Infinity

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Abstract

This essay surveys the different types of infinity that occur in pure and applied mathematics, with emphasis on:

(i) the contrast between potential infinity and actual infinity;
(ii) Cantor's distinction between transfinite sets and absolute infinity;
(iii) the constructivist view of infinite quantifiers and the meaning of constructive proof;
(iv) the concept of feasibility and the philosophical problems surrounding feasible arithmetic;
(v) Zeno's paradoxes and modern paradoxes of physical infinity involving supertasks.

Keywords: actual infinity, potential infinity, transfinite set theory, Cantor, constructivism, intuitionism, proof, feasibility, physical infinity, Zeno's paradoxes, supertasks.

1. Introduction

Infinity occurs in many shapes and forms in mathematics. The points at infinity in projective geometry are very different from the infinite and infinitesimal quantities that occur in nonstandard analysis, or the transfinite numbers in set theory, or the infinity involved in a limiting process $\lim_{n\to\infty} a_n$. To classify this variety, it is helpful to distinguish between *actual* infinity and *potential* infinity (the distinction originates with Aristotle, who however meant it in a narrower sense than we do today (Lear, 1979–80)). According to the idea of actual infinity, infinite and finite quantities are subsumed under the same theory; an actually infinite quantity is just like a finite quantity, only bigger. According to the idea of potential infinity, infinity is merely a figure of speech: whenever we speak of infinity we are really talking about arbitrarily large finite quantities.

For example, the concept of natural number can be understood in terms of a generating process $0 \mapsto 1 \mapsto 2 \mapsto 3 \mapsto \cdots$, which can be continued as long as one pleases without coming to an end; this is potential infinity. Or it can be understood in terms of the set of all natural numbers $\mathbf{N} = \{0, 1, 2, 3, \ldots\}$, thought of as given once and for all; this is actual infinity. At first sight it may appear as if these two views come to the same thing, but we shall see in §3 that the difference is fundamental and its consequences pervasive.

To take another example, consider the sum of an infinite series, $\sum_{n=1}^{\infty} a_n$. From an actual-infinity viewpoint this would be understood as an addition of infinitely many quantities (think for example of infinitely many blobs of water that are lumped together into a single big blob); the total may of course be finite or infinite. Such a conception has some appeal in the case of physical quantities, as we shall see in §4.4. Similarly, an integral $\int_a^b f(x)\,dx$ can be seen as the addition of infinitely many infinitesimal areas $f(x)\,dx$. From this perspective, the three equations $\sum_{n=1}^{N} a_n + b_n = \sum_{n=1}^{N} a_n + \sum_{n=1}^{N} b_n$, $\sum_{n=1}^{\infty} a_n + b_n = \sum_{n=1}^{\infty} a_n + \sum_{n=1}^{\infty} b_n$, and $\int_a^b f(x) + g(x)\,dx = \int_a^b f(x)\,dx + \int_a^b g(x)\,dx$ are instances of the same rule (as the notation suggests).

This is not, however, the way infinite series are conventionally understood in modern mathematics. $\sum_{n=1}^{\infty} a_n$ is not viewed as the sum of infinitely many terms but as a limit of *finite* sums, $\sum_{n=1}^{N} a_n$; the apparent reference to infinity (∞) is explained away in finite terms. Likewise $\int_a^b f(x)\,dx$ is defined in terms of the supremum and infimum of finite sums of areas; the apparent reference to infinitesimals (dx) is explained away in terms of finite intervals. This explaining away of apparent references to infinity in finite terms is characteristic of potential infinity. The above three equations are regarded as separate rules requiring separate proofs. The rigorous analytic theory of limits, infinite sums, differentiation and integration developed in the nineteenth century banished infinitesimals and infinite quantities in favour of arbitrarily small positive numbers ε and arbitrarily large natural numbers n. This certainly represents a supplanting of actual infinity by potential infinity. However, underlying this use of potential infinity are two uses of actual infinity: the concept of an infinite set (primarily the set of all natural numbers, **N**, and the set of all real numbers, **R**) and the concept of infinite quantifiers ('for all x, ...', 'there exists a δ ...').

