Chapter 6

Developments in the Foundations of Mathematics, 1870–1910

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6.1. Introduction

This chapter deals with some of the most important work in foundations from the time of the investigations into the foundations of analysis in the early 1870s up to the publication in 1910–1913 of Whitehead and Russell's *Principia mathematica*. My treatment of the foundations of analysis is limited to the work of Richard Dedekind, for Cantor's contribution has already been covered in chapter 5; moreover, Dedekind is most suitable for my purposes because his motivation is most explicit. In general, I have restricted myself to positive contributions to the foundations of mathematics, and have not tried to include the criticism and polemic which is especially prominent in the writings of Frege, Poincaré and Brouwer. This in part explains why there is not a separate section on intuitionism. Brouwer's early writings (1907–1914) on intuitionism are largely critical; the real development of intuitionistic mathematics does not occur until 1918. For readers who would like to supplement this chapter—which concentrates on the classical foundations of Dedekind, Frege, Russell and Zermelo—with an account of Brouwer's intuitionistic mathematics, I recommend chapter 5 of E. W. Beth's *Mathematical thought* (*1965a*).

The most prominent feature of the period under consideration here is the tendency, culminating in *Principia mathematica*, towards the logical systematisation of mathematics, and replacement of so-called 'intuitive' explanations and arguments pertaining to the elements of mathematics by formal proofs based on logically precise definitions or systems of axioms. The first area of mathematics to be so reconstructed was mathematical analysis. As far back as 1858 Dedekind had been motivated to improve matters in the foundations of the calculus by his first experience with teaching differential calculus. This made him

feel 'more keenly than ever before the lack of a really scientific foundation for arithmetic' (*1872a*, preface). Although he had worked out his basic definitions already in 1858, his pamphlet *Stetigkeit und irrationalen Zahlen* ('Continuity and irrational numbers') was not published until 1872. Even before this work appeared, he had a plan for a similar treatment of the foundations of the theory of natural numbers, which was eventually realised as *1888a* in his *Was sind und was sollen die Zahlen?* (known in English as 'The nature and meaning of numbers'). In *1889a*, art. 1 Giuseppe Peano presented independently a system of axioms for the theory of natural numbers which is very closely related to Dedekind's basic definition. Earlier, Gottlob Frege had published a penetrating investigation *1884a* of the general concept of cardinal number and of the finite cardinals.

Dedekind had intended to present the whole subject of natural, negative, rational, irrational and complex numbers 'in a systematic form', but he never achieved it. A very comprehensive systematic presentation of logic and mathematics was, however, effected by Peano and his school, and an even more thorough-going exposition of a system of logic and arithmetic was given by Frege in his *Grundgesetze der Arithmetik* ('Foundations of arithmetic'), which was published in two volumes as *1893a* and *1903a*. Frege's work, though it dealt with a much less extensive part of mathematics, was executed with remarkable attention to detail and an exactness of expression which goes far beyond what was achieved by Peano or anyone else. For example, he was especially clear on the distinctions between the use and mention of expressions and between logical theses and rules of inference.

The foundations (in some cases only implicit) of the works of Dedekind, Cantor, Frege and Peano turned out to be inconsistent, as was shown by the antinomies (especially Russell's). These antinomies elicited a variety of responses from mathematicians and logicians interested in foundations. One reaction was the determination to find a system for avoiding the antinomies which would include as much as possible of the results obtained before their appearance. There are a number of methods of avoiding the antinomies, because there are a number of characteristics common to the paradoxical cases. Depending upon which characteristic is chosen as a basis for avoiding the antinomies and what supplementary assumptions (such as Russell's axiom of reducibility) are made, systems comprehending various portions of the classical results (perhaps in a somewhat modified form) are obtained. In some cases—especially Russell's—there has been an attempt to formulate a system which not only avoids the antinomies but also eliminates the precise fallacy to which they are due. It could, thus, count as a *philosophical* solution to the antinomies.

6.2. *Dedekind on continuity and the existence of limits*

I shall begin my more detailed discussions with the work of Dedekind. Like many other mathematicians of the mid-19th century, Dedekind was dissatisfied with the foundations which had so far been provided for the calculus and for the arithmetic of real numbers. The primary motivation behind his 'Continuity and irrational numbers' was the desire to replace undefined concepts (more or less geometrical) and the so-called intuitive justifications based upon them with proofs from precisely formulated definitions. Above all, he wished to find definitions from which the basic theorems on the existence of limits could be proved. To accomplish this he needed to define a system having a certain sort of completeness or continuity. The term 'continuous' is not an especially apt one for the characteristic involved, but it indicated the correlate in the old system—continuous magnitude. It had been the practice to present the calculus as being concerned with continuous magnitudes. But this property of continuity, which was attributed to such things as line-segments and motions, was not defined—at least not in a way which could serve as a basis for proofs. As Dedekind put it: 'By vague remarks upon the unbroken connection in the smallest parts obviously nothing is gained; the problem is to indicate a precise characteristic that can serve as the basis for valid deductions' (*1872a*, pt. 3; *1901a*, 10).

The sort of complete system Dedekind needed to define had to be a densely ordered system for which arithmetical operations could be defined and in which could be proved propositions such as that every element of the system has a square root. This completeness of arithmetical operations may be described by saying that the system must be closed under arithmetical operations satisfying the laws of elementary algebra. Furthermore, the system must be complete with respect to limits; that is, every convergent sequence of its elements must have a limit.

The property of completeness or continuity which Dedekind found satisfactory for his purposes is a characteristic of ordered systems. A *cut* in an ordered system M is a pair of classes M_1, M_2, called the 'lower' and 'upper' sections of the cut, which together exhaust M and are such that every element of M_1 precedes every element of M_2. Then a densely ordered system is *complete* (*continuous*) in Dedekind's sense if every cut in the system is produced by exactly one element of the system, that is, if there is an element of the system which is either the maximum of the lower section or the minimum of the upper section.

Dedekind seems to have thought of this definition by reflecting on lines, and contrasting the system of points on a line with the system of rational numbers. But he was not willing to settle for an axiomatisation

of the concept of magnitude, which would feature an axiom of continuity. Geometry was to serve only as the source of the idea for constructing an arithmetical foundation. The continuous system which was to be Dedekind's foundation would be arithmetical in the sense that its operations would ultimately be defined in terms of operations on natural numbers, and no mention would be made of any geometrical objects. Thus, besides a basis for proofs, Dedekind also sought a purely arithmetical foundation for the calculus.

A demonstrably complete (continuous) domain was defined by Dedekind in terms of the cuts in the rationals. This domain, called 'the system of real numbers', is to contain all rational numbers, and, in addition, for each cut in the rationals which is not produced by a rational, exactly one new object, which is called an irrational number. By reference to the system of cuts, Dedekind defined among the real numbers a relation which could be proved to have the properties of a dense ordering. Furthermore, and this is the main point, it can be proved from the definition of the real numbers and the definition of '$<$' that the system of real numbers is complete: every cut in it is produced by one of its elements, which is to say that for every cut in the system of real numbers, either its lower section has a maximum or its upper a minimum. The fundamental theorems on limits follow from this property of completeness. In particular, he proved (1) that every bounded increasing sequence of real numbers has a limit, and (2) that a function f whose arguments and values are real numbers has a limit when $x \to \infty$, if for every positive δ there is an x_0 such that $|f(x)-f(x_0)| < \delta$ for all $x > x_0$.

Arithmetical operations (addition, multiplication, and so on) may be defined in terms of the cuts in the system of rationals and the corresponding arithmetical operations defined for the rationals. The operations on rationals can in turn be defined in terms of the operations between natural numbers. From the definitions of the operations on the real numbers such algebraic laws as

$$a+b=b+a \quad \text{and} \quad a(b+c)=ab+ac \tag{6.2.1}$$

can be proved. It also becomes possible to demonstrate equations like $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$, which so far as Dedekind knew had 'never been established before'. Although the difficulties concerning existence theorems for limits were fairly well recognised, some mathematicians were reluctant to admit that the rule for multiplying square roots had never really been proved. For instance, R. Lipschitz maintained in correspondence with Dedekind that the basis for the proof of the theorem in question was already in Euclid's *Elements*. Dedekind's reply was that, even setting aside the desire to avoid a geometrical foundation of arithmetic, no axiom of completeness for the domain of magnitudes is to be found in Euclid. But no generally valid definition of, for example, multiplication is possible for an incomplete domain, because for any two quantities of such a domain it may be that *no* quantity of the domain is the product of the two quantities. 'If, to be sure, general definitions of addition, subtraction, multiplication, and division are relinquished, one only needs to say: I understand by the product $\sqrt{2} \cdot \sqrt{3}$ the number $\sqrt{6}$, consequently $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$, which was to be proved' (see Dedekind *Works*, vol. 3, 474).

Many authors who adopted Dedekind's basic ideas preferred not to follow him in defining the real numbers as creations of the mind corresponding to cuts in the system of rational numbers. In *The principles of mathematics* (*1903a*) Bertrand Russell emphasised the advantage of defining the real numbers simply as lower sections of cuts whose upper sections have a minimum or, as he said, segments of the rationals. A *segment* of the rationals is a non-empty proper subclass of the rationals having the property of being identical 'with the class of rationals x such that there is a rational y of the said class such that x is less than y' (Russell *1903a*, 271). Russell needed to use a definition like this because it was his aim to define all mathematical concepts in terms of the vocabulary of logic and the theories of classes and relations. But Dedekind had his reasons (though not necessarily good ones) for defining the real numbers as he did. When Heinrich Weber expressed his opinion in a letter to Dedekind that an irrational number should be taken to be the cut, instead of something new which is created by the mind and supposed to correspond to the cut, Dedekind replied (*Works*, vol. 3, 489):

> We have the right to grant to ourselves such a creative power, and besides it is much more appropriate to proceed thus because of the similarity of all numbers. The rational numbers surely also produce cuts, but I will certainly not give out the rational number as identical with the cut generated by it; and also by introduction of the irrational numbers, one will often speak of cut-phenomena with such expressions, granting to them such attributes, which applied to the numbers themselves would sound quite strange.

