, was calculated to replace inferences to the existence of the objects of description and hence to vindicate objects of description. In Hilbert, too, showing how higher-order reasoning can be reduced to combinatorial reasoning was an essential step in proving that such reasoning cannot lead to contradictions and thereby in vindicating higher-order reasoning.

(7) What does Hilbert want to say about logical reasoning? A careful scrutiny of Hilbert's statements fails to reveal any doubt of the actual validity of our ordinary interpreted principles of logical reasoning, which according to Hilbert apparently include the axiom of choice. He is not trying to revise our logic or to replace interpreted logic by something else. He is trying to justify it by showing its combinatorial character.

In fact, Hilbert spells out almost in so many words that he is not trying to replace the ordinary methods of logical inference by anything less but to formulate the rules of inference explicitly ("formally") and to understand them better. Admittedly, he was fully aware of the view that "mathematicians [have] paid little attention to the validity of their deductive methods" and that this neglect is what led to set-theoretical paradoxes. Yet he firmly believed that there is an unproblematic core of sound deductive principles.

> Does material [or, perhaps more accurately translated, interpreted (*inhaltlich*)] logical deduction somehow deceive us or leave us at lurch when we apply it to real things or events? No! Material logical deduction is indispensable. (Hilbert 1926, 191 of the English translation)

Moreover, Hilbert believed that an essentially combinatorially interpreted deductive logic can be adequately captured by the usual formalization of logic.

> . . . we find logical calculus already worked out in advance. . . . We possess in the logical calculus a symbolic language which can transform mathematical statements into formulas and express logical deductions by means of formal procedures. . . . Material [interpreted] deduction is thus replaced by a formal procedure governed by rules. The rigorous transition from a naive to a formal treatment is affected, therefore, both for the axioms . . . and for the logical calculus (which was originally supposed to be merely a different language).

Hilbert's strategy makes no sense unless it is assumed that the interpreted logical deductions he proposes to formalize capture all the valid methods of purely logical deduction. (See Hilbert 1926, 197 of the English translation.)

Another relevant observation here is that Hilbert cared a lot of the actual correctness of interpreted principles of reasoning. The prime example is the axiom of choice. But if so, he could not have wanted to restrict our actual reasoning to finitistic formal reasoning. Such reasoning was for him merely a ploy for consistency proofs. Once again, a Hilbertian reduction is calculated to vindicate what is being reduced, not to eliminate it.

(8) This line of thought can be continued. Taken strictly literally, Hilbert's formulation allows for the possibility that the symbols he is considering have a meaning and that the concrete manipulations we perform on them

likewise has an intuitive content, e.g., that they can be interpreted as logical inferences. The only condition is that we are dealing with concrete operations on given discrete objects.

In particular, Hilbert's application of his "formalistic" ideas to number theory, with number symbols playing the role of the concrete objects Hilbert wants to focus on, is not the only possible way of putting his combinatorial approach to work. Furthermore, it is not a precondition of this application that we forget about the meaning of numerical symbols. Hilbert's emphasis is merely of actual operations on concrete given objects. There could in principle be other applications of the same basic ideas.

It is important to realize that this is not merely an abstract possibility of interpreting Hilbert's words. Interesting and important developments in the philosophy of logic turn out to exemplify my interpretation of Hilbert's statement, and large segments of his own foundational work turns out to be in keeping with the interpretation.

(9) As an example of conceptualizations falling under Hilbert's formulation, let us consider the Hintikka-Beth perspective on first-order proofs (see Hintikka 1955; Beth 1955). An attempted proof of say, an implication from F to G, is on this interpretation a step-by-step procedure aiming to construct a description of a model in which F is true but G false. If such an attempt is inevitably frustrated in all directions, a countermodel is impossible, and G is therefore implied logically by F.

Hence this procedure has a clear interpretation. At the same time, it can be carried out as a concrete operation whose rules refer only to the signs (symbols) involved. Not only can the rules which govern the countermodel (*tableau*, model set) construction be formulated purely formally. If the countermodel construction does succeed, one can use a device originating from Henkin's (1949) completeness proof and use the set of formulas (configurations of symbols) which results from the constructions as its own model. Hence the entire procedure can be thought of as an operation on concrete symbols from beginning to end.