Potential infinity and actual infinity are intertwined in modern mathematics. For example, we nowadays think of a line as something extending to infinity in both directions (unlike the ancient Greeks, who thought of a line as a line segment, indefinitely extendible in either direction). Nevertheless, each point on the line has a finite coordinate; no point has an infinite coordinate. The phrase 'going to ∞' refers to a direction, whereas 'going to 100' refers to a destination.

The idea of treating the infinite on the same footing as the finite, which is characteristic of actual infinity, is often very fruitful in mathematics. An obvious example is the points at infinity in projective geometry and complex analysis. But the same idea motivates concepts such as that of a Hilbert space. When one tries to generalise the theory of finite-dimensional vector spaces to infinitely many dimensions, one finds that many useful concepts and theorems break down. A possible reaction to this would be to say that infinite-dimensional vector spaces are fundamentally different from finite-dimensional ones, and so perhaps the general concept of vector space is not very useful. This would be hasty, however. It turns out that by imposing a few extra technical conditions (completeness and separability, and by considering closed linear subspaces rather than arbitrary linear subspaces), much of the finite-dimensional theory does generalise: for example, the notion of the closest point to a (closed) linear subspace, the idea of a basis-free treatment of the space, orthonormal expansions, the characterisation of a separable Hilbert space by its dimensionality, and the isomorphism between a Hilbert space and its dual space. It turns out that the distinction that matters is not the one between a finite-dimensional and an infinite-dimensional space (as we first thought) but the one between a Hilbert space (or possibly a separable Hilbert space) and other inner-product spaces. The mathematical pay-off of this is that it enables us to treat function spaces almost as if they were finite-dimensional.

Another example of this phenomenon is the concept of a compact set in topology. Finite point sets have many topological properties that do not hold for point sets in general. However, by formulating the notion of compactness the properties of finite sets can be generalised to a useful class of infinite sets. Hence there is no separate theory of finite sets in topology: it is simply subsumed in the theory of compact sets.

An advocate of actual infinity would draw a general moral from these examples: that the distinction between finite and infinite is less important than it naively seems, and often masks a deeper technical distinction; by recognising this deeper distinction we can bring the finite and the infinite under a single theory. Let us apply this thought to a philosophically controversial case: the notions of infinite set and infinite number seem very problematic at first sight, but, according to Cantor, set theory and arithmetic can be extended from the finite into the infinite by introducing the concept of the *transfinite*. The distinction between the transfinite and absolute infinity turns out to be more significant than the distinction between finite and infinite (see §2). Cantor's followers today therefore argue that acceptance of actual infinity is in the spirit of best mathematical practice. This viewpoint is typical of a platonist philosophy of mathematics.

On the other side, Brouwer's philosophy of intuitionism, and other schools of constructivism, view actual infinity with suspicion and attempt to found mathematics solely on potential infinity; they would regard this as in the spirit of best mathematical practice. Their views will be discussed in §3.

These questions of the nature of infinity in pure mathematics cannot be seen in isolation from applied mathematics and physics. It is a great defect of the literature on the paradoxes of mathematical infinity that it ignores the paradoxes of physical infinity, and vice versa. Mathematical and physical infinity are intimately interdependent in many philosophies of mathematics, including Field's (1980) nominalism, Hellman's (1989) modal structuralism, and logicism. Historically, ideas of infinitesimals and continuity arose from spatial and kinematic considerations. Even today, platonists frequently rely on infinitistic physical thought experiments to bolster their support of actual infinity. To many, it seems simply *obvious* that there could exist actual infinities in the physical world (for example, an actual infinity of stars), and they infer that actual infinity is also a coherent idea in pure mathematics (Russell, 1935–6; Benardete, 1964, p. 31). I shall examine the idea of physical infinity in §4; it turns out that it is so paradox-ridden that it counts more against actual infinity than in favour of it. However, it must be said that constructivists have so far failed to provide an alternative account of physics based on potential infinity.

In general, platonists believe that the same philosophical considerations apply to physical and mathematical infinity (Tait, 1986), whereas constructivists typically believe that different considerations apply (Dummett, 1994; Fletcher, 2002).