But of course Dedekind's method of introducing the irrationals was only a matter of preference, and in a letter to Lipschitz he remarked parenthetically that if one does not wish to introduce numbers in his sense, 'I have nothing against it; the theorem I prove [on completeness] then reads: the system of all cuts in the discontinuous domain of rational numbers forms a continuous manifold' (*ibid.*, 471).

Although there were often different preferences and philosophical convictions among classical mathematicians, they were not such as to

lead to any essential differences in the mathematics produced. By contrast, the philosophical differences between classical and constructive mathematicians result in completely different mathematical theories. Let me illustrate the point. Some of the classical mathematicians were realists, and regarded their work as formulating truths describing the objective facts concerning such abstract entities as functions and sets which were taken as existing independently of thought. Others were idealists and considered mathematical systems to be created by the mind. Many constructivists were also idealists. But the idealism of a classical mathematician like Dedekind was much more similar to realism than to the idealism of a constructivist like Poincaré or Brouwer. In fact, the idealism sometimes advocated by classical mathematicians could very well be called 'quasi-realism'. For, although a mathematical structure is said to be created in the sense of being thought up, rather than discovered, it is conceived of as, so to speak, a system of simultaneously existing inter-related entities. But the constructive idealist conceives of his mathematical entities as things coming into being one after another, individually created. Thus the infinity which enters into constructive mathematics is only potential infinity, whereas the infinity treated in classical mathematics is always actual infinity.

While Dedekind's definition or ones serving the same purposes were quickly adopted by many mathematicians interested in greater rigour in foundations, Leopold Kronecker found such definitions completely unacceptable. He disapproved of definitions for which it is not decidable in the case of any entity whether it satisfies the definition or not (see his *1882a*, art. 4). But Dedekind and Cantor considered knowledge of a decision procedure for membership in a particular set to be of no account for their purposes; as Dedekind explained, 'the general laws to be developed in no way depend upon it; they hold under all circumstances' (*1888a*, art. 2, note; compare Cantor *Papers*, 150–151, and Frege *1884a*, art. 80). Kronecker also had the conviction that the infinite should not be introduced except in cases in which it could be eliminated (see *1886a*, 334–336; *Works*, vol. 3, 155–156), and maintained that the various concepts of irrational numbers are of the sort which 'must be avoided in arithmetic-algebraic theories'. Dedekind's reply was simply that the restrictions on concept formation advocated by Kronecker did not seem justified, but that there was no point in going into the matter further until Kronecker published his reasons (Dedekind *1888a*, art. 2, note). Unfortunately, Kronecker seems never to have published the desired reasons. While he had carried on vigorous debates with his colleague Weierstrass concerning the foundations of analysis, his writings contain only a few brief expressions of his opinions on the foundations of mathematics (compare sections 5.4 and 5.8 above).

It is interesting to consider the account given by Cantor of the point of view of those who disapprove of the method of introducing the irrational numbers by means of infinite sets or sequences. In his 1883 *Grundlagen* Cantor explained that those who reject this approach hold that 'a merely *formal* significance should belong to irrational numbers in pure mathematics, in that they serve as it were only as marks of computation for fixing and describing in a simple, uniform way properties of groups of whole numbers' (*Papers*, 172). The advantages of reducing the content of analysis to relations among finite integers are a greater security and completeness in its foundations as well as an improvement in its methodology. Hence (*Papers*, 173):

> In this way, a definite, even if rather prosaic and obvious principle is assumed, which is recommended to all as a guiding principle; it is supposed to serve for indicating the true limits for the flight of the desire for mathematical speculations and conceptions, where it runs no danger of falling into the abyss of the transcendent, where, as it is said with fear and holy dread, 'everything is possible'. Setting this aside, who knows if it has not been just the point of view of expediency alone which caused the authors of this opinion to recommend it to the powers, so easily endangered by enthusiasm and extravagance, as an effective regulative [principle], as a protection against all errors, although a fruitful principle cannot be found in it . . .

Cantor did not think that those who recommended the restrictive principles had made their discoveries by adhering to them. No true advances are due to the observation of such principles, and if science actually proceeded in accordance with them, it 'would be held back or still would be bound within the narrowest limits' (*ibid.*). He himself recognised only two restrictions on concept formation: the concepts must be consistent, and the new concepts must be related by definitions to concepts already recognised. He did not think that there was much danger to science in his point of view, because poor ideas would fade away; on the other hand, he did see a real threat to science in unnecessary restrictions (*Papers*, 182).

6.3. *Dedekind and Frege on natural numbers*

The desire to base arithmetic on a system of precisely formulated definitions motivated both Dedekind's and Frege's work on the foundations of the theory of natural numbers. Regarding what was perhaps the most significant point, the basis of inference by mathematical induction, their systems are the same. But Dedekind, unlike Frege, did not define a particular set of objects as the natural numbers. Rather, he defined a class of structures, which he called 'simply infinite systems', any one of which could serve as the subject of the arithmetic of natural numbers. Indeed, he understood the primary concern of arithmetic to be the 'relations or laws' derivable from the essential characteristics of simply infinite systems (*1888a*, art. 73). A class N is *simply infinite* if there is a one–one function ϕ mapping N into itself and an object b such that b is not a value of ϕ for an argument in N, and N is the intersection of all classes containing b and also $\phi(y)$ whenever they contain y. Thus the essential characteristics of a simply infinite system N are the following (Peano's axioms in *1889a* coincide with or are immediate consequences of them):[1]

$$(\forall y)(y \in N \rightarrow \phi(y) \in N), \tag{6.3.1}$$

$$N = \bigcap\{Z \mid b \in Z \,.\, \& \,.\, (\forall y)(y \in Z \rightarrow \phi(y) \in Z)\}, \tag{6.3.2}$$

$$(\forall y)(y \in N \rightarrow b \neq \phi(y)), \tag{6.3.3}$$

$$\phi \text{ is one–one.} \tag{6.3.4}$$

Dedekind said that the elements of a simply infinite system may be called 'numbers' if 'we entirely neglect the special character of the elements; simply retaining their distinguishability and taking into account only the relations to one another in which they are placed by the order-setting transformation ϕ' (*1888a*, art. 73). Thus it may be said that any simply infinite set could be defined to be the set of (natural) numbers.

The appropriateness of Dedekind's definition is easily perceived. A number series must have a first element, $b = 1$, from which the succession of numbers proceeds without end. That 1 is the first term of the succession of numbers means that 1 is a number which is not the successor of any number. The series proceeds without end in that every number has a successor. Moreover, each number has only one successor, that is, a successor relation is a function; and different natural numbers have different successors, so that the successor function is one–one. Thus we have the basic requisites concerning 1 and a successor function ϕ. But the most significant part of the definition of a natural number series (simply infinite system) N is the specification that N be the intersection of all classes Z which contain the 'base-element' 1 and which contain $\phi(x)$ whenever $x \in Z$ and ϕ has a value for x. The idea for the definition of a set of natural numbers N is that it should contain a base-element 1, the successor $\phi(1)$ of 1, and so on, and *no*

[1] Attention is recalled to the list of notations in section 0.5.

other elements, that is,

$$N=\{1, \phi(1), \phi(\phi(1)), \ldots\}. \qquad (6.3.5)$$

The means of defining N so as to exclude undesirable elements was, Dedekind says, 'one of the most difficult points of my analysis and its mastery required lengthy reflection' (letter to Keferstein, in van Heijenoort *1967a*, 100). For it cannot be said without 'the most pernicious and obvious kind of vicious circle' that n belongs to N if n is 1 or a value of ϕ after a *finite number* of iterations of ϕ beginning from 1. The method which Dedekind found sufficient for his purposes was to define N as the class which contains exactly those things which belong to *every* class Z which contains 1 and also all values of ϕ for arguments in Z.

This definition of the finite numbers has the following principle of mathematical induction as a consequence:

$$1\in M \,.\, \&\,.\,(\forall y)(y\in N\cap M\rightarrow \phi(y)\in M)\,.\rightarrow .\, N\subseteq M. \qquad (6.3.6)$$

In words, any class M containing 1 as well as the successor of any number belonging to it contains every number. Dedekind also proved a theorem (*1888a*, art. 126) justifying inductive or recursive definitions such as the following definition of addition:

$$x+1=\phi(x), \qquad (6.3.7)$$

$$x+\phi(y)=\phi(x+y). \qquad (6.3.8)$$

His theorem justified this definition in that it asserts that there exists one and only one function of two arguments which satisfies the conditions (6.3.7) and (6.3.8). In general, the theorem on definition by induction states that for any functions g and h there is one and only one function f such that

$$f(x, 1)=h(x) \qquad (6.3.9)$$

$$f(x, \phi(y))=g(f(x, y)) \qquad (6.3.10)$$

(*1888a*, arts. 126 and 135).

We have seen that Dedekind did not specify any particular set of things as the natural numbers, but he did prove the existence of simply infinite systems in order to show the consistency of his definition. Instead of the example of a system N, ϕ, b, which he actually used, we shall consider a more mathematical example suggested by his *1888a*, arts. 66 and 72. Let $\phi(x)=\{x\}$ and $b=\varnothing$. From these definitions it follows that ϕ is one–one and that b is not a value of ϕ. The class

$$N=\{\varnothing, \{\varnothing\}, \{\{\varnothing\}\}, \{\{\{\varnothing\}\}\}, \ldots\}, \qquad (6.3.11)$$

which is the intersection of all classes containing $\varnothing$ as well as the unit class of anything belonging to them, is simply infinite. This method of proving the existence of a simply infinite system is, however, affected by the antinomies (see the end of section 6.8 below).

Dedekind's treatment of the concept of the cardinal number of a set or the number (*Anzahl*) of things in a set was restricted to finite sets. He proved that for each finite set M there is a unique natural number n such that there is a one–one correspondence between M and the initial segment

$$Z_n \underset{\mathrm{Df}}{=} \{x \mid 1\leqslant x\leqslant n\} \qquad (6.3.12)$$

of the set of natural numbers (*1888a*, art. 160). This justified his definition of the number of elements belonging to a finite set M, or the cardinal of M, as the natural number n such that M is in one–one correspondence with Z_n.