Even if some countermodels take an infinite number of steps to complete in this way, every step and every initial segment of the process is finite, and governed by purely formal operating rules. This includes all ordinary first-order proofs, for they correspond to attempted constructions that reach a dead end after a finite number of steps in all directions.

Hence Hilbert might as well have considered the entire first-order as a safe and unproblematic part of logic. It has only individual variables and quantifiers ranging over individuals, but no variables ranging over concepts or their extensions. First-order logic is not a logic of abstract general

concepts. It can be viewed as a logic which turns on instantiation rules, that is, rules governing the choice of concrete objects calculated of instantiate general concepts (usually complex ones). We may perhaps formulate this (feasible) view by saying that first-order logic is a combinatorial rather than conceptual discipline within logic, even though the term "combinatorial" has to be taken here with a grain of salt.

The only thing that could have prevented Hilbert from adopting this way of looking upon first-order logic is his finitism (e). For the models (or countermodels) constructed by means of the *tableau* rules or by means of the rules of the tree method are not always completed in a finite number of steps. Such models can perhaps be called combinatorial, but they are not finitistic.

(10) If you want to see a small scale example of Hilbert's thinking, the usual naive rule of existential instantiation and its philosophical vindication from the Beth-Hintikka viewpoint will fill the bill. Suppose that you move from an existential formula $(\exists x)S[x]$ to a substitution instance of $S[x]$, say $S[a]$, in virtue of the rule of existential instantiation. Suppose further that your first-order language in question is an interpreted one, so that individual constants actually refer to certain objects. But the "dummy name" a introduced in existential instantiation cannot do so. A logical rule does not provide an actual example of the entities that satisfy $S[x]$. The symbol a plays in a sense a purely formal role in the rest of the logical argument. Here, then, is a familiar example of how the manipulation of a "purely formal" symbol can serve the purposes of logical reasoning, just as Hilbert envisaged.

Admittedly, even the "dummy names" of existential instantiation have an interpretation in a wider sense of the word, not as standing for some mythical "arbitrary objects" but as ingredients of an attempted experimental countermodel construction á la Beth. But this interpretation only strengthens Hilbert's hand, for the only model in the literal sense of the word that is being constructed is a set of formulas. By an interesting switch of perspective (first employed by Henkin), the formulas of this set are reinterpreted so as to speak of the purely formal entity that this set itself is. Hence this interpretation does not diminish the value of existential instantiation as an illustration of Hilbert's point about the uses of symbols qua symbols in reasoning.

(11) A perceptive reader will have noticed the similarity, indeed virtual identity, of my way of viewing first-order logic and Kant's conception of the characteristically mathematical method (see here Hintikka 1973). As

I have shown, Kant considered it to be a characteristic of the method of mathematicians that they consider their general concepts through particular representatives of these concepts. Kant referred to this as a method of constructing those concepts. This is closely similar to Hilbert's idea to base the logic that we need in mathematics on operation on concrete particulars.

There is an important difference, however, between Kant and Hilbert. Hilbert's attempt to find precedents to his views in Kant may perhaps have helped to hide the fact that he was dealing here with the interpretation of logical thinking rather than mathematical one, as Kant was. But this confusion is partly terminological only. It does not affect the insight we have reached in the nature of first-order logic as satisfying Hilbert's requirements on the kind of reasoning that can be used in a *Neubegründung der Mathematik*. Perhaps we should paraphrase or perhaps parody the Genesis once again: In the beginning there was first-order logic. Or, in less poetic terms part of Hilbert's formalism is merely a preference of first-order logic over higher-order logics and over set theory in the foundations of mathematics. In the passage quoted earlier, Hilbert even lists three misleading ideas. They involve: (i) *Begriffsrealismus*, that is, reliance on concepts; (ii) reification of extensions of concepts into objects; (iii) the idea of a *System der allen Dinge*, i.e., the idea of the totality of all (possible) objects. It is instructive to see that all these dubious ideas go beyond the realm of first-order logic.