Whatever view you take of infinity, there is a pervasive problem you are certain to encounter, which I call the *horizon problem*. Imagine that you call yourself a 'finitist'; you proclaim that you only believe in finite things and that the infinite is illegitimate and incoherent. Someone asks you *how many* finite things you believe in. Embarrassingly, you have to admit that you believe in *infinitely many* of them. Notwithstanding all your 'finitist' rhetoric, you require a theory of infinity in order to give a global account of your mathematical worldview.

Suppose that, in the course of rising to this challenge, you come to accept that infinite collections (such as $\mathbf{N}$ and $\mathbf{R}$) are perfectly legitimate. Now, the same question recurs: *how many* infinite collections do you believe in? The answer is 'infinitely many', of course, but this latter infinity is a much larger, more unmanageable, kind of infinity than the humdrum infinities you do accept. In Cantorian terminology, the infinities you accept are called 'transfinite', and the infinity of everything transfinite is called 'absolute infinity'. To give a general account of transfinite infinity you require a theory of absolute infinity. You are worse off than when you were a finitist.

Suppose that, in desperation, you resolve to revert to finitism, and (to distance yourself as far as possible from infinity) to restrict yourself to the *feasible* finite, i.e., numbers you can actually count up to, such as 200, rather than practically inaccessible numbers such as 100^{100}. You resolve to found mathematics entirely on what is feasible. Again, the question recurs: how many feasible numbers are there? The answer, surely, is 'infeasibly many'.

All three of these positions (finitism, Cantorianism and feasibilism) suffer from the same problem: to characterise the domain of acceptable mathematical objects one has to use concepts that are not acceptable. This is the horizon problem, and it is the underlying problem of mathematical infinity; we shall encounter it repeatedly in §§2–3, though it is more acute in some theories than in others.

I had better admit my own bias here. I am a constructivist, and my views can be found in Fletcher (1998). However, in this essay I shall try to expound and criticise each theory on its own terms, without trying to reach a final judgement between them.

2. Actual Infinity

2.1 Introduction

The 'actual' view of infinity goes naturally with a general philosophy of realism. By *realism* I mean the doctrine that the purpose of our physical and mathematical theories is to describe objective reality, that the meaning of statements is given by their truth conditions, that truth is independent of our means of knowing it, and that the proper names, variables, and other 'noun-like' expressions in statements denote *objects*. From such a standpoint it is natural to suppose that there is an infinity of objects and that quantifiers allow us to express infinitely many facts about the world in a single statement. Quantifiers were invented by Frege in 1879 and Peirce and Mitchell in 1883. Peirce and Mitchell devised the notation $\Sigma_i x_i$, meaning that x_i is true for *some* value of i, and $\Pi_i x_i$, meaning that x_i is true for *all* values of i, by a conscious analogy with infinite sums and infinite products in analysis (Peirce, 1885). Nowadays we write these as $\exists i\ x_i$ and $\forall i\ x_i$, but we still (if we are realists) construe $\exists i\ x_i$ as a disjunction $x_1 \vee x_2 \vee x_3 \vee \cdots$ and $\forall i\ x_i$ as a conjunction $x_1 \wedge x_2 \wedge x_3 \wedge \cdots$ over all possible values of i. The fact that this disjunction and conjunction may contain infinitely many terms and so be impossible to compute is dismissed as irrelevant: the realist insists that the objects i exist independently of us, that the property x_i holds or fails to hold for each i irrespective of our ability to verify it, and that there is a *fact of the matter* about whether x_i is true for all, some or none of the objects i, irrespective of our ability to run through all the objects checking them.

An attractive feature of realism is that it gives us a uniform semantic account of both mathematical and nonmathematical discourse. As Benacerraf says, it allows us to give the grammatically similar sentences 'There are at least three large cities older than New York' and 'There are at least three perfect numbers greater than 17' a similar logical analysis (1973).

Nowadays, the realist view of mathematics is usually based on set theory. The choice of sets as the fundamental objects, rather than, say, functions, relations or categories, is a historical accident (e.g., functions are taken as the basic notion by von Neumann (1925), and Bell (1981) discusses the possibility of using categories); what is essential is that mathematics is seen as the study of a fixed universe of actually-infinite mathematical objects, existing independently of our ability to construct them or of any other anthropocentric considerations. A few set-theoretic realists believe that mathematics describes the same universe of objects as physical theories (Maddy, 1990; Mayberry, 2000); but most regard mathematical objects as forming a separate realm disjoint from the physical world (this variety of realism is known as *platonism*).