I turn now to Frege's definition, in which the cardinal number of a class A is the class of all classes in one–one correspondence to A (*1884a*, art. 72).[1] This definition is quite appropriate, if a cardinal number is conceived as something belonging to a class of things which is common to different classes when they are 'equal in number'. Now it is possible to say, without using numerical terms, when two classes are equal in number: they are *numerically equal* or *equipollent* when there is a one–one correspondence between them. By saying a relation R is 'one–one' it is meant that for every x, y and z,

$$zRy \,.\, \& \,.\, zRx \,.\rightarrow .\, x=y, \qquad (6.3.13)$$

$$xRz \,.\, \& \,.\, yRz \,.\rightarrow .\, x=y. \qquad (6.3.14)$$

Thus classes are numerically equal if and only if there is a relation having the properties (6.3.13) and (6.3.14) between their elements. Frege specified something which is common to different classes when they are equipollent in the sense just defined. Moreover, in his system it could be proved that each class has a unique cardinal number and that the cardinals belonging to classes in one–one correspondence with each other are identical. The same treatment was later given independently by Russell, first in his *1901b*, arts. 1–2.

Frege defined the number 0 as the number belonging to the empty class; since there is only one empty class, $0=\{\varnothing\}$ (*1884a*, art. 74). The number 1 may now be defined as the class of all classes equipollent to 0, that is, to $\{\varnothing\}$, the class whose only element is the empty class.

[1] In this chapter I use both the words 'class' and 'set'. Certain historical and philosophical considerations have guided the choice of those terms, but the reader can take them as synonymous if he so wishes.

Any class equipollent to $\{\varnothing\}$ could have been used to define 1, but Frege wished to use only classes definable in the vocabulary of logic. The number 1 could also be defined thus:

$$1 \underset{\mathrm{Df}}{=} \{Z|(\exists y)[y \in Z \,.\, \& \,.\, (\forall x)(x \in Z \rightarrow x = y)]\}. \quad (6.3.15)$$

In either case, we get 1 as the class of all unit classes. In similar ways, the cardinals 2, 3, and so on could be defined (*1884a*, arts. 77–83).

The relation S of immediate succession between finite numbers is also easily defined in the vocabulary of logic (*1884a*, art. 76). Let $C(M)$ be the cardinal number of the class M. Then the relation S may be defined as follows:

$$nSm \leftrightarrow (\exists x, A)\{x \in A \,.\, \& \,.\, C(A) = n \,.\, \& \,.\, C(A - \{x\}) = m\}. \quad (6.3.16)$$

Using this definition and the definitions of 0 and 1, it can be proved that 1 immediately succeeds 0, that is, $1S\,0$: just let $x = \varnothing$ and $A = \{\varnothing\}$. It can also be demonstrated from Frege's definitions that the relation S is one–one and that 0 is not the successor of any number.

Using the same method which Dedekind employed in his definition of a simply infinite system, Frege defined the set N of natural numbers (finite cardinals) as the intersection of all classes Z such that $0 \in Z$ and whenever $x \in Z$ and ySx, then also $y \in Z$. The principle of mathematical induction is a consequence of this definition, and it can be proved that every natural number n has a successor $\neq n$. Moreover, from this proposition and his definitions and logical assumptions, Frege could prove that there exists an infinite cardinal number. The cardinal number of the class N is infinite, for N is equipollent to, for example, the class $N \cup \{N\}$. Therefore, the cardinal number of N succeeds itself and, hence, is not a finite number. This also shows why Frege's S is only suitable as the relation of immediate succession among finite numbers.

Not all mathematicians were satisfied with such foundations of arithmetic as were formulated by Dedekind and Frege. Some preferred to define the natural numbers as the results of a process of construction which proceeds according to a rule (see, for example, Kronecker *1887a*, art. 1; Poincaré *1913a*, 469; and Brouwer *1907a* in *Works*, vol. 1, 15), and to take the methods of proof and definition by induction as fundamental. The principle of induction involved in 'constructive' number theory has a totally different content from the proposition which occurs in the systems of Dedekind and Frege, and it has a different justification. The latter is a principle concerning sets, while the former brings in the concept of possibility. According to the principle of induction belonging to constructive mathematics, if a property can be verified for 1 and a method is known for turning a verification of the property for n into a verification of the property for $n+1$, then the property can be verified for an arbitrary number, which is what is meant, from the constructive point of view, by saying that 'all' numbers have the property. The justification of this method of induction was explained by Poincaré thus: 'the mind . . . knows it can conceive of the indefinite repetition of the same act, when the act is once possible. The mind has a direct intuition of this power, and experiment can only be for it an opportunity of using it, and thereby becoming conscious of it' (*1913a*, 39).

Of course, such a justification would appear quite unsatisfactory to those seeking a logical foundation of arithmetic; indeed, it was the very thing they wished to avoid. Thus Russell preferred the justification of inductive reasoning as a consequence of the definition of the finite integers rather than 'in virtue of any mysterious intuition' (*1919a*, 27), and both Frege and Russell found the assertions of possibility which are involved in justifications like Poincaré's doubtful (Frege *1903a*, arts. 125 ff.; Russell *1904a*). On the other hand, Poincaré had his reasons for rejecting the methods used to define the finite numbers and prove the principle of induction used in the logical foundation (*1913a*, 481).

6.4. *Logical foundations of mathematics*

Both Dedekind and Frege intended to reconstruct arithmetic, understood in the broad sense, on the basis of a system of precisely formulated definitions, from which the theorems were to be rigorously deduced by means of logical principles. But Frege went further than Dedekind by systematising the logical principles used in these deductions. He gave the following description of his 'ideal of a strictly scientific method in mathematics' (*1893a*, introduction):

> It cannot be demanded that everything be proved . . . but we can require that all propositions used without proof be expressly declared as such, so that we can see distinctly what the whole structure rests upon. After that we must try to diminish the number of these primitive laws as far as possible by proving everything that can be proved. Furthermore, I demand—and in this I go beyond Euclid—that all methods of inference employed be specified in advance . . .

Other mathematicians and logicians such as Peano and the American C. S. Peirce were engaged in formulating a logic of mathematical arguments, but no one carried it out with Frege's thoroughness and rigour.[1]

[1] In this chapter I am not describing the development of Boolean algebra by Boole, Peirce, Schröder and others; consult, for example, Kneale and Kneale *1962a*, ch. 6.

It is not possible to go into the details and peculiarities of Frege's system of logic here, but the basic ideas may be presented.

The principles of inference used in mathematical proofs were intended to be such that when applied to truths of any subject as premises, the conclusions derived would also be truths. Thus, by means of basic logical principles whose correctness or validity could be recognised, and certain axioms whose truth could be apprehended, further principles and truths could be derived.

The rules of Frege's logic were formulated solely on the basis of the meaning intended for the logical symbols '$\neg$', '$\rightarrow$', '&' '$\vee$', '$\leftrightarrow$', '$(\forall x)$', '$(\exists x)$' (see, for example, the account in Frege *1923a*, 40). The first five of these are used to form sentences from sentences in order to have compound sentences whose truth or falsehood depends solely on the truth or falsehood of the sentences from which they are constructed. Sentential connectives used in this way are called 'truth-functional'. The intended meaning is given in the following table (where the letters 'A' and 'B' represent sentences):

A	B	$\neg A$	$A \vee B$	$A \& B$	$A \rightarrow B$	$A \leftrightarrow B$
T	T	F	T	T	T	T
T	F		T	F	F	F
F	T	T	T	F	T	F
F	F		F	F	T	T

Some of these connectives can be defined in terms of others. For example, '$A \rightarrow B$' could be defined as '$\neg A \vee B$', and '$A \vee B$' could be introduced, alternatively, as an abbreviation for '$\neg A \rightarrow B$'.

When the sentential connectives are used in the way just explained, the basic logical rules and formulas of classical logic are obviously valid. Thus, given the intended meaning (from the table) of the conditional connective '$\rightarrow$', the correctness of such rules as the following is evident:

$$\frac{A,\ A \rightarrow B}{B} \quad \frac{A}{B \rightarrow A} \quad \frac{\neg A}{A \rightarrow B}. \tag{6.4.1}$$

The first of these rules, the rule of detachment, may be read: Given the sentences 'A' and '$A \rightarrow B$', the sentence 'B' may be inferred. This rule is sometimes called the '*modus ponens*'.

The general theory of deduction constituting Frege's logic comprises, besides the principles concerning arbitrary sentences, also principles relating to universal and existential statements. For example, it follows from the meaning of 'for all' and 'there is' that it is not the case that everything lacks the property P if and only if there is something having P.

The obvious rule of inference for existential propositions is this: From the statement 'Pa' infer '$(\exists x)Px$'. Thus, the existence of a thing having a certain property may be proved by giving an example of a thing having that property. But Frege's logic also includes another way of proving existence; for if a contradiction can be derived from the supposition '$\neg(\exists x)Px$', then, by means of the rule:

$$\frac{\neg(\neg A)}{A}, \tag{6.4.2}$$

it can be concluded that $(\exists x)Px$. (Inferences of this type are not valid in the logic of intuitionistic mathematics, where a different meaning is given to the logical symbols.)

Frege's programme of deriving mathematical theorems from definitions using only a small list of logical principles of the sort just described involved only arithmetic and did not extend to geometry. For Frege considered the axioms of Euclidean geometry to be intuitively apprehended truths, and therefore did not intend to reduce them to logic by the definition of basic geometrical concepts (see his *1906a*). By contrast, Russell aimed at a logical reconstruction of all pure mathematics, including geometry insofar as it belonged to pure mathematics as Russell conceived it: 'As a branch of pure mathematics, Geometry is strictly deductive, indifferent to the choice of its premisses and to the question whether there exist (in the strict sense) such entities as its premisses define' (*1903a*, 372). Geometry throws light on actual space only indirectly through 'increased analysis and knowledge of possibilities' (*1903a*, 374).

