SOME REMARKS ON THE FOUNDATION OF SET THEORY

TH. SKOLEM

To begin with I would like to express my best thanks to the chairman of this section, Professor Tarski, for the kindness and the great honour he has shown me by allowing me to give an address to you on the present occasion. I accepted the invitation only reluctantly, because I was afraid that it was too great an honour. My fear in this respect was chiefly due to the fact that in recent years I have worked more as an ordinary mathematician than as a logician. But nevertheless I decided to deliver this address, because I should really like to make some remarks concerning the logical foundation of mathematics even if they scarcely contain any real novelty. But my points of view are not quite in accordance with the current ones I think. In the last part of my lecture I shall indeed express a view of the logical development of mathematics that is perhaps rather subjective, but I think that there are good reasons to support it.

I shall make six different remarks. The first gives a unifying point of view for diverse possible set theories. The second deals with the axiom of infinity. The third remark concerns the illusory character of extensions of a set theory which is already sufficiently extensive. The fourth is a survey of the chief desires among people concerning the foundation of mathematics. The fifth remark concerns the ramified theory of types. The sixth remark is a proposal to make a serious attempt to build up mathematics in a strictly finitary way.

1. My first remark deals in the first instance with the so-called naive reasoning with sets. This reasoning is very clearly exposed in Dedekind: "Was sind und was sollen die Zahlen". Let us call this the first set theory, abbreviated FST. If we should formalize FST—which by the way we know is inconsistent but of this let us for the moment pretend ignorance—, it would have to be done as follows: We start with the fundamental relation $x \in y$, x is an element of y, which is a propositional function of two variables which run through all elements of the considered domain, D. From this propositional function others are built up using the operations of the lower predicate calculus. Let P be the totality of these propositional functions. We build expressions of the form $\hat{x}A(x)$ meaning "the class of all the x for which $A(x)$ is valid", where $A(x)$ is a propositional function containing x as a free variable. If x is the only free variable, $\hat{x}A(x)$ is called a set. Otherwise $\hat{x}A(x)$ is a set function of the other free variables in $A(x)$. In FST every set is again an element of D. We have the equivalence

$$(1) \qquad (y \in \hat{x}A(x)) \rightleftarrows A(y).$$

Further the identity of sets is defined thus

$$(2) \qquad (x = y) \rightleftarrows (z)((z \in x) \rightleftarrows (z \in y)),$$

and we have the axiom

$$(3) \qquad (x = y) \rightarrow (z)((x \in z) \rightarrow (y \in z)).$$

Now I assert that in FST all definable sets will be given by the expression $\hat{x}X(x)$, if here X runs through all propositional functions in P. To prove this I shall first show that every propositional function which can be built when we use the symbols $\hat{x}A(x)$ is always equivalent to some function belonging to P, or in other words: The symbols $\hat{x}A(x)$ can be eliminated. Indeed besides (1) we have

$$(4) \qquad (\hat{x}A(x) \in y) \rightleftarrows (z)((u)((u \in z) \rightleftarrows A(u)) \rightarrow (z \in y))$$

and

$$(5) \qquad (\hat{x}A(x) \in \hat{y}B(y)) \rightleftarrows (z)((u)((u \in z) \rightleftarrows A(u)) \rightarrow B(z)).$$

Hence every propositional function $\Phi(x, y, \cdots)$ which is built up by use of the set symbols is equivalent to a function $F(x, y, \cdots)$ in P. Therefore, if $\Phi(x)$ is an arbitrary propositional function, $\hat{x}\Phi(x) = \hat{x}F(x)$, where $F(x)$ belongs to P. Thus it is proved that all definable sets in FST are of the form $\hat{x}X(x)$, where X runs through all elements in P with one free variable.

Now, using the natural numbers, it is very easy to enumerate all elements of P. Hence it follows that it is possible to enumerate all definable sets in FST. The natural numbers can be defined as sets in D. The enumeration of the sets yields a class of ordered pairs (ν, n), ν running through all elements of D, n running through all natural numbers. This class is defined syntactically, i.e., in quite a new manner so that it cannot be said to be a set or, in other words, be an element of D. Hence all logically, not syntactically, definable sets in FST can be syntactically enumerated. If Dedekind, Cantor, and others had been aware of this fact, they might have wondered, and perhaps some doubt as to the transfinite of Cantor would have resulted.