This platonist orthodoxy is a comparatively recent phenomenon, dating back only a century or so. Before then, it was more common to view infinity as potential infinity. It is illuminating to look at how and why the change-over to actual infinity occurred. The transition arose out of the needs of nineteenth-century mathematics, particularly the arithmetisation of analysis. Four reasons can be traced.

1. *Rejection of spatial and temporal intuition.* Newton based his ideas of limits and differentiation on intuitions of motion; other mathematicians based their ideas of continuity on spatial intuition. These kinematic and geometric conceptions fell into disfavour in the nineteenth century, as they had failed to provide satisfactory theories of negative numbers, irrational numbers, imaginary numbers, power series, and differential and integral calculus (Bolzano, 1810, preface). Dedekind pointed out that simple irrational equations such as $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$ lacked rigorous proofs (1872, §6). Even the legitimacy of the negative numbers was a matter of controversy in the eighteenth and nineteenth centuries (Ewald, 1996, vol. 1, pp. 314–8, 336). Moreover, Bolzano, Dedekind, Cantor, Frege and Russell all believed that spatial and temporal considerations were *extraneous* to arithmetic, which ought to be built on its own intrinsic foundations: 'it is an intolerable offence against *correct method* to derive truths of *pure* (or general) mathematics (i.e. arithmetic, algebra, analysis) from considerations which belong to a merely *applied* (or special) part, namely *geometry*' (Bolzano, 1817, preface). These considerations led them to reject the potential-infinity notion of a *quantity capable of augmentation without end*, with its connotations of time and change, and to replace it with the static notion of the *infinite set* of all possible values of the quantity.

2. *Quantifier combinations.* Before the nineteenth century, mathematical statements typically took the form of general laws ('$(x+y)^2 = x^2 + 2xy + y^2$') and constructions ('given any prime numbers we can construct a larger one'). These are expressible, in modern logical notation, using one or two quantifiers, and they can be understood readily in terms of potential infinity: they are all of the form 'given any numbers we can perform certain calculations, with certain results'. Bolzano (1817) pioneered the use of more logically complex statements requiring more quantifiers: for example, in modern notation, a sequence (x_n) is said to *converge* to a limit l iff

$$\forall \varepsilon > 0\, \exists N\, \forall n > N\ |x_n - l| < \varepsilon;$$

and a function $f\colon \mathbf{R} \to \mathbf{R}$ is said to be *continuous* iff

$$\forall a\, \forall \varepsilon > 0\, \exists \delta > 0\, \forall x\ |x - a| < \delta \Rightarrow |f(x) - f(a)| < \varepsilon.$$

Such statements are much harder to understand in terms of potential infinity. The quantified variables all vary in a complex interdependent way over an infinite range of values – it is hard to state this without thinking of the value-ranges as provided once and for all, as actually-infinite sets.

3. *Infinite sets as single objects.* The arithmetic theories of real numbers developed by Weierstrass, Cantor and Dedekind represented each individual real number by an infinite object (either a sequence of rationals or a Dedekind cut of rationals). Dedekind himself did not actually *identify* a real number with the corresponding Dedekind cut, but others did take this natural step (1888a). The set of real numbers, $\mathbf{R}$, is then an infinite set of elements that are themselves essentially infinite. In the light of this it is hard to maintain the view that there is no such thing as actual infinity, only unboundedly varying finite quantities.

4. *Cantor's set theory.* Cantor was drawn into set theory by his investigations of Fourier series (Dauben, 1979, chapter 2). He began by studying the representation of functions by trigonometric series, then he turned his attention to the sets of points on which such representations break down; this led him to study sets of points for their own sake, and then sets in general. For Cantor, a set was no longer simply the range of variation of a variable quantity; it had become a mathematical object in its own right.