(12) The perspective reached by this line of thought seems to accord well with what Hilbert was trying to do. Much of his concrete efforts in logic consisted in attempts to extend the unproblematic character of first-order reasoning to other kinds of reasoning used in mathematics, especially set-theoretical reasoning.

In these efforts, an important role was played by Hilbert's way of looking at first-order reasoning. As Warren Goldfarb (1979) has pointed out, Hilbert saw the nature of quantifiers as tacitly embodying certain choice functions (Skolem functions). The function of quantifiers is thus closely related to the principle of choice. The idea of choice is indeed closely tied to Hilbert's vision of a combinatorial basis of mathematics.[9] The trouble with the axiom of choice is that it either involves the introduction of a new concept (the choice function) or else an infinite number of choices. Hilbert tried to solve these problems by first in effect postulating a universal choice function in the form of his epsilon-terms, and then trying to vindicate this procedure by showing that only a finite number of choices are involved in any one logical argument.

A separate examination is required to answer the question whether Hilbert succeeded in his attempt – whether, for instance, he realized his ambition of showing that in suitable formulation the axiom of choice is as undeniably valid as $2 + 2 = 4$.[10] For the present purposes, it suffices to have shown that Hilbert himself construed his "formalism" in the same spirit as we. It would not even be to far fetched to suggest that one of the aims of Hilbert's famous program was to show the combinatorial character of all mathematics.

(13) The comparison with Kant mentioned briefly is important enough historically to be deepened further. In a sense, Hilbert is reversing the Kantian view of the relation of mathematics to logic. For Kant, logic deals with general concepts while mathematics uses particular concrete representatives of general concepts, i.e., instantiation rules. For Hilbert, it is logic that has to be based on concrete individuals instantiating certain complex concepts codified by open formulas. In contrast, mathematics involves certain general concepts to be analyzed in terms of logic and set theory, such as limit, continuity, differentiability, integral, etc. This inversion of Kant's conception of mathematical reasoning and its relation to logic makes doubly ironic the attribution of the label "formalist" to Hilbert. For Kant, the kind of use particular concrete representatives of general concepts that Hilbert emphasizes in his combinatorial view of logic is precisely what he means by the use of intuitions. Hence, in the historically accurate Kantian terminology, Hilbert was asserting that the basis of logic and mathematics is the use of intuitions, which Kant even defines as particular representatives of general concepts.

(14) Suggestions have in fact been made in the literature to interpret Hilbert's program in roughly the same spirit as I am doing, that is, as emphasizing the combinatorial character of the logical basis of mathematics outlined in the preceding section. For instance, Georg Kreisel (1958, 210 of the reprint) notes, speaking of Hilbert's basic assumptions and noting the failure of Hilbert's original finitistic methods to do their job, that

> . . . instead of having a single kind of elementary reasoning whereby we understand the use of transfinite symbols, there will now be methods of reasoning involving a hierarchy of conceptions such as, e.g., more and more abstract conceptions of "construction", and we have a hierarchy of Hilbert programmes of *discovering the appropriate complex of such methods which is needed for understanding the use of transfinite symbols in given systems* (modified Hilbert programme). [Emphasis Kreisel's]

In this paper I am mainly interested in a couple of members of the kind of hierarchy of methods Kreisel envisages. I am interpreting "the use of trans-

finite symbols" to amount to an essential use of set-theoretical or higher-order concepts, and the elementary methods Kreisel mentions as first-order methods, which could also be called combinatorial (in a sufficiently wide sense). The initial passage quoted from Hilbert and my comments on it will show you how close this way of looking at Hilbert's words is to their literal meaning. The main remaining difference between Hilbert and myself would then be that he restricted his attention to finite combinatorics while I am also considering infinitary combinatorial methods.