We know now that FST is inconsistent, because, for example, Russell's antinomy can be deduced in it. However, it is quite clear that the syntactical enumerability will remain valid in every set-theory which arises from FST by restricted use of the requirement that the classes $\hat{x}X(x)$ shall be sets, i.e., again belong to the considered domain D as individuals therein. The characteristic feature of FST is the unrestricted requirement that every class of individuals in D is again an individual in D or, in other words: Every symbol $\hat{x}X(x)$ is one of the values the variable x can take. This demand is called impredicative, as well as every demand that a logical expression containing a variable shall be one of the values of the variable. Such impredicative principles can easily lead to antinomies. On the other hand it is certain that it is not necessary to avoid completely the impredicative demands in order to obtain a consistent theory. There may therefore be many consistent theories arising from FST by stronger or weaker restrictions in the use of the expressions $\hat{x}A(x)$ as being both individuals and sets of individuals. The strongest restriction would be to declare that the classes $\hat{x}A(x)$ are altogether new objects, not belonging to the original domain D. Such is the case in the theory of types. Between FST and STT, meaning the simple theory of types, we can imagine a great variety of set theories differing from one another in as far as some classes $\hat{x}A(x)$ are still elements of D and are called sets, whereas the remaining classes do not belong to D. Naturally there will be other differences, too. When the classes mostly belong to D, it will perhaps be sufficient to take only the elements of D into account; whereas when the classes do not or only to a small extent do belong to D, it will be necessary to take into account also the classes not belonging to D and even perhaps the classes of classes and so on. We obtain in this way a certain survey of the logical systems which can be used.

2. My second remark concerns the best known axiomatic theory of sets, namely Zermelo's. In his formulation there was an obscure point, the notion "definite Aussage". An improvement was given by A. Fraenkel, and independently I gave a precise definition of "definite Aussage" in an address at the Fifth Congress of Scandinavian Mathematicians in Helsinki in 1922. I identify "definite Aussage" and "propositional function belonging to P" as just explained. This definition will be convenient for my considerations now. It is easy to see that all the axioms of Zermelo except the axioms of infinity and of choice can immediately be formulated either in the demand that $\hat{x}A(x)$ shall belong to the considered set theoretic domain D, where $A(x)$ is a certain propositional function, or in the demand $(\hat{x}A(x) \in D) \rightarrow (\hat{x}B(x) \in D)$, $B(x)$ denoting a propositional function. For example the existence of the null set in D means that

$$\hat{x}((x \in x) \ \& \ (x \notin x))$$

belongs to D, x having D as its range of variation. Similarly the existence of the "elementary" sets $\{m\}$ and $\{m, n\}$ can be formulated. The axiom of separation takes the pretty form

$$(\hat{x}A(x) \in D) \rightarrow (\hat{x}(A(x) \ \& \ B(x)) \in D).$$

The axiom stating the existence of union is

$$(\hat{x}A(x) \in D) \rightarrow (\hat{y}(Ez)((y \in z) \ \& \ A(z)) \in D).$$

The axiom asserting the existence of the power set, i.e., set of all subsets, is

$$(\hat{x}A(x) \in D) \rightarrow (\hat{y}(x)((x \in y) \rightarrow A(x)) \in D).$$

The "Ersetzungsaxiom", or axiom of replacement, does not belong to Zermelo's axioms, but may be added with advantage. It is of the form $(\hat{x}A(x) \in D) \rightarrow (\hat{x}B(x) \in D)$. It may be stated in the form:

$$(\hat{x}A(x) \in D) \rightarrow \hat{x}(Ey)(F(x, y) \ \& \ A(y) \ \& \ (z)(F(z, y) \rightarrow (z = x))) \in D.$$

I leave the axiom of choice quite out of account, because it is of a different character from the other axioms. But the axiom of infinity can be put in the form $\hat{x}A(x) \in D$ for a certain $A(x)$ in P. This is not so in the ordinary formulation, namely: There exists a set Z containing 0 as one of its elements, and whenever $x \in Z$, we have $\{x\} \in Z$. It is evident that this axiom is not of the form $\hat{x}A(x) \in D$, because an axiom of the latter kind uniquely determines a set relative to D. I should like to show, however, that an improvement is possible here. The previous axioms yield among others such sets as

$$\{0\}, \quad \{0, \{0\}\}, \quad \{0, \{0\}, \{\{0\}\}\}, \cdots.$$

We can call an element b of the set m an i-element (initial element) of m, if no element c of m exists such that $b = \{c\}$. Similarly we call $s \in m$ an f-element (final element) of m, if $\{s\}$ is not $\in m$. Some sets m (see above) will have the following property:

1) $0 \in m$; 2) m contains a single f-element; 3) every subset of m contains an f-element; 4) every subset of m containing 0 as element and containing the same single f-element as m is identical with m. This property can be expressed as a propositional function $I(m)$ belonging to P. Then the axiom of infinity can be written in the form

$$\hat{m}I(m) \in D.$$

Indeed the elements of $\hat{m}I(m)$, i.e., the sets having the property $I(m)$, are the sets $\{0\}$, $\{0, \{0\}\}$, $\{0, \{0\}, \{\{0\}\}\}$, $\cdots$ ad inf. However, it may be preferred to introduce the Zermelo number series $\{0, \{0\}, \{\{0\}\}, \cdots\}$. This is the union

$$\hat{n}(Em)((n \in m) \,\&\, I(m)).$$

I do not think this is the right occasion to go into details of a proof of this. I shall mention only that it can be proved by the aid of the following lemmas:

1) $I(m) \,\&\, (m_1 \subset m) \,\&\, (0 \in m_1) \,\&\, (m_1$ containing a single f-element$) \rightarrow I(m_1)$.
2) $I(m) \rightarrow (m$ contains no i-element $\neq 0)$.
3) $I(m_1) \,\&\, I(m_2) \rightarrow (m_1 \subset m_2) \vee (m_2 \subset m_1)$.
4) $I(m) \,\&\, (s$ is f-element of $m) \rightarrow I(m + \{s\})$.
5) $(0 \in n) \,\&\, (n$ contains no f-element$) \,\&\, I(m) \rightarrow (m \subset n)$.

The proofs of these lemmas are straightforward.

It would seem possible to introduce infinite sets in easier ways. We could for example set up the axiom

$$\hat{x}(Ey)(x = \{y\})) \in D.$$

The set thus introduced would contain $\{0\}$, $\{\{0\}\}$, $\cdots$ ad inf. as elements, but it is an element of an element of itself, and it would be possible to deduce a contradiction.

As I already said above, we can have many different set theories: from FST at one extremity to the type theories, especially STT, the simple type theory, and RTT, the ramified type theory, at the opposite extremity. Somewhere between we have ZST, meaning Zermelo's set theory without the axiom of choice, but with addition of the axiom of replacement.

As to STT a reinterpretation is possible which shows that it is a weakened form of ZST. For greater simplicity I shall take the part of STT we obtain when apart from the $\in$-relation only relations $R(x, y)$, $R(x, y, z)$, $\cdots$ are allowed, where x, y, z, $\cdots$ are all of the same type. Then we can conceive the ordered pair (x, y) as the set $\{\{x, y\}, \{x\}\}$, the ordered triplet as $\{\{x, y, z\}, \{x, y\}, \{x\}\}$, and so on. The advantage of this is that we can conceive the relations also as sets. Then in the domain D we must suppose given the fundamental relations $\in$, the identity relation $=$, and the equivalence relation $\sim$ meaning "of the same type as", together with the usual axioms for the identity and for this special equivalence relation. Further it is assumed that some of the individuals in D have no elements, i.e., if a is such an element, we have in D, $(x)(x \mathrel{\overline{\in}} a)$. Then the following axioms shall be valid:

$$(x)(x \mathrel{\overline{\in}} a) \;\&\; (y)(y \mathrel{\overline{\in}} b) \rightarrow (a \sim b), \qquad (x)(x \mathrel{\overline{\in}} a) \;\&\; (Ey)(y \in b) \rightarrow (a \not\sim b)$$

$$(a \in m) \;\&\; (b \in n) \rightarrow ((a \sim b) \rightleftarrows (m \sim n))$$

$$(Ex)((x \sim a) \;\&\; A(x)) \rightarrow (\hat{x}((x \sim a) \;\&\; A(x)) \in D).$$

Here $A(x)$ means an arbitrary propositional function which can be derived from $\in$, $=$, and $\sim$ by the lower predicate calculus. All this still leaves undetermined the set of all individuals without elements. To make the development of mathematics possible, it is then suitable to let this be the set N of natural numbers, setting up the Peano axioms.