According to Russell's explication of the concept *proposition of pure mathematics*, the axioms of a particular mathematical theory (for example, Euclidean geometry) are *not* propositions of pure mathematics. What any branch of pure mathematics asserts is that 'such and such consequences follow from such and such premisses . . .' (*1903a*, 373; compare p. 458). The axioms of a mathematical theory are not actually propositions asserted in pure mathematics: 'The so-called axioms of Geometry, for example, when Geometry is considered a branch of pure mathematics, are merely the protasis [conditional clause] in the hypotheticals which constitute the science. They would be primitive propositions if, as in applied mathematics, they were themselves asserted . . .' (*1903a*, 430). Russell construed these 'so-called axioms' as 'parts of a definition' of a class of relations (*1903a*, 397). The asserted propositions of the theory, in the form 'Axioms $\rightarrow T$' (where T is a theorem), would then be those derivable by logic from the definition of a class of relations. Thus, the only genuine axioms used in the deduction of the asserted propositions of a mathematical theory would be logical axioms.

Besides the derivation of mathematical propositions from definitions, logic (in Russell's sense) had another very important function. It provided the existence theorems justifying the definitions formed from the axioms of a theory. An *existence theorem* for a theory asserts that there is at least one relation satisfying the conditions of the definition formed by its axioms. The demonstration of the existence theorem was also regarded as a proof of consistency.

The appearance of the antimonies showed that existence theorems proved by logical constructions did not give complete assurance of consistency. Russell consequently did not claim absolute certainty for his method, and he pointed out that such certainty is not had in any science. Since the antinomies have shown that logical common sense is not infallible, ' an element of uncertainty must always remain, just as it remains in astronomy. It may with time be immensely diminished; but infallibility is not granted to mortals . . . ' (*1906b*, 631 ; *1973a*, 194). Moreover, he thought that ' it follows from the very nature ' of an attempt to base mathematics on a system of undefined concepts and primitive propositions that the ' results may be disproved ' by the discovery of a contradiction, ' but can never be proved . . . All depends, in the end, upon immediate perception ' (*1903a*, 129). As Whitehead put it, ' the only rigid proofs of existence theorems are those which are deductions from the premises of formal logic. Thus there can be no formal proofs of the consistency of the logical premises themselves ' (*1907a*, 3).

6.5. *Direct consistency proofs*

Logicians and mathematicians like Frege, Russell and Whitehead had been inclined to think that the only way the consistency of a definition or axiomatic theory could be established was by an existence theorem. Russell, for example, asserted that ' freedom from contradiction can never be proved except by first proving existence : it is impossible to perform *all* the deductions from a given hypothesis, and show that none of them involve a contradiction ' (*1910b*, 438). He apparently did not recognise the possibility of proving general theorems about the proofs which are possible in a theory. The first one to seek consistency proofs along such lines was David Hilbert. His consistency proofs for arithmetic theories were to be ' direct ' ; it was to be shown by purely formal considerations that from the axioms of a particular theory a statement and its negation cannot be derived by means of a finite number of logical inferences.

Hilbert's ' Über die Grundlagen der Logik und Arithmetik ' (' On

the foundations of logic and arithmetic ' : *1904a*) contains the first presentation of his ideas for direct consistency proofs. The first step in his method involves the specification of symbols which are to be used, the axioms, and the means of inference ; thus logic and mathematics are formulated concurrently. Actually his procedure in this first attempt was not nearly so rigorous as it was to become in another twenty years. In *1904a* he did not make a clear distinction between the formal axiomatic system which is to be investigated for consistency and the metatheory in which the object theory (that is, the axiomatic system) is studied ; and, what is of particular importance, there is no characterisation of the means of proof to be used in demonstrating the consistency of logico-arithmetic object theories. Indeed, it was on issues related to this point that critics concentrated.

Since Hilbert needed to prove a general proposition relating to a denumerable infinity of possible formal derivations, it seemed that a form of inductive argument would be necessary ; but one of the systems whose consistency he aimed to establish was the theory of finite integers, which contains an axiom of induction (see Poincaré *1913a*, 455 ; and Brouwer *1907a* in *Works*, vol. 1, 93). Hilbert's *1904a* has nothing to satisfy people who wonder about that situation, but later he did clarify this matter. Proofs about the formal axiomatic theory of numbers were to be based solely on the construction and decomposition of numerical expressions, and this method was, in his view, essentially different from that principle of induction which needs and is capable of proof (*1922a*, 164 ; compare what was said in section 6.3 above concerning the constructive principle of induction). His clarifications, however, were not entirely satisfactory to everyone. (See van Heijenoort *1967a*, 480–482, where the editor gives an excellent account of this issue.)

The system that Hilbert dealt with in ' On the foundations of logic and arithmetic ' contained two axioms of identity and three arithmetical axioms. Because this system is rather peculiar, I shall discuss his treatment of a very similar, but simpler, system which is found in his ' Neubegründung der Mathematik ' (' New foundations of mathematics ' : *1922a*) and contains the following five axioms :

$$a=a, \quad (6.5.1)$$

$$a=b \rightarrow a+1=b+1, \quad (6.5.2)$$

$$a+1=b+1 \rightarrow a=b, \quad (6.5.3)$$

$$a=c \rightarrow (b=c \rightarrow a=b), \quad (6.5.4)$$

$$a+1 \neq 1, \quad (6.5.5)$$

together with the rule of detachment (6.4.1). A *proof* with respect to these axioms is defined to be a list of formulas having a last entry, such that each formula in the list is either one of the axioms or the result of substituting numerals or other variables for the variables in an earlier entry, or a formula B with the formulas A and $A \to B$ occurring among the preceding entries (that is, a formula inferred in accordance with the rule of detachment). For example, the following list of formulas is a proof of the symmetrical property of equality:

$$a=c \to (b=c \to a=b), \quad (6.5.6)$$

$$a=a \to (b=a \to a=b), \quad (6.5.7)$$

$$a=a, \quad (6.5.8)$$

$$b=a \to a=b. \quad (6.5.9)$$

The last line of a proof, the formula proved, is called the 'end formula'.

The axiom system (6.5.1)–(6.5.5) is consistent if an equation $\alpha=\beta$ and its negation $\alpha \neq \beta$ are never both provable. In order to prove that the system is consistent, Hilbert established the lemma: A provable formula can contain the sign '$\to$' at most twice. Suppose that a proof contained a formula in which there were more than two occurrences of '$\to$'. If a proof contains such a formula at all, it must have a first such. But the first formula B in a proof containing '$\to$' more than twice could neither be derived from earlier lines by substitution nor by means of the rule

$$\frac{A,\ A \to B}{B},$$

for the premise $A \to B$ would already have to contain more than two occurrences of '$\to$'.

Hilbert also proved a second lemma stating that an equation $\alpha=\beta$ is provable only if α is the same symbol as β. It should be noted here that the symbols 'α' and 'β' do not belong to the vocabulary of the object theory under investigation; they are variables belonging to the meta-theory, and their range of values consists of the numerals and variables of the object theory together with the expressions made up from them by using '+'. Thus, according to his second lemma, an equation written in the language of the object theory is only provable from axioms (6.5.1)–(6.5.5) if the same symbol occurs on both sides of the sign '='.

By means of the two lemmas, Hilbert demonstrated the consistency of his axiom system as follows. Since only equations having the same sign on both sides of '=' are provable, if the system were inconsistent, some formula of the form

$$\alpha \neq \alpha \quad (6.5.10)$$

would have to be provable. But only inequalities of the form

$$\alpha+1 \neq 1 \quad (6.5.11)$$

are provable by direct substitution in the axioms (for only axiom (6.5.5) contains the sign '$\neq$'). If a formula $\alpha \neq \alpha$ were provable by means of the detachment rule, then a premise of the form $C \to \alpha \neq \alpha$ would have to be used; but since it could not be derived directly by substitution, it would require a premise $B \to (C \to \alpha \neq \alpha)$, which in turn would depend on a premise $A \to (B \to (C \to \alpha \neq \alpha))$. But according to the first lemma, no provable formula has more than two occurrences of '$\to$'.

The axiom system which Hilbert used to illustrate direct proofs of consistency was a rather elementary part of the arithmetic of natural numbers and did not contain an axiom of mathematical induction. But he was confident that essentially the same methods of reasoning would yield consistency proofs for much more advanced systems, such as the full arithmetic of integers, the theory of real numbers, and also the theories of Cantor's higher number-classes. However, in *1931a* Kurt Gödel published a theorem, called 'Gödel's theorem on consistency proofs', which showed that consistency proofs for (say) Peano's axiomatisation of arithmetic, using only the very elementary (finite combinatorial) methods of the sort which Hilbert had in mind, are impossible. Nevertheless, interesting results (in particular, Gentzen's theorem on the consistency of elementary number theory) have been obtained by modifying Hilbert's programme. The interested reader may consult Andrzej Mostowski's *Thirty years of foundational studies* (*1966a*) for further information on the work of Gödel and Gentzen, and on other advances in foundations occurring after the period covered in this chapter.

6.6. *Russell's antinomy*

Cesare Burali-Forti was the first mathematician to publish an antinomy of set theory. In his *1897a* he considered the class of all Cantorian ordinals and showed that its ordinal Ω satisfied the contradictory properties

$$\Omega+1>\Omega \quad \text{and} \quad \Omega+1 \leqslant \Omega. \quad (6.6.1)$$

However, his result does not seem to have made much of an impression. People were perhaps inclined to believe that some error had been made in reasoning in a new and unfamiliar subject. The same applied to the next antinomies that were reported in print—of the set of all alephs and the set of all powers—which were similar to Burali-Forti's. For example, when Hilbert mentioned them to Frege in 1900, Frege seems to have been uninterested (see Frege *1971a*, 12). While he was a firm supporter of the new theory of the transfinite, he considered Cantor's formulations to be imprecise ; perhaps he thought that some error had been made on that account.

The case was entirely different with the antinomy of the class of all classes which do not belong to themselves, which was discovered independently by Russell and by Zermelo and first published in 1903 in the second volume of Frege's *Grundgesetze* (*1903a*, appendix) and in Russell's *The principles of mathematics* (*1903a*, ch. 10). This antinomy involves only the concepts of class and membership, and it follows almost immediately from the axiom which had been the implicit basis of set theory. Yet it is an interesting fact that Russell was led to discover it by reflecting on the implications of Cantor's theorem which asserts that for every set there is another set, its power-set, of greater cardinal number (see section 5.9).

