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On a Symposium on the Foundations of Mathematics (1971)

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(Zum Symposium über die Grundlagen der Mathematik)

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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*, which was initiated by Erwin Engeler 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.

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 "*Das Continuum*". 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.

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 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,” 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.[1]

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 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

[1] [*Editorial note: the term "" is not translated uniformly in this passage.*]

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."

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; 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 "*Über die Unabhängigkeit der Kontinuumshypothese*" "On the independence of the continuum hypothesis"] 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 theory" 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 open-mindedness 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.

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,[2] who in connection with it found the "independent"[3] theory of ordinal numbers (at about the same time as Zermelo and von Neuman.

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 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

[2]The condition expressed by this axiom is of course already implicitly contained in the intuitive notion of set.

[3]so-called because the ordinal numbers do not need to be introduced as abstraction classes of similar well-ordered sets.

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 *Grundzğe der Mengenlehre* 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*."

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. 101–2): 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 mathematical 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".

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.[4]

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

[4]The mapping need not be *onto* B. Thus the codomain B need not coincide with the set of values of f.

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 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." 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*.

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.[5] 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.

[5] 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.

The formal systems all have in common a number theoretic character, which derives from their recursive construction. This is particularly evident 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.[6] 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.

[6]It may be remarked by the way that the more general method of Gödel numbering, as applied in *Grundlagen der Mathematik II*, 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."

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.

This question is taken up by Marian Boykan Pour-El in her symposium contribution "A recursion-theoretic view of axiomatizable theories," 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.[7]

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

[7] [*Editorial note: The title* "Frau" *has been omitted*]

or constants, is said to be *satisfied* by a substitution of each of the free variables (occurring as arguments) by [names for] elements of the domain of 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 $\bigvee x A(x)$ (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" 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 *po ints 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 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 "*Vom Unendlichen zum Endlichen*" ["From infinite to finite"] 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.[8] 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 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

[8]Such a verification could more plausibly begin with the recently published treatise "Contradiction within pure number theory because of a system-internal 'Consistency'-deduction" (*International Logic Review*, Bologna, N. 9, June 1974 [vol. V, n. 1], pp. 51–62).

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 "Vom Unendlichen zum Endlichen". At some points it sounds as though he believes his results are in opposition with that theorem.[9]

In fact, Gödel's proof of the incompleteness theorem is not indirect.[10] 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

[9]Cf. the remarks in *Dialectica* 24, 4 p. 315, l. 14–16 and p. 316, l. 22-24.

[10]A proof is said to be indirect if an assumption is made, which in the course of the proof is shown to be incorrect.

its expressiveness, deductive strength, and strictly formal character. The requirement that the system F is consistent, on the other hand, is initially not 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.[11]

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

[11] Such geometries have been considered by G. Järnefelt, P. Kustaanheimo, and B. Qvist.

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.