However, the relation $\sim$ can be omitted without destroying the deducibility of ordinary mathematics. Indeed, it will be sufficient to start with the natural number series N characterized by the Peano axioms and then presuppose the following two axioms:

$$(Ex)((x \in a) \;\&\; A(x)) \rightarrow \hat{x}((x \in a) \;\&\; A(x)) \in D.$$

$$\hat{x}((Ey)(y \in x) \;\&\; (y)((y \in x) \rightarrow (y \in a)) \in D.$$

A void class is here never a set. I scarcely believe that a simpler formal theory, equivalent to the STT with the axiom of infinity for its original individuals, is possible. It is easily seen that any set which can be constructed by the two axioms has only elements of the same type, if we define the type of an arbitrary individual a in D as meaning the number of terms in a decreasing $\in$ series starting from a and ending with a_ν, ν a natural number, viz.,

$$a_\nu \in \cdots \in a_2 \in a_1 \in a.$$

I must be content with this hint.

3. Now I come to my third remark. Let us consider the ZST. Let Z denote Zermelo's number series, and let Z^2 be the set of all ordered pairs from it. Then assuming the consistency of ZST, it can be proved that a subset S of Z^2 must be definable such that the axioms of the domain D will be valid for S, if every-where $(x, y) \in S$ is written instead of $x \in y$. More generally, if A is a consistent system of axioms for a set theoretic domain D_A , there exists a definable subset S_A of Z^2 for which A is valid in the same sense. Whether all this is important or not will of course depend on the answer to the question whether ZST is consistent or not. If ZST is consistent, the mentioned theorem shows that it is in a certain sense logically closed.

Because of the uncertainty with regard to consistency, I shall on this occasion be content with giving the hint concerning the proof that it is only a proof of the theorem of Löwenheim adapted to ZST. The essential thing is that a *set* S can be found satisfying the axioms A in the mentioned sense. That a subclass of D could have this property without being a set would not be so astonishing.

4. My fourth remark refers to the question: Which one of the different theories shall we prefer? That depends on the desires we have in the foundation of mathematics. There are, I believe, at present chiefly three different sorts of desire.

1) One desires only to have a foundation which makes it possible to develop present day mathematics, and which is consistent so far as is known yet. Should any contradiction occur, we may try to make such restrictions in the underlying postulates that the deduction of the contradiction proves impossible. This may perhaps be called the opportunistic standpoint. It is a very practical one. See for example N. Bourbaki, J. Symbolic Logic vol. 14 pp. 1–18. He says: "Let the rules be so formulated, the definitions so laid out, that every contradiction may most easily be traced back to its cause, and the latter either removed or so surrounded by warning signs as to prevent serious trouble". But this standpoint has the unpleasant feature that we can never know when we have finished the foundation of mathematics. We are not only adding new floors at the top of our building, but from time to time it may be necessary to make changes in the basis.

If one agrees in this standpoint, the best thing to do will be to use STT or the axiomatic set theory as it is proposed by Zermelo, Fraenkel, or von Neumann, because this will be most convenient for the development of present day mathematics.

2) One desires to obtain a way of reasoning which is logically correct so that it is clear and certain in advance that contradictions will never occur, and what we prove are truths in some sense. This standpoint might be called the natural one or perhaps the logicistic one. Indeed, it was the generally assumed point of view before the discovery of the set theoretic antinomies. These, however, scattered the conviction that it was possible to find logical principles which were reliable. But, certainly, the mistake that the naive set theory was reliable does not prove that it should not be possible to detect the error in the classical set theoretic thinking and perhaps formulate a really correct reasoning. It was emphasized by Poincaré that the error in the classic set theory was the impredicative definitions. B. Russell has agreed in this and to avoid the impredica-