Russell noticed that although Cantor had proved that there is no greatest cardinal number, there must nevertheless be such a number. In fact, the greatest number should be the number of the class of all entities, for there cannot possibly be a larger class. This antinomy, which was already known to Cantor, is usually called 'Cantor's paradox'.[1] Russell became aware of it in January of 1901 and gave it its first mention in print in an article *1901a*, in which he said that if Cantor's proof that there is no greatest number were valid, 'the contradictions of infinity would reappear in a sublimated form. But in this one point, the master has been guilty of a very subtle fallacy, which I hope to explain in some future work' (*1917a*, 89).

Thus Russell's reaction to the first antinomy which he discovered was that there must be some subtle mistake in Cantor's argument ; he did not conclude that there is something fundamentally wrong in set theory. Russell minutely examined the proof of Cantor's theorem, expecting to find some error ; in cases like that of the class of all entities, the class of all classes, or the class of all propositions, it seemed to him 'as though Cantor's proof must contain some assumption which is not verified' (*1903a*, 362).

Expecting an error of this sort, Russell examined the results of applying the method of Cantor's proof to such classes. The result was his discovery of the antinomy which has come to be called 'Russell's paradox'. In his *Introduction to mathematical philosophy* he says that when he first came upon the contradiction of the greatest number in 1901, 'I attempted to discover some flaw in Cantor's proof that there is no greatest cardinal . . . Applying this proof to the supposed class of all imaginable objects, I was led to a new and simpler contradiction . . .' (*1919a*, 136).

[1] I avoid using the word 'paradox' to describe such results, for 'paradox' is used very loosely in common speech to cover antinomies, correct arguments with puzzling or counter-intuitive though non-contradictory conclusions, and also only apparently valid arguments, such as the old-fashioned 'paradoxes of the infinite'. The antinomies of set theory are real contradictions derivable by means of specified logical rules (correctly applied) from an apparently true thesis concerning the existence of sets.

In order to see how Russell discovered the antinomy of the class of all classes which do not belong to themselves, recall from section 5.7 the method of showing in the proof of Cantor's theorem that any set A is not equipollent to the power-set $P(A)$ of all its subsets. For any one–one correspondence f whose domain of arguments is $A' \subseteq A$ and whose range of values is a subset of $P(A)$, the class

$$K \underset{\mathrm{Df}}{=} \{x \mid x \in A' \,.\, \& \,.\, x \notin f(x)\} \tag{6.6.2}$$

belongs to $P(A)$ but not to the range of values of f. If it is supposed that K is an f-correlate of some element y of A', that is, if

$$y \in A' \,.\, \& \,.\, f(y) = K, \tag{6.6.3}$$

it follows that

$$y \in K \leftrightarrow y \notin K, \tag{6.6.4}$$

which implies the contradiction $y \in K \,.\, \& \,.\, y \notin K$. Now let us see what happens when A is the class U of all things, A' is the subclass C of U containing all classes, and for each $c \in C$, $f(c) = c$. In this case, K is the class R of all classes which do not belong to themselves, and it *follows* that

$$R \in C \,.\, \& \,.\, f(R) = R \tag{6.6.5}$$

(compare (6.6.3), which was only a supposition), and consequently that

$$R \in R \leftrightarrow R \notin R. \tag{6.6.6}$$

It is in this or some very similar way that Russell discovered his antinomy ; I would guess that the same is true in the case of Zermelo.

Once R is thought of, it is evident that its existence and the antinomy (6.6.6) follow immediately from the principle that for any sentential form 'ϕx',

$$(\exists M)(\forall x)\{x \in M \leftrightarrow \phi x\}, \tag{6.6.7}$$

and hence that this principle is false. Thus, while Russell set out to correct a supposed error in Cantor's reasoning, he obtained another antinomy which made it entirely clear that something was wrong with the very first principles of set theory, or, as Russell would have said, with the principles of logic, since he regarded the general theories of classes and relations as branches of logic.

The principle (6.6.7) usually referred to as 'the naive principle of comprehension' (or abstraction), asserts that every propositional function 'ϕx', or every property, determines a class. This was taken to be an evident truth by Russell (see *1903a*, 102). He also pointed out that the naive axiom of abstraction is unofficially present in Peano's system: 'Peano holds (though he does not lay it down as an axiom) that every proposition containing only one variable is reducible to the form "x is an a"' (*1903a*, 28; compare pp. 19 and 103). A principle having the same effect as (6.6.7) was also advanced by Frege.

Perhaps no one was so upset by Russell's antinomy as Frege. At first he made some attempt to resolve this antinomy (*1903a*, appendix), but apparently he became dissatisfied with any of the methods which were introduced to avoid the antinomies. Near the end of his life, he said that set theory had been 'destroyed' by the antinomies (*1969a*, 289). He gave up attempting a logical reconstruction of arithmetical theories and settled for a geometrical foundation of arithmetic (*1969a*, 298–302). In contrast, Russell's attitude was that the antinomies 'can all be removed by patience in distinguishing and defining' (*1910a*, 373).

6.7. *The foundations of* Principia mathematica

When Russell began to deal with the problem of the antinomies, he 'hoped the matter was trivial and could be easily cleared up' (*1944a*, 13). But it took five years of effort before he produced the system for avoiding the antinomies which is used in *Principia mathematica*. Indeed, he never succeeded in formulating a system which completely satisfied him. One thing that made the problem of the antinomies so hard for him was that he sought not merely a way of avoiding them; he also desired an independent explanation for the necessity of making the particular restrictions. In *My philosophical development* he says that while he was 'looking for a solution' he considered it a requisite of a 'wholly satisfying' solution that it 'should, on reflection, appeal to what may be called "logical common sense"—i.e. that it should seem, in the end, just what one ought to have expected all along' (*1959a*, 79). And in *1906b* he said that satisfactory principles should 'recommend themselves to intuition' and 'show exactly how we formerly fell into error' (*1906b*, 631; *1973a*, 195). Although he wanted to find a way of avoiding the antinomies which appeals to logical common sense, in his *The principles of mathematics* he had said the antinomy of the class of all classes which do not belong to themselves 'springs directly from common sense, and can only be solved by abandoning some common-sense assumption' (*1903a*, 105).

Russell's main idea for a solution of the antinomies is already contained in *Principles*: the doctrine that each propositional function[1] has a 'range of significance', and is *meaningless* with arguments outside this range. The formulas which give rise to the antinomies are, of course, the meaningless ones: in particular, the expression 'x belongs to x' and its negation were declared to be meaningless. But in order to have a 'solution' of the antinomies, Russell needed an explanation of why certain apparently meaningful statements about classes are really meaningless; he did not wish merely to propose that certain combinations of symbols be declared not well-formed as one way of avoiding the antinomies. Now Russell did not believe that (for example) '$x \in x$' and its negation would be meaningless if there are such things as classes: 'That it is meaningless . . . to regard a class as being or not being a member of itself, must be assumed for the avoidance of a . . . mathematical contradiction; but I cannot see that this could be meaningless if there were such things as classes' (*1910a*, 376).

Consequently, another feature of Russell's efforts with the antinomies was his tendency to employ some form of 'no-class theory', that is, a theory in which it is not assumed that many entities ever 'collectively form a single entity which is the class composed of them' (*1906a*, 46; *1973a*, 155). Russell formulated two such theories. The first one, the substitutional theory, was only briefly described in Russell's publications, but he wrote a long exposition of it which has now been published (*1973a*, 165–189; for commentary, see Grattan-Guinness *1974b*, 389–401). The second no-class theory appears in Russell's 'Mathematical logic as based on the theory of types' (*1908a*) and in *Principia mathematica*. Both of Russell's no-class theories provided a meaning for some statements purporting to be about classes, but neither provided a meaning for such statements as '$x \in x$' or '$x \notin x$'.

Russell eventually became convinced that the basic fallacy underlying the antinomies was some sort of vicious circle. This idea had appeared in Poincaré *1906a*, where it was explained by reference to the following antinomy formulated in Jules Richard *1905a*.[2] Let E be the set of all

[1] The term 'propositional function' is not clearly used with a single definite meaning in Russell's writings. Sometimes it seems to mean a function whose values are propositions in the sense of objective truths and falsehoods, while at other times it means a sentential form, that is, an expression like a sentence except that it contains one or more variables and becomes a sentence when constants are substituted for its variables. Unfortunately, it is sometimes impossible to be sure what Russell intended.

[2] Although the idea that a sort of vicious circle was involved in the use of bound functional variables had occurred to Russell as early as 1904, he did not (or was at least uncertain whether to) regard the antinomies as 'vicious circle fallacies' until 1906. The paper on the substitutional theory written before Poincaré's article does not claim that the antinomies are due to vicious circles. Rather the suggestion was that the antinomies are due to 'false abstraction' (see *1973a*, 165). In a letter of January 1906

decimals which can be defined in a finite number of words; the set is obviously denumerable and, hence, can be arranged in a sequence. But, by reference to a sequence of the elements of E, it is possible to define in a finite number of words a decimal N which does not belong to E. N is the decimal containing the digit $p+1$ in its n-th place if p is in the n-th place of the n-th element of E and is not 8 or 9, but containing the digit 1 if p is 8 or 9. Now, according to Poincaré, 'the true solution' of Richard's antinomy is this: 'E is the aggregate of *all* the numbers definable by a finite number of words *without introducing the notion of the aggregate E itself*. Else the definition of E would contain a vicious circle; we must not define E by the aggregate E itself' (*1913a*, 480). Since N is defined 'with the aid of the notion of the aggregate E', it does not belong to E. The other antinomies are supposed to be explicable in a similar manner.

But no definition given in the statement of Richard's (or any other) antinomy is circular in the ordinary sense: the term defined or a synonymous term does not occur in the defining expression, and the definitions are not defective for the reason that the definitions usually called 'circular' are. Thus Peano asserted that 'the definitions of Richard do not contain a vicious circle' (*1906a*, art. 4; *1973a*, 214), and pointed out that the usual definition of the least common multiple of two integers is of the kind which Poincaré alleges to contain a vicious circle (*1973a*, 215). Zermelo also emphasised in *1908a* the fact that definitions of the form called 'viciously circular' by Poincaré have been very frequently used in mathematics 'and up to now it has not occurred to anyone to regard this as something illogical' (van Heijenoort *1967a*, 190–191). As a matter of fact, Russell himself had denied, in an appendix to *The principles of mathematics*, that there is any vicious circle involved in the method of Frege's definition of the natural numbers (*1903a*, 522), though that definition is a prime example of the sort classified as viciously circular by Russell and Poincaré in 1906.