(15) In sum, it is Hilbert's emphasis on the concrete operations on symbols as the true foundation of logic that comes closest to justifying the application of the term "formalist" to him. And, in conjunction with his belief (e) in the finitary character of rock-bottom, fully analyzed mathematical reasoning, it does lead him to a form of formalism. But even so, this label can be misleading. It should be understood by reference to his historical background in mathematics and in the light of his own work on the foundations of mathematics. In particular, these sources of information and insight should be kept in mind in examining and evaluating Hilbert's program in the foundations of mathematics.

(16) But is Hilbert's vision of the combinatorial character of the true logical basis of mathematics true, or is it a mirage? Kreisel (1958, 213 of the reprint) has said that even after Gödel's results,

> When asked "What is mathematics about?" Hilbert could still have said: about arithmetico-combinatorial facts of finitist mathematics. . . .

Unfortunately,

> Hilbert's answer is simply not true even for the very weak sense of "equivalence of content" expressed in statements of formal deducibility and nondeducibility (loc. cit.).

My answer to the question "What is mathematics all about?" is a variant of Hilbert's, to wit, "about arithmetico-combinatorial facts, whether finitistic or not". Since the relevant sense of "arithmetico-combinatorial" here comes closely to "first-order", the crucial question becomes: Can all mathematical reasoning be understood as first-order reasoning?

It might at first seem that there is no hope whatsoever of vindicating the idea that logically speaking one could reconstruct all mathematical reasoning on the first-order level. Most of the characteristically mathematical concepts and modes of reasoning cannot be expressed by means of ordinary first-order logic, including mathematical induction, infinity, equicardinality, power set, well-ordering, etc. The most commonly used conceptual framework for mathematics is set theory, whose very name codifies the idea of mathematical reasoning as dealing with such universals as

sets (and possibly concepts) in contradiction to such concrete particulars as Hilbert put his faith on.

Unlikely or not, recent work by myself and my associates throws some sharp light on this question and (with certain not unimportant qualifications) even vindicates Hilbert's vision. The main qualification is that we cannot restrict ourselves, *pace* Hilbert, to finite combinatorics but must indulge in infinitary combinatorics.

But even so, the shortcomings of the ordinary first-order logic are so conspicuous that any attempt to think of it as a viable framework of all mathematics is bound to seem quixotic. It is here that we come to the decisive new insight that eluded Hilbert, as it has eluded other recent foundationalists. The ordinary codification of first-order logic is not an adequate logic of first-order (combinatorial) reasoning. To put the point in different terms when Frege and Russell formulated what is now called first-order logic (quantification theory), they made a serious mistake. They formulated their logic too narrowly. When these unnecessary limitations are removed, we obtain a stronger logic. It might be called independence-friendly (IF) first-order logic.[11] Unlike ordinary first-order logic, it can serve as a philosophically satisfactory framework of all mathematics. All normal mathematics can in principle be thought of as being conducted within the framework of IF first-order logic. Needless to say, this is neither heuristically nor expositionally the natural procedure.)

This claim prompts two questions: (i) What is IF first-order logic? (ii) In what sense can it serve as a framework of all mathematics? Let us take these two questions in order. Since I will be dealing with this point in some more detail elsewhere, I can perhaps be rather brief here.

(i) Hilbert's very own emphasis on quantifiers as codifying choice functions provides a clue to the idea of informational independence. A choice is always make on the basis of certain given information, that is, depending on certain given parameters. What are these parameters in the case of first-order logic? Consider, as an example, the sentence

$$(1) \qquad (\forall x)(\exists y)(\forall z)(\exists u)S[x, y, z, u]$$

Here the choice of the value of y depends on the value of x, and the choice of the value of u depends on the values of both x and z. This is reflected by the second-order Skolem-function translation of (1),[12] which is

$$(2) \qquad (\exists f)(\exists g)(\forall x)(\forall z)S[x, f(x), z, g(x, z)]$$

Here the order of the different quantifiers in (1) is reflected in (2) by the fact that the function f which replaces y has x as its sole argument whereas

the function g replacing u has both x and z as its arguments. But from the viewpoint of quantifiers as embodiments of choice functions, it would make perfect sense to require instead that u depend only on z, i.e., that the function replacing u have z as its only argument. Then (2) changes into