tive reasoning he invented the theory of types. But there are different type theories. Especially well known are the STT and the RTT. Of these two, only RTT is really free from impredicative notions. Therefore it seems to me that if we will put ourselves on the natural standpoint, and if we believe that the impredicative definitions constitute the error in classical set theory, the only thing to do is to use the RTT. Now most logicians prefer STT. We can use STT putting ourselves on the opportunistic standpoint, but then also the ZST could be convenient. From the natural point of view we have to choose RTT. Only then could we be justified in believing that our reasoning is logically sound. Now Professor F. B. Fitch has given a consistency proof of RTT or perhaps rather a certain modification of it. (J. Symbolic Logic vol. 3 pp. 140–149). However, taking the natural standpoint we cannot need any such proof, because when we are sure that our reasoning is correct, we are also sure in advance that paradoxes cannot occur. But of course it may be doubted, if there are not other weak points in the classic reasoning than the impredicative definitions. Indeed the use of the unrestricted quantifiers may be a weak point. I shall return to this.

3) The Hilbert program. This is the result of the giving up of the logicistic standpoint and not being content with the opportunistic one. We may distinguish the original and the modified Hilbert program. According to both of them we have to formalize mathematics and then prove that the formalism is consistent. Such a proof requires the proof of numerically general theorems, viz., theorems valid for an arbitrary number of applications of axioms or rules of procedure. To prove numerically general theorems we must at least use ordinary complete induction. Now the original Hilbert program takes into account only what is called finitary reasoning, and the ordinary complete induction in the intuitive sense belongs hereto. However a result of Gödel is known showing that this sort of reasoning is not sufficient to enable us to prove the consistency of the usual formal systems of mathematics. Thus it is well known that the proof of the consistency of number theory (say, the system Z in Hilbert-Bernays, *Grundlagen der Mathematik* vol. 1 p. 371) can be performed only by use of a certain form of transfinite induction, i.e., a higher form of induction than that occurring in number theory. If we would formalize a theory containing this transfinite induction, we could again prove the consistency of the latter theory only by using a still higher form of induction, and so on. This is as if we should hang up the ground floor of a building to the first floor, this again to the second floor, etc. I cannot understand the enthusiasm with which these ideas have been met among so many mathematicians. To me it seems that a more natural foundation ought to be tried, and again I think that it is the use of the unrestricted quantifiers which makes all the trouble.

5. My fifth remark concerns mostly RTT. As I just explained, one should from the logicistic standpoint have strong reason to be interested in RTT. However it has not been treated very much. Apart from some studies of Chwistek, which I must confess I have never read, the previously mentioned paper by F. B. Fitch, and the second edition of *Principia Mathematica*, there seems only little to be found in the literature concerning RTT. I have been somewhat astonished by the general lack of interest in this theory. In the book *Theoretische Logik*, 2d ed., by Hilbert and Ackermann it is said on p. 122 that the ramified theory of types is unnecessary, and that there is no reason to take it into account. Further it is said that the STT is proved consistent. However, as far as I know, the consistency proof for STT has been set up only when the number of individuals of the lowest type is finite, and this is insufficient for mathematics. The necessity of an axiom of infinity is a weakness of both STT and RTT, because a logical reason for it can scarcely be found. I had many years ago made an attempt to base arithmetic on RTT but did not succeed very well at that time. Recently I tried again with better results. These investigations have not been published. In his review of Fitch's paper Professor Bernays also asserts that arithmetic can be developed within RTT. This utterance of Bernays makes it doubtful whether I shall continue my investigations, because the matter is perhaps sufficiently treated already. As to the second edition of *Principia Mathematica*, I have seen only the first volume of it. It is well known that traditional analysis in its whole extent cannot be founded on RTT, but a great part of it remains valid. We have surely no right to condemn RTT, because it does not yield the whole of ordinary analysis. We ought not to regard all that is written in the traditional textbooks as something sacred.

6. Now I come to my sixth and last remark which I shall emphasize most of all. As I already said, the difficult thing in the logical development of mathematics is the use of quantifiers. However, it is to a great extent possible to develop mathematics without use of quantifiers. Indeed, I showed in a paper published in 1923 (Skrifter Norske Videnskaps-akademi, Oslo. 1923, I, no. 6b.) that ordinary arithmetic could be established by the aid of definition by recursion and proof by complete induction without use of quantifiers. By the way, it can be remarked that the use of restricted quantifiers, i.e., restricted to a finite range of variation, does not matter. In the same paper I also gave a hint of the fact that arbitrary arithmetical functions could be treated in the same way, and this makes also a sort of analysis possible. Indeed, the treatment of arbitrary functions or arbitrary series of integers according to these principles will be almost the same thing as the theory of progressions of choice, in the German of Brouwer's terminology "Wahlfolgen". Apart from my own paper which I wrote 30 years ago I have found this kind of mathematics treated in only two places, namely Hilbert-Bernays, *Grundlagen der Mathematik* vol. 1 pp. 286–346, and in a paper by H. B. Curry, *A formalization of recursive arithmetic*, Amer. J. Math. vol. 63 (1941) pp. 263–282.