In *Principia mathematica* Russell tried to explain why statements purporting to assert something about absolutely *all* propositions, propositional functions, or classes must be considered meaningless. The reason why such general statements are supposed to be meaningless is that the totalities to which they refer cannot be definite. A proposition such as: all propositions are true or false, 'could not be legitimate unless "all propositions" referred to some already definite collection, which it cannot do if new propositions are created by statements about "all propositions"' (*1903a*, 37; see also *1959a*, 82). Many years

Russell wrote to Philip Jourdain that 'The error seems to me to lie in supposing that many entities ever combine to form one new entity, the class composed of them' (see Grattan-Guinness *1977a*, 68).

later he said in his book *My philosophical development*: 'I must confess that this doctrine has not won wide acceptance, but I have seen no argument against it which seemed to me cogent' (*1959a*, 83). It may be remarked, however, that some have found his explanations hard to understand (see Chihara *1973a*).

The scheme for avoiding the antinomies in *Principia* is called the 'theory of types', or the 'ramified theory of types' (to distinguish it from the 'simple theory of types' which was developed later by L. Chwistek, F. P. Ramsey and others). The principle guiding its formulation is the *vicious circle principle* (VCP), which may be stated as follows: A totality T may not contain elements which are only definable by means of an expression containing a bound variable (such as 'x' in 'for all x, ... x ...' and in 'there is an x such that ... x ...') whose range of values contains *all* elements of T. For example, the property P of having all properties Q of the class T must not be a member of T.

In accordance with the VCP, propositional functions which have a value for an object a as argument (a-functions or functions 'significant' for a) are classified into orders. The VCP rules out a totality of all a-functions, for there are a-functions which can only be defined by means of a bound variable ranging over some totality of a-functions. Such functions must lie outside of the totalities in terms of which they are defined: at least, they must according to the VCP. But note that, strictly speaking, we cannot say that any 'legitimate' totality of a-functions does not include *all* a-functions, or that for any legitimate totality of a-functions *there is* some a-function not belonging to it: such statements are supposed to be meaningless (see *PM*, vol. 1, 55).

Let us say that a function presupposes or involves a totality T if it is definable only by means of a bound variable whose range includes all of T; it also presupposes whatever is presupposed by the members of such a totality, and so on. The *order* of a function depends on what totalities it presupposes. The functions significant for a particular entity a as argument are of infinitely many different orders above the order of a. The objects of the absolutely lowest order are the individuals (concrete objects). First-order functions are those which presuppose only the totality of individuals, while second-order functions of individuals presuppose only a totality of first-order functions, in addtion to the totality of individuals. There are also functions of individuals of arbitrarily many higher orders, and functions of order m can be arguments to functions of arbitrarily many orders above m. A function is said to be *predicative* if its order is next above the order of its highest-order argument; in other words, 'if it is of the lowest order compatible with its having the arguments it has' (*PM*, vol. 1, 53). Alternatively explained, 'a predicative function of a variable argument is one which involves no totality except that of the possible argument, and those that are presupposed by any one of the possible arguments' (*ibid.*, 54). It should be noted that the concept of a predicative function is a primitive idea in *Principia mathematica*.

In addition to orders, there is also a hierarchy of types. The *type* of a function depends not only upon its order but also on the number and kind of the arguments that it takes; thus it is a sub-classification of orders. For example, second-order functions of two individual arguments constitute a type.

Russell defined finite cardinal numbers as classes of equipollent classes independently of, though in broadly the same manner as, Frege's definitions in section 6.3 above; but because of type theory numbers have to be defined for each type. (Russell also extended his definitions to include transfinite cardinal and ordinal arithmetic.) Now adherence to the system of orders is sufficient to develop these definitions and also to avoid the antinomies, but it makes it impossible to define the finite numbers by means of the property of having absolutely all hereditary properties of 0 (a property is *hereditary* if whenever a number has it, so does its successor). According to Russell's doctrine, expressions containing the phrase 'all properties' or 'all functions' are meaningless, and the property of having all of a certain totality of properties is of higher order than any of the properties belonging to that totality. If the integers are defined as the things having all hereditary properties of a particular order m which belong to 0, then the principle of induction is not a consequence of the definition. Since, by the original definition, the property N of being a number is the property of having *all* hereditary properties of 0, it follows that if P is a hereditary property of 0, then whatever has N has P. But if N is only the property of having all hereditary properties of order m which belong to 0, and P is a hereditary property of 0 whose order is higher than m, it does not follow from the definition of N that every number has P. To take a simple example from Russell *1908a*, art. 5, if the finite numbers are defined as those having all first-order hereditary properties of 0, then 'we shall be unable to prove that if m, n are finite numbers, then $m+n$ is a finite number. For, with the above definition, "m is a finite number" is a second-order property of m . . .' (van Heijenoort *1967a*, 167).

The theory of real numbers is also affected. It will be recalled from section 6.1 that the virtue of Dedekind's definition was that it had the property of completeness (continuity) as a consequence: that is, using Russell's modification of the definition of the system of real numbers, it follows that every segment of the real numbers has an upper limit. A segment of reals is a certain class of classes of rationals, and its upper limit is its union. But, owing to the bound variable occurring in its definition, the union of a class A of classes will generally be a class of higher order than the elements of A and consequently cannot, according to the VCP, belong to a class containing A as a subclass. Thus, according to the VCP, the upper limit of a class of real numbers cannot generally be a member of the system of real numbers, and the system will not be complete.

In order to compensate for the much too negative effect of the vicious circle principle, Whitehead and Russell postulated the axiom of reducibility: 'The axiom of reducibility is introduced in order to legitimate a great mass of reasoning, in which, prima facie, we are concerned with such notions as "all properties of a" or "all a-functions", and in which, nevertheless, it seems scarcely possible to suspect any substantial error' (*PM*, vol. 1, 56). The axiom of reducibility is the statement that any propositional function satisfied by an object a is formally equivalent to a predicative function (or predicate) of a. Two propositional functions are formally equivalent if they are satisfied by exactly the same arguments, that is, if they are co-extensive. Thus, the axiom of reducibility means that for a propositional function of *any order* whatsoever which is satisfied by the object a there exists a co-extensive function whose order is next above that of a.

Assuming the axiom of reducibility, if the finite numbers are defined as those having all hereditary predicates of 0, then it will be possible to prove that a higher-order hereditary property P of 0 belongs to all finite numbers. For it follows from the definition that a hereditary predicate of 0 belongs to all finite numbers, and the axiom of reducibility asserts that there is a predicate Q of numbers which is coextensive with the property P. Since Q is coextensive with P, Q is a hereditary property of 0, and P belongs to all finite numbers because Q does. Similarly, the axiom of reducibility saves the theory of real numbers in spite of the VCP.

6.8. *Axiomatic set theory*

After Cantor discovered that a contradiction is sometimes implied by the supposition that there exists a set of all things having a certain property, he began to distinguish two kinds of multiplicities, which he called 'consistent' or 'sets' and 'inconsistent' or 'absolutely infinite'. Though this distinction was not satisfactory, his basic idea for avoiding the contradictions has—after being extended and improved—become the most widely accepted reformulation of set theory.

Cantor explained an inconsistent multiplicity, in a letter of 1899 to Dedekind, as one for which 'the assumption that all of its elements "are together" leads to a contradiction, so that it is impossible to conceive of the multiplicity as a unity, as "one finished thing"' (*Papers*, 443 ; van Heijenoort *1967a*, 114). But this is practically as much as to say that there are no such things as inconsistent multiplicities ; as Cantor himself said to Jourdain, 'inconsistent multiplicities . . . can never be conceived *complete* and *actually existing*' (Grattan-Guinness *1971a*, 119). Thus Cantor does not really have 'two kinds' of multiplicities. It is no wonder that Dedekind found Cantor's purported distinction 'unclear' and did not know what Cantor meant by 'Zusammensein aller Elemente einer Veilheit' ('togetherness of all elements of a multiplicity'; Grattan-Guinness *1974a*, 129). The fact is, Cantor spoke of an inconsistent multiplicity when he might better have spoken of a property such that the supposition that there is a set of all things having that property leads to a contradiction.

Later, John von Neumann formulated a system which avoids the antinomies by not assuming that every class belongs to further classes (see especially his *1925a*). The term 'set' is reserved for those classes which are elements of other classes. Here two kinds of classes really are distinguished. It is usually stated that Cantor's inconsistent multiplicities are von Neumann's classes which are not elements ; but in the case of the former a contradiction results from the supposition of their existence, while the latter are such that the supposition that they are elements leads to a contradiction. What is common to the systems of Cantor and von Neumann is not the distinction between 'sets' and 'proper classes' (Cantor having no satisfactory distinction), but the supposition that it is the very large totalities which are involved in the antinomies.

Which multiplicities Cantor thought to be inconsistent is, it seems to me, evident from his alternate description of them as 'absolutely infinite'. He had always characterised the series of transfinite ordinals as absolutely infinite. His idea seems to have been something like this : each transfinite number is surpassed by other transfinite numbers, but what is absolutely infinite has the property of being essentially incapable of enlargement (see *Papers*, 167, 175 and 375). Thus I take his identification of the inconsistent multiplicities with those which are absolutely infinite as an expression of his belief that the *only* inconsistent multiplicites are the absolute totalities such as the totality of all things, all sets, or all ordinals, or totalities which would have to be as large, that is, which would have to contain a part equipollent to one of these absolutes. As Gerhard Hessenberg put it in Grelling and Nelson *1907a*, 330 (Nelson *1959a*, 82) :

> Certainly every set containing a part equivalent to W [the set of all ordinals] is infected with the same contradiction as W itself, and its power is greater than every aleph. Now Herr Cantor conjectures that conversely this condition is sufficient, that therefore every set, whose power is an aleph, is consistently conceivable, so that one has to designate the type W so to speak as the 'first' or 'smallest' paradoxical type.