(3) $\quad (\exists f)(\exists g)(\forall x)(\forall z)S[x, t(x), z, g(z)]$

But what will now be the first-order counterpart to (3) (like (1) to (2))? A moment's thought shows that the obvious intuitive import of (3) cannot be captured by means of any linear sequence of quantifiers. One thing that we can do is to use branching quantifiers:

(4) $\quad \left.\begin{matrix}(\forall x)(\exists y)\\(\forall z)(\exists u)\end{matrix}\right\} \quad S[x, y, z, u]$

Instead, and much more systematically, we can introduce a special slash notation $(\exists u/\forall x)$ to exempt $(\exists u)$ from the scope of $(\forall x)$. Then (3) and (4) are equivalent to

(5) $\quad (\forall x)(\exists y)(\forall z)(\exists u/\forall x)S[x, y, z, u]$

as well as equivalent to

(6) $\quad (\forall x)(\forall z)(\exists y/\forall z)(\exists u/\forall x)S[x, y, z, u]$

When this idea (and this notation) is used systematically, we obtain what I have called independence-friendly (IF) first-order logic.

An important additional explanation must be added, however. In If logic, dependent and independent disjunctions have to be dealt with in the same way, *mutatis mutandis*, as dependent and independent existential quantifiers. For instance, we can have sentences like

(7) $\quad (\forall x)(\exists y)(\forall z)(S_1[x, y, z](\vee/\forall x)S_2[x, y, z])$

which is equivalent with

(8) $\quad (\exists f)(\exists g)(\forall x)(\forall z)((S_1[x, f(x), z] \,\&\, g(z) = 0) \vee$
$(S_2[x, f(x), z] \,\&\, g(z) \neq 0))$

but not with any ordinary first-order sentence.

Such independent disjunctions are an integral part of IF first-order logic.

(ii) But in what sense can IF first-order logic serve as a framework for "all" mathematics? An answer to this question can be given in two steps.

(a) First, it can be shown that every Σ^1_1 second-order sentence can be translated into an IF first-order language. This result has been known for a while. It goes back at least to Walkoe (1970). (b) Second, there is a sense in which each question concerning the theoremhood of a normal mathematical proposition is equivalent to a question concerning the validity of a Σ^1_1 second-order sentence. This reduction holds for any mathematical theory (finite axiom system) T that can be formulated in the theory of finite types.

What we can do then is to reconstruct the requisite part of the theory of types as a many-sorted first-order theory, in an obvious way. Then the only thing that is not captured by doing so is the requirement that for each class of lower-order entities there must exist a higher-order entity having them, and only them, as its members. This can be done by means of a finite number of second-order sentences of the form

(9) $\quad (\forall X)S[X]$

where $S[X]$ is a first-order formula. In this way T is transformed into a Π^1_1 second-order sentence T^*.

Then the question whether a sentence C is a theorem of T becomes the question whether a second-order sentence of the form

(10) $\quad (T^* \supset C^*)$

is logically true (valid), where C^* is the translation of C into the language of the many-sorted first-order theory. But (8) is of the Σ^1_1 form, and hence translatable into an IF first-order language. Hence the question whether C is a theorem of T equals the question whether a certain sentence of IF first-order logic is valid.

Questions of this kind are not finitary, for IF first-order logic is not axiomatizable, but they are in an obvious sense combinatorial. They concern the possibility or impossibility of certain relational structures of individuals. They do not involve any quantification over sets or any other higher-order entities. Hence this reduction shows a sense in which practically all mathematical problems can be taken to be at bottom combinatorial questions.

But does that reduction of all classical mathematics (i.e., all mathematics that can be expressed in higher-order logic) vindicate Hilbert? It remains to be studied in what sense (if any) IF first-order languages can help us to fulfill Hilbert's dream of showing the consistency of arithmetic and analysis. What is clear in any case is that it does not save Hilbert's specific program of finding finitistic consistency proofs for arithmetic.

But that specific program is impossible anyway. Hilbert believed that the kind of logic needed in mathematical reasoning is axiomatizable. Indeed, Hilbert (1918, 153) believed that such an axiomatization had already been accomplished.