Further, this finitistic way of foundation has been briefly mentioned by the Hungarian logician R. Péter in one of her articles *Über den Zusammenhang der verschiedenen Begriffe der rekursiven Function*, Math. Ann. vol. 110 pp. 612–632. But on the whole there has been among logicians a conspicuous lack of interest in it, which has been a disappointment to me. When I wrote my article I hoped that the very natural feature of my considerations would convince people that this finististic treatment of mathematics was not only a possible one but *the* true or correct one—at least for arithmetic. Now I can well understand that the lack of interest in the finitism is due to the circumstance that most logicians and mathematicians do not believe that it will be sufficient for mathematics. Of course this is also true, if mathematics shall mean present day mathematics altogether, as we find it in textbooks and scientific journals. It is trivial to say that present day mathematics, which is partially built up by the aid of transfinite ideas, cannot without change be based on finitary reasoning. The question is, however, what we shall lose or gain by such a change. As to clearness and security we certainly can only gain much. As pointed out in Hilbert-Bernays, *Grundlagen der Mathematik* vol. 1, the reasoning without quantifiers only yields propositions which are true in the sense of being finitarily interpretable and verifiable. As a special consequence of this no inconsistency can occur. However, it can be asked whether the change will not have the effect that we get a form of analysis which is more cumbersome and less effective than the classic one. This is generally believed to be an inevitable effect, I think. However, I should like to express some doubt in this respect. Indeed, we are certainly too much bound by tradition in considering the possibilities of establishing mathematical theories. That this is true we have had an excellent example of quite recently. In the theory of numbers we have a famous theorem called the prime-number theorem asserting that the quotient between $x/\log x$ and the number of primes up to x approaches 1 when x tends to ∞. It was maintained by the greatest experts in this field, E. Landau and G. H. Hardy, that an elementary proof of this theorem, i.e., a proof without use of the theory of analytic functions, was impossible. Quite recently, however, A. Selberg and P. Erdös established such an elementary proof. This urges us to be careful not to believe that what has been done before and has become classical mathematics is the only possible thing. I think that the fear that mathematics will be crippled by the restriction to the use of only free logical variables is exaggerated. I am aware that it may look different to mathematicians accustomed to analysis—to the theory of functions say—and those only working in the theory of numbers, but there are certainly many more ways of treating mathematics than we know today.

Now I will not be misunderstood. I am no fanatic, and it is not my intention to condemn the nonfinitistic ideas and methods. But I should like to emphasize that the finitistic development of mathematics as far as it may be carried out has a very great advantage with regard to clearness and security. Further it may be good reason to conjecture that it can be carried out very far, if one would make serious attempts in that direction. I would also like to say that the more importance we can attach to the purely finitistic development of mathematics, the less we need to be worried by the difficult consistency proofs of logical systems containing quantifiers. The great ingenuity in the setting up of these consistency proofs, especially the proof by Gentzen and Ackermann of the consistency of formalized number theory, must be admired. But it has happened before that products of great ingenuity have lost their interest, because simpler ways of thinking have been found. Another thing which can properly be mentioned in this connection is the general lack of interest among ordinary mathematicians with regard to symbolic logic. This is I think mostly due to the circumstance that the mathematicians believe—or have an instinctive feeling—that a very great part of the problems treated in symbolic logic are rather irrelevant to mathematics. As to the proof of the consistency of number theory, for example, there is a characteristic of which Gentzen himself is well aware, expressing it as follows: (*Die Widerspruchsfreiheit der reinen Zahlentheorie*, Math. Ann. vol. 112 p. 533) "The problem to prove the consistency of Number Theory is more a foundation of *possible* conclusions than of *actually used* ones". Of course mathematicians will be content if the conclusions actually used are tenable.

MATEMATISKE INSTITUT,
OSLO, NORWAY.