Cantor's method of dealing with the antinomies is of the sort which Russell discussed (but did not adopt) in his *1906a* under the title 'theory of limitation of size'. 'This theory', Russell said, 'is naturally suggested by the consideration of Burali-Forti's contradiction, as well as by certain general arguments tending to show that there is not . . . such a thing as the class of all entities' (*1906a*, 43 ; *1973a*, 152). He also stated that the theory of limitation of size 'naturally becomes particularised into the theory' that a propositional function determines a class if there is a one–one relation between the things satisfying it and some initial segment of the ordinals. He even thought certain considerations made it seem likely that if an antinomy can be derived from the supposition of a class of all things having the property ϕ, then it will be possible to define a one–one relation between all ordinals and some (or all) of the things having ϕ (*1906c*, 36 ; *1973a*, 144). In Cantor's terminology : If A is an inconsistent multiplicity, then some submultiplicity of A is equivalent to W. This is just a particularisation of the conviction, maintained by Cantor, that *only* absolutely infinite multiplicities are inconsistent.

Russell himself showed that there are at least as many classes which do not belong to themselves as there are ordinal numbers. The following argument is suggested by his considerations (*1906a*, 35 ; *1973a*, 143), but is not exactly the one he gave. If x is a set of sets which do not belong to themselves, then neither x nor $x \cup \{x\}$ belongs to itself. The union of a set of sets whose members do not belong to themselves is a set of sets not belonging to themselves ; hence it does not belong to itself. Consequently, if x is any set not a member of itself, the series :

$$\left.\begin{aligned} S_0 &\underset{\mathrm{Df}}{=} x, \\ S_{\alpha+1} &\underset{\mathrm{Df}}{=} S_\alpha \cup \{S_\alpha\}, \\ S_\lambda &\underset{\mathrm{Df}}{=} \bigcup_{\alpha<\lambda} S_\alpha, \text{ for limit numbers } \lambda, \end{aligned}\right\} \qquad (6.8.1)$$

is a series of sets which do not belong to themselves, and the series is isomorphic to the series of all ordinals.

In order to reconstruct set theory on the basis of Cantor's analysis of the antinomies, it is necessary to formulate a system of axioms from which the theorems can be proved but from which the existence of the large ' sets ' involved in the antinomies does not follow. In correspondence with Dedekind, Cantor formulated several of the most important axioms of set theory. Let us first consider these axioms formulated in Cantor's terminology (writing ' $S(A)$ ' for ' A is a set ') :

$$S(A) \,.\, \& \,.\, B \sim A \,.\, \rightarrow .\, S(B), \quad (6.8.2)$$

$$S(A) \,.\, \& \,.\, A' \subseteq A \,.\, \rightarrow .\, S(A'), \quad (6.8.3)$$

$$S(A) \rightarrow S(\cup A). \quad (6.8.4)$$

But what is the range of the variables ' A ', ' A' ', and ' B ' ? It surely cannot be multiplicities in general comprising both consistent and inconsistent multiplicities, for the latter do not exist. This defect in the formulation of Cantor's system is, however, easily removed. Instead of inconsistent multiplicities, we could speak, as Russell did, of properties which do not determine a set. (A property does not determine a set if there is no set having as its elements exactly the things possessing the property.) Thus, Cantor's axioms could be formulated as follows : (1) If ϕ is a property of sets and there is a one–one relation between the things having ϕ and the set M, then ϕ determines a set, that is :

$$(\exists A)(\forall x)\{x \in A \leftrightarrow \phi(x)\}. \quad (6.8.5)$$

(2) If M is a set, then the property of having ϕ and belonging to M determines a set. (3) The property of being a member of a member of the set M of sets determines a set.

The first published system of axioms based on the theory of limitation of size was formulated in 1908 by Ernst Zermelo.[1] But his system was designed to avoid the antinomies of ' finite definability ', such as Richard's antinomy, as well as those concerning very large totalities. Russell made no distinction between the various antinomies, but considered them all as ' vicious circle fallacies '. By contrast, Zermelo, following Peano and Hessenberg, distinguished between antinomies which could be formulated in terms of the primitive concepts of set theory and antinomies involving definability.

In order to avoid the antinomies of finite definability, Zermelo introduced the concept of a ' definite ' assertion. An assertion about sets is definite if the relations of set membership and identity ' by means of the axioms and the universally valid laws of logic, determine without arbitrariness whether it holds or not ' (*1908b*, art. 1 ; van Heijenoort *1967a*, 201). A sentential form ' ϕx ' is definite, if every assertion resulting from an assignment of a value to ' x ' is definite. Now, instead of axiom (2) above, Zermelo's system contains the following

[1] Some similar ideas are in Harward *1905a*, which seems not to have been influential.

axiom of ' separation ' (axiom III) : A definite sentential form determines a subset of the set M ; that is, if ' ϕx ' is definite, then

$$(\exists A)(\forall x)\{x \in A \,.\, \leftrightarrow .\, x \in M \,.\, \& \,.\, \phi x\}. \quad (6.8.6)$$

This is to avoid including such a set as the set of all decimals which are definable in a finite number of words.

While Zermelo's definition of ' definite ' is hardly satisfactory, it is not in the least doubtful which assertions he had in mind. The definite assertions were to be all those which can be expressed using only variables for sets, ' = ', ' $\in$ ', and logical symbols such as ' $\neg$ ', ' & ', ' $\rightarrow$ ', ' $(\forall x)$ ' and ' $(\exists x)$ '. The first precise definition of the concept of a definite assertion was given in *1910a* by Hermann Weyl (*Papers*, vol. 1, 304).

In addition to the axiom of separation, Zermelo's system included the axiom of extensionality

$$(\forall x)(x \in A \leftrightarrow x \in B) \,.\, \rightarrow .\, A = B, \quad (6.8.7)$$

and axioms asserting the existence of : an empty set, a set $\{a\}$ for any a, a set $\{a, b\}$ for any a and b, the union of any set, the set of all subsets of any set. It also included the following axiom of choice : The union of any set T of pairwise disjoint non-empty sets contains at least one subset whose intersection with each member of T is a unit set (see section 6.9 below). He also formulated an axiom of infinity suggested by Dedekind's proof (*1888a*, art. 66) that a simply infinite system exists. According to this axiom, there exists a set which contains the empty set, and whenever it contains x, also contains $\{x\}$. A set whose elements may be called ' natural numbers ' is defined as follows : Let Z be a set whose existence is postulated by the axiom of infinity. The set Z_0 of natural numbers is the intersection of the set of all subsets of Z which contain $\varnothing$ and the unit set of each of their elements :

$$Z_0 \underset{\mathrm{Df}}{=} \bigcap\{X \mid X \subseteq Z \,.\, \& \,.\, \varnothing \in X \,.\, \& \,.\, (\forall x)(x \in X \rightarrow \{x\} \in X)\}. \quad (6.8.8)$$

Z_0 cannot simply be defined as the intersection of the set A of all sets containing the empty set as well as the unit set of any element they contain, because such a set A would be ' too big ', and its existence could not be proved from Zermelo's axioms.

In *Principia mathematica* axioms of choice and infinity are formulated, but not assumed to be true ; they are taken as hypotheses, and many conditional theorems are established having one of these propositions as antecedent. There is a significant difference between Zermelo's and Russell's axioms of infinity : the former asserts the existence of an infinite set of sets, while the latter asserts the existence of an infinite set of individuals or concrete objects. Russell had to formulate such an axiom of infinity because of his theory of types (see especially his *1908a*, art. 10).

6.9. *The axiom of choice*

Before Zermelo's *1904a* focussed attention on the axiom of choice, various mathematicians had without realising it formulated proofs whose validity depended upon that axiom. A good example is provided by Dedekind's argument for the proposition that every set S containing for each natural number n a subset equivalent to Z_n (which is the set (6.3.12) of natural numbers between 1 and n) is equipollent to some proper subset of itself (*1888a*, art. 159). According to the hypothesis of the theorem, for each n there is a one–one function mapping Z_n into S; that is, for each n, the set A_n of one–one mappings from Z_n into S is not the empty set. Dedekind went beyond the hypothesis of the theorem when he assumed that there is a sequence of functions α_n such that α_n belongs to A_n.

Russell also unwittingly used an argument (due to Cantor) which needs the axiom of choice as a premise (*1903a*, 122–123). But he eventually recognised as a separate assumption the form of the axiom of choice which he called the 'multiplicative axiom': For every class A of mutually exclusive non-empty sets, there exists at least one set comprising exactly one element from each member of A. In a letter to Jourdain written in 1906, he relates how this came about (Grattan-Guinness *1972a*, 107; *1977a*, 80):

> As for the multiplicative axiom, I came on it so to speak by chance. Whitehead and I make alternate recensions of the various parts of our book, each correcting the last recension made by the other. In going over his recensions, which contained a proof of the axiom, I found that the previous proposition used in the proof had surreptitiously assumed the axiom. This happened in the summer of 1904. At first I thought probably a proof could easily be found; but gradually I saw that, if there is a proof, it must be very recondite.

The problems which eventually made the axiom of choice prominent were those concerning the comparability of powers and well-ordering. Cantor had always been convinced that any two transfinite powers are comparable, and so fully deserve the name 'cardinal number'. Two powers are comparable if for any sets having those powers, one is equipollent to a subset of the other. Now he had succeeded in showing that any two well-ordered sets are comparable, and hence that any two alephs are comparable, and that a transfinite power which is less than

some aleph is itself an aleph. It only remained to show that every power is an aleph, or that every set has at least one well-ordering. Now my guess is that he thought that any power must at least be comparable with the alephs (compare Hardy *1904a*, 88), and, therefore, that two incomparable powers must both be greater than any aleph. I speculate further that it was in pursuing this line of thought that he came upon the antinomies and subsequently made his distinction between consistent and inconsistent multiplicities. He then formulated the following argument, which he communicated to Dedekind in 1899, for the proposition that every transfinite power is an aleph. Suppose that the multiplicity V does not have an aleph as its power. In that case, for every ordinal α, V is not equivalent to W_α (the initial segment of the ordinals determined by α). Cantor assumes then that W is 'projectible into' V, which means that 'there must exist a sub-multiplicity V' of V that is equivalent to the system' W (*Papers*, 447; van Heijenoort *1967a*, 117). By the axiom (6.8.2) V' is inconsistent because W is, and therefore V is inconsistent by (6.8.3). Thus, if V does not have an aleph for its power it is an inconsistent multiplicity; consequently, if V is a set, then its power is an aleph.