> The completion of this grand enterprise of Russell's of an *axiomatization of logic* can be viewed as the crowning achievement of the project (*Werk*) of axiomatization in general.

Alas, Hilbert's trust in a deductively (semantically) complete logic of mathematics turned out to be misplaced. Gödel's results show Hilbert's belief simply not true. This point is reinforced by IF first-order logic. Even though it is our basic ground-floor logic, in that it is the true logic of quantifiers, it is not axiomatizable.

This leads immediately to the nod question. What is a realistic way of fulfilling as much of Hilbert's vision as possible? What is the best thing that *can* be done? More analysis and more discussion is undoubtedly needed to answer that question. But a couple of facts seem to be clear to me. First, in his own statements, like the one quoted in the beginning of this work, Hilbert emphasizes the need of a combinatorial foundation of the logic needed in mathematics as much as, and more than, the allegedly purely formal nature of mathematical reasoning, which he interpreted as reasoning by means of an axiomatizable logic. In this respect, the reduction to IF first-order logic seems to me to vindicate the essential elements of Hilbert's vision as fully as it is possible to vindicate Hilbert in any sense.

There exists of course alternative post-Gödelian reconstructions of Hilbert's program. Most of them are nevertheless proof-theoretical. While acknowledging their great interest, it seems to me that the only way to clear-cut progress here is a model-theoretical one. And this is the way which I have followed. The question I have shown how to reduce to IF first-order logic is precisely: Are all the models of a given axiom system included in the models of a putative theorem? And each such question turns out to be equivalent to a question concerning the validity (truth in each model) of some one sentence of IF first-order logic.

But a critic might still object to my vindication of combinatorial reasoning as the "tool of all tools" of mathematical thinking. For a critic might question the status of my reasoning in this paper about IF first-order logic and more generally the status of model-theoretical and other metatheoretical reasoning about my IF first-order logic. The cornerstone of all model theory is the notion of truth in a model. And when one looks at that, one immediately sees that Tarski-type truth-definitions inevitably involve second-order logic. Likewise, so do my truth-conditions for IF first-order sentences.

This seems to entail a catastrophe for my variant Hilbertian program of construing all mathematical reasoning as being at bottom combinatorial, that is, If first-order reasoning. Not only does a second-order truth-definition threaten to transcend the scope of first-order conceptualizations. Most philosophers and logicians think of second-order logic as involving most of the same problems of set existence as set theory. In Quine's words, second-order logic is for them "set theory in sheep's clothing". Thus there seems to be a type of mathematical reasoning, viz., a metamathematical one, which belies my thesis of the essentially combinatorial character of mathematics.

It is here that the virtues of IF first-order logic are seen at their best (cf. here Hintikka 1996, Chap. 6). A moment's look at truth-conditions for IF first-order sentences, e.g., a look at such second-order sentences as (3) or (8), shows that they are of the Σ_1^1 form. As was mentioned above, that implies that they can be translated into an IF *first-order* language.

Furthermore, it can be shown that these truth-conditions for IF first-order sentences can in the most natural sense imaginable be combined into a truth predicate definable in the very same IF first-order language, assuming of course that it is rich enough to allow us to express its own syntax, for instance via the technique of Gödel numbering. In brief, the notion of truth for a suitable IF first-order language can be handled in that very same language. More generally, there are in principle no obstacles to developing a model-theoretical metatheory of a suitable IF first-order language in that same language. IF first-order languages can be self-applied not only syntactically but semantically.

Thus IF first-order logic also enables us to carry out an essential part of Hilbert's overall vision. It enables us to construct a metalanguage for the language of each mathematical theory, a metalanguage which does not involve any objectionable set-theoretical or higher-order elements. In fact, if the language of the given theory is rich enough, then it can serve, after having been reconstructed in IF first-order terms, as its own metalanguage. Admittedly, such languages are not finitistic and hence do not fulfil Hilbert's daydream. But in a deeper sense, it seems to me, they do vindicate his combinatorial vision.

In modifying the biblical opening line for the purpose of his motto, Hilbert perhaps ought to have opted for the other pole of Quine's contrast:

In the beginning there was the particular object.

NOTES

[1] Hilbert thought that the axiomatic method was needed to identify the substantive assumptions of a scientific, e.g., physical, theory. A statement to this effect is found in Frege (1976, 68–69) (letter to Frege, 29 December 1899). The same point is also the burden of the sixth of his famous list of mathematical problems (Hilbert 1900). Hilbert practiced what he preached for instance in his axiomatization of thermodynamics; see Hilbert (1912).
[2] For instance, in the index of Torretti (1978) we find a reference to Hilbert's "formalism". But all that there is in the text is a description of how "Hilbert proves the consistency of his axiom system by proposing an interpretation which satisfied it", followed by a statement that in his "later life, Hilbert devoted much effort to prove the consistency of arithmetic directly, by constructing a sound, complete, syntactically consistent formalization of it". But the notion of completeness used here by Torretti refers to the model-theoretical notion of logical consequence and hence is not a formalistic one.
[3] For the background of this quip, see Toepell (1986a, 40–43).
[4] Cf. Dauben (1979, 142–148).
[5] Indirect support for attributing this idea to Hilbert is found in Tarski (1956, 199), who describes Hilbert's project as capturing the idea of truth for a class of models ("individual domains") under the cover of the term "general validity".
[6] This background of Hilbert's later program in the foundations of mathematics is often neglected. Against this background, it is seen that Hilbert's program had nothing to do with the applicability of mathematics to reality, with the reliability of interpreted logical reasoning in mathematics, etc. Here my interpretation is in contrast to such works as Detlefsen (1986).
[7] See Kreisel (1958, 346).
[8] See Hilbert (1918, 149).
[9] With the following, cf. Hilbert and Bernays (Vol. 2; 1034–1039); Leisenring (1969).
[10] See Hilbert (1922, 157; 1923, 151–152).
[11] For it, see Hintikka (1995, 1996, Chaps. 3–4).
[12] All such translations are (both in ordinary first-order logic and in IF first-order logic) of the Σ^1_1 form, that is, they have the form of a string of second-order existential quantifiers followed by a first-order formula.
[13] Cf. here Hintikka (1996, Chap. 9).

REFERENCES

Benacerraf, Paul and Hilary Putnam (eds): 1983, *Philosophy of Mathematics*, 2nd ed., Cambridge University Press.
Beth, Evert: 1955, 'Semantic Entailment and Formal Derivability', *Mededelingen van de Koninklijke Nederlandse Akademie van Wetenschappen*, Afd. Letterkunde, N.R., Vol.18, No.13, pp. 309–342.
Curry, Haskell B.: 1954, 'Remarks on the Definition and Nature of Mathematics', *Dialectica* **8**, 228–33; reprinted in Benacerraf and Putnam, 1983, pp. 202–206.
Dauben, J. W.: 1979, *Georg Cantor*, Harvard University Press, Cambridge, Massachusetts.
Detlefsen, Michael: 1986, *Hilbert's Program*, D. Reidel, Dordrecht.
Frege, Gottlob: 1976, in G. Gabriel et al. (eds.), *Wissenschaftliche Briefwechsel*, F. Meiner, Hamburg.

Goldfarb, Warren: 1979, 'Logic in the Twenties: The Nature of the Quantifier', *Journal of Symbolic Logic* **44**, 351–68.

Henkin, Leon: 1949, 'The Completeness of the First-Order Functional Calculus', *Journal of Symbolic Logic* **14**, 159–66.

Hilbert, David: 1899, *Grundlagen der Geometrie*, originally in *Festschrift zu Feier der Enthüllung des Gauss-Weber-Denkmals*, Teubner, Leipzig. Several later revised editions, including second, 1903, seventh, 1930, tenth, 1968.

Hilbert, David: 1990 'Mathematische Probleme', *Nachrichten der Kgl. Gesellschaft der Wissenschaften zu Göttingen*, Math.-phys. Klasse **3**, 253–297.