In an editorial note to this argument in his edition of Cantor's works, Zermelo explained objections which led him to formulate his own proof. To explain why Cantor asserts that W is 'projectible' into a multiplicity V whose power is not an aleph, Zermelo supposes Cantor to have thought in terms of a procedure of successive assignments of members of V to ordinals. Zermelo's objection is that 'the intuition of time is applied here to a process that goes beyond all intuition . . .' (Cantor *Papers*, 451; van Heijenoort *1967a*, 117). But Cantor may not have intended any such thing; it is possible that his reasoning is based on the following proposition:

$$(\forall\alpha)\{\neg(V \sim W_\alpha)\} \rightarrow (\exists V')\{V' \subseteq V \,.\, \& \,.\, V' \sim W\} \qquad (6.9.1)$$

taken as an axiom.

Zermelo remarks that the theorem which Cantor wished to prove could only be established by means of the axiom of choice 'which postulates the possibility of a *simultaneous* choice . . .'. But how it would be possible to make so many arbitrary choices 'simultaneously' is not evident. Actually Zermelo did not understand his axiom to assert anything about the possibility of choices. Indeed, Sierpinski quotes Zermelo as having said in a letter that the formulation of the axiom in terms of choice 'concerns only the psychological method of presentation, while the axiom, as its wording by the way makes sufficiently clear, should be regarded as a pure axiom of existence' (*1965a*, 96). Cantor might well have attempted to defend his argument in some such terms.

Had he done this, Zermelo could have brought into play his much more serious objection to Cantor's argument—the objection to the employment of inconsistent multiplicities. Surely the purported mention of inconsistent multiplicities cannot occur in an axiom.

Zermelo's own proof in *1904a* that every set has at least one well-ordering, which implies the comparability of powers, was constructed in accordance with the following requisites. The proof was to avoid 'not only all notions that were in any way dubious [such as that of an inconsistent multiplicity] but also the use of ordinals in general'; also, only 'principles and devices that have not yet by themselves given rise to any antinomy' were used (Zermelo *1908a*, art. 2, sect. c; van Heijenoort *1967a*, 192). The idea of using the axiom of choice to prove the well-ordering theorem was due to Erhard Schmidt. The form of the axiom in Zermelo *1904a* is: for any set M, there is at least one mapping γ such that for each non-empty subset M' of M, $\gamma(M')\in M'$.

The axiom of choice has many important consequences in set theory. It is used in the proof that every infinite set has a denumerable subset, and in the proof that every set has at least one well-ordering. From the latter, it follows that the power of every set is an aleph. Since any two alephs are comparable, so are any two transfinite powers of sets. The axiom of choice is also essential in the arithmetic of transfinite numbers. For example, it is needed to prove that the cardinal of the union of α disjoint sets each having β elements is $\alpha\times\beta$.

The axiom also plays a role in various parts of Weierstrassian analysis (see Sierpinski *1918a*, and Grattan-Guinness *1977a*, *passim*), whose development was described in sections 3.11–3.14. Here are some of its uses: to prove that a limit-point of a set is an accumulation point; to prove that every field has an algebraic closure which is unique (up to isomorphism); to construct non-measurable sets; and to prove the Bolzano–Weierstrass theorem, if it states that an infinite (in the non-inductive sense) set has a limit-point (as opposed to an accumulation point).

After Zermelo's proof of the well-ordering theorem in *1904a*, the proof and the axiom on which it was based became the subject of a considerable amount of controversy (see Zlot *1960a*; and Fraenkel, Bar-Hillel and Levy *1973a*). In *1908a* Zermelo published a new proof of the well-ordering theorem and answered the criticisms directed against the first proof and the axiom of choice. One reason for the disagreements was the fact that not everyone understood the axiom in the same way. Thus someone to whom it seems an evident truth might well grant that it is quite doubtful when interpreted in a different way. But metaphysical convictions determine what a given author considers to be the possible interpretations.

Let us first consider Peano's remarks on the axiom of choice, which he understood to mean 'that we may arbitrarily choose an infinite number of elements' (*1906a*, art. 1; *1973a*, 207). He pointed out that he had already rejected this as a principle of inference in 1890. The only objection which he mentioned to the axiom of choice was that it is not provable from the axioms of his system of logic, which he apparently considered as definitive of the concept of proof: 'In some cases we do not know how to eliminate the postulate of Zermelo. Then these proofs are not reduced to the ordinary forms of argument, and the proofs are not valid, according to the ordinary meaning of the word "proof"' (*1973a*, 210). In a letter written to Russell in 1906, Peano expressed his point as follows: 'this form of reasoning is not reducible to the usual forms (for example, to those contained in pages 1–14 of the *Formulaire*, vol. 5 [*1908a*]); and to prove a proposition means to deduce it from known propositions by the usual forms of reasoning, without adding new principles' (Kennedy *1975a*, 209). Peano considered the question of the truth or falsehood of the axiom of choice to be of no consequence (*1973a*, 210). Thus he did not pass any judgment on the truth-value of the axiom of choice, but only on its legitimacy as a principle of demonstration.

What did Zermelo say to this? He had stated in *1904a* that the principle 'cannot, to be sure, be reduced to a still simpler one' (van Heijenoort *1967a*, 141). But he did not consider, as Peano apparently did, that the system of mathematical principles was already complete. Moreover, the axiom of choice emerged in the same way as the principles included in Peano's system must once have done, by analysis of 'the modes of inference that in the course of history have come to be recognised as valid'. It is also justified in the same way, namely, 'by pointing out that the principles are intuitively evident and necessary for science' (*1908a*, art. 1; van Heijenoort *1967a*, 187). The very extensive implicit use of the axiom of choice by many mathematicians could, Zermelo said, 'be explained only by its *self-evidence* . . . No matter if this self-evidence is to a certain degree subjective—it is surely a necessary source of mathematical principles, even if it is not a tool of mathematical proofs, and Peano's assertion [*1973a*, 210] that it has nothing to do with mathematics fails to do justice to manifest facts' (*ibid.*).

Russell was one of those who had implicitly used the axiom of choice (or the multiplicative axiom) in arguments; as we have already seen, when he first became aware of the latter as a proposition which had not yet been proved, he thought it must be provable. Perhaps he had thought this because the proposition seemed evident and, as Zermelo would say, 'necessary for science'. But, unlike Zermelo, Russell, after realising that the axiom was probably independent of the system of assumptions he had made so far, became sceptical about the axiom of choice and its equivalents. Fortunately, he explained quite clearly the source of his doubt.

Russell's first publication dealing with the multiplicative axiom is his *1906a*. Although he opens his discussion of the axiom by presenting the difficulty as one about the possibility of making an infinite number of arbitrary choices, the real point at issue is the *existence* of a selection set for each class *k* of mutually exclusive, non-empty sets : ' What is required is not that we should actually be able to pick out one term from each class which is a member of *k*, but that there should be (whether we can specify it or not) at least one class composed of one term from each member of *k* ' (Russell *1906a*, 48 ; *1973a*, 158). Now because of what he meant by a class it seemed doubtful to Russell that there always is a selection set. He conceived of a class as something determined by a property or propositional function : If no property, then no class. Thus, from this point of view, ' what we are primarily in doubt about is the existence of a norm or property such as will pick out one term from each of our aggregates ; the doubt as to the existence of a *class* which will make this selection is derivative from the doubt as to the existence of a norm ' (Russell *1906a*, 52 ; *1973a*, 162–163).

Now the multiplicative axiom does seem to be as evident as any of the other axioms of set theory on the pure extensional concept of set. Moreover, as Gödel has said (with this concept in mind), ' nothing can express better the meaning of the term " class " than the axiom of classes and the axiom of choice ' (*1944a*, 151). But with Russell's concept of a class as something dependent on a property, the multiplicative axiom really is doubtful. For then it amounts to the assertion that for any class *k* of pairwise disjoint, non-empty classes, there is at least one property possessed by exactly one element from each member of *k* and by no other things. It would be quite possible to agree with Russell's opinion that ' this is not at all obvious ' (*1911a*, 33) and yet think the multiplicative axiom an evident truth, by taking sets in the purely extensional sense to be the objects of set theory. His metaphysical convictions prevented him from doing this, but it is interesting to note that later he was persuaded for a while by Frank Ramsey and Henry Sheffer to assert the truth of the multiplicative axiom (Russell *1927a*, 299 ; compare Ramsey *1931a*, 58).

What was Zermelo's concept of set ? Unfortunately, he made no positive statement, but that he did not conceive sets as extensions of properties (as Russell did) is suggested by a couple of passages in his writings (van Heijenoort *1967a*, 189, last para. ; Cantor *Papers*, editorial note on p. 442). It remains possible that Zermelo intended his system to concern the purely extensional concept of set.

6.10. *Some concluding remarks*

What kinds of conclusion can we draw from such a miscellany of studies and techniques ? Perhaps two main points will suffice. Firstly, the introduction of set theory into mathematics and propositional functions into logic brought these two topics into newly intimate contact. Russell and Frege saw the connection as so close that they espoused a doctrine of ' logicism '—that mathematics (for Frege, only arithmetic) was a branch of logic. These forms of logicism are not normally asserted today, but the location of the dividing line between logic and mathematics is still a controversial matter. Secondly, the development of meta-mathematics by Hilbert and the distinction between use and mention by Frege (though, unfortunately, not by Russell) led mathematicians and philosophers to see the profound importance of the distinction between theory and meta-theory in the study of the foundations of logic and mathematics.

These remarks largely refer to the later developments in foundational studies. They lie outside the time-period of this book, which now draws to its close. A fitting conclusion to the book is provided by a return to the 17th century, where the final words of Descartes's *La géométrie* (*1637a*) may apply here also :

> But it is not my purpose to write a large book. I am trying rather to include much in a few words, as will perhaps be inferred from what I have done . . .
>
> I hope that posterity will judge me kindly, not only as to the things of which I have explained, but also as to those which I have intentionally omitted so as to leave to others the pleasure of discovery.

From the Calculus to Set Theory, 1630–1910

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Princeton University Press
Princeton and Oxford