Hilbert, David: 1912, 'Begründung der kinetischen Gastheorie', *Mathematische Annalen* **72**, 562–77.

Hilbert, David: 1918, 'Axiomatisches Denken', *Mathematische Annalen* **78**, 405–15.

Hilbert, David: 1922, 'Neubegründung der Mathematik' ('A New Foundation for Mathematics') *Abhandlungen aus dem Mathematischen Seminar der Hamburg Universität* **1**, 157–177.

Hilbert, David: 1923, 'Die Logische Grundlagen der Mathematik', *Mathematische Annalen* **88**, 151–165.

Hilbert, David: 1926, 'Über das unendliche', *Mathematische Annalen* **95**, 161–190, in P. Benacerraf and H. Putnam (trans./eds.), *Philosophy of Mathematics: Selected Readings*, 2nd ed., Cambridge University Press, 1983, pp.183–201, esp. pp.190–191.

Hilbert, David: 1935, *Gesammelte Abhandlungen*, Vol. 3, Springer-Verlag, Berlin.

Hilbert, David: 1972 (1912), 'Foundations of the Kinetic Theory of Gases', in Stephen Brush (ed.), *Kinetic Theory*, Pergamon Press, New York, pp. 89–101. (English translation of the 1912 original.)

Hilbert, David and Paul Bernays: 1934–1939, *Grundlagen der Mathematik*, Springer-Verlag, Berlin.

Hintikka, Jaakko: 1955, 'Form and Content in Quantification Theory', *Acta Philosopica Fennica* **8**, 11–55.

Hintikka, Jaakko: 1973, *Logic, Language-Games and Information*, Clarendon Press, Oxford.

Hintikka, Jaakko: 1995, 'What is Elementary Logic? Independence-Friendly Logic as the True Core Area of Logic', in K. Gavroglu et al. (eds.), *Physics, Philosophy and the Scientific Community*, Kluwer, Dordrecht, pp. 301–326.

Hintikka, Jaakko: 1996, *The Principles of Mathematics Revisited*, Cambridge University Press.

Kreisel, Georg: 1958, 'Hilbert's Programme', *Dialectica* **12**, 346—72; reprinted with revisions in Benacerraf and Putnam, 1983, pp. 207–238.

Leisenring, A. C.: 1969, *Mathematical Logic and Hilbert's ε-Symbol*, MacDonald Technical & Scientific, London.

Peckhaus, Volker: 1990, *Hilbertprogramm und Kritische Philosophie*, Vandenhoeck & Ruprecht, Göttingen.

Poincaré, Henri: 1902, 'Review of Hilbert's *Grundlagen der Geometrie*', original French in *Bulletin des Sciences Mathématiques* **26**, 249–272; English translation in Philip Ehrlich (ed.), *Real Numbers, Generalizations of the Reals, and Theories of Continua*, Kluwer, Dordrecht, 1994, pp. 147–168.

Russell, Bertrand: 1938, *The Principles of Mathematics*, 2nd ed. (1st ed. 1903), George Allen & Unwin, London.

Sinaçeur, Hourya: 1993, 'Du formalisme à la constructivité: Le finitisme', *Revue Internationale de Philosophie* **47**, 251–83.

Tarski, Alfred: 1956, *Logic, Semantics, Metamathematics,* Clarendon Press, Oxford.
Toepell, M.-M.: 1986a, *Über die Entstehung von David Hilbert's* 'Grundlagen der Geometrie', Vandenhoeck & Ruprecht, Göttingen.
Toepell, M.-M.: 1986b, 'On the Origins of David Hilbert's "Grundlagen der Geometrie" ', *Archive for the History of Exact Sciences* **35**, 329–44.
Torretti, Roberto: 1978, *Philosophy of Mathematics from Riemann to Poincaré,* D. Reidel, Dordrecht.
Walkoe, W. Jr.: 1970, 'Finite Partially Ordered Quantification', *Journal of Symbolic Logic* **35**, 535–55.

Department of Philosophy
Boston University
Boston, MA 02215
USA
hintikka@bv.edu