Many forms of actual infinity are in use in mathematics today, for example, points at infinity in complex analysis and projective geometry, and infinitesimals in nonstandard analysis (Robinson, 1966) and smooth infinitesimal analysis (Kock, 1981; Bell, 1998). Many others have been proposed and not received as much attention as they deserve, such as Peirce's theory of the continuum (Zink, 2001) and Vopěnka's alternative set theory (Vopěnka, 1979; Sochor, 1984). Nevertheless, in this section I shall concentrate on the most important variety, Cantor's theory of transfinite sets and transfinite arithmetic. I shall restrict my attention to those aspects of set theory most relevant to infinity, neglecting other controversial aspects such as: the existence of sets and whether they are material or abstract objects; the existence of the empty set and singleton sets, and the comparison between set theory and mereology (for which see Lewis (1991)); the axiom of foundation and non-well-founded set theory (Aczel, 1988); and the axiom of choice (Moore, 1982).

2.2 Infinite numbers

Cantor's boldest achievement was to extend the concept of number into the infinite, with his theory of transfinite ordinal numbers and transfinite cardinal numbers. Let us take the idea of cardinal number and try to isolate what is distinctive about Cantor's contribution.

There are two natural criteria by which one could compare the 'size' or 'multiplicity' of two classes, A and B (I am using the word 'class' in a rough, pre-Cantorian sense for the moment).

(1) There are fewer As than Bs if every A is a B but not every B is an A.
(2) There are as many As as Bs if there exists a bijection between the As and the Bs (i.e., a one-to-one correspondence, or a one-to-one mapping of all the As onto all the Bs).

Both these criteria were familiar since antiquity and were generally accepted for finite classes. But it was well known that they came into conflict when one tried to apply them to infinite classes, and this was sometimes used as an argument against infinite numbers. Leibniz said:

> The number of all numbers implies a contradiction, which I show thus: To any number there is a corresponding number equal to its double. Therefore the number

of all numbers is not greater than the number of even numbers, i.e. the whole is not greater than its part. (Quoted in Benardete, 1964, p. 44.)

The key step in resolving this conflict is to *separate* the two criteria. Maybe they are both valid, but they refer to different concepts of size. If we develop the consequences of the two criteria separately we might arrive at two valid theories of size, both applicable to infinite classes.

Thus Bolzano explicitly adopted (1) as his criterion of size and rejected (2) in the case of infinite classes (1851, §§19–24). Bolzano's paper is notable for upholding the existence of actual infinity and for insisting that one infinity could be larger than another (for example, a line, bounded in one direction and unbounded in the other, can contain another such line (§19)). His rejection of criterion (2) led him into extraordinary complications when trying to evaluate sums of infinite series (§18). Nevertheless, his criterion of size has been incorporated into modern set theory in the form of the proper subset relation, and whenever we speak of, e.g., 'a maximal orthonormal set' or a 'maximal connected subset' we are using the word 'maximal' in Bolzano's sense.

Cantor, of course, adopted criterion (2) and replaced (1) with a weaker version.

(1′) There are fewer or as many As as Bs if every A is a B.

By combining the two ideas (1′) and (2) we obtain the modern concept of the *cardinality*, $|A|$, of a class A:

$$\begin{aligned} |A| = |B| &\quad \text{iff there is a bijection between } A \text{ and } B; \\ |A| \leq |B| &\quad \text{iff there is a bijection between } A \text{ and a subclass of } B; \\ |A| < |B| &\quad \text{iff } |A| \leq |B| \text{ but } |A| \neq |B|. \end{aligned}$$

If $|A| = |B|$ then A and B are said to have the same *power*, or the same *cardinal number*. Beginning with his (1874) paper, Cantor accomplished cardinal comparisons between the set of natural numbers ($\mathbf{N}$), the set of real numbers ($\mathbf{R}$), the set of algebraic numbers, the set of irrational numbers, the one-dimensional interval $[0, 1]$, and the two-dimensional square $[0, 1]^2$. He showed that there is an unending succession of infinite cardinal numbers, of which the smallest is $|\mathbf{N}|$, known as *countable* infinity. In parallel with this, Cantor developed a theory of transfinite *ordinal* numbers, $0, 1, 2, \ldots \omega, \omega + 1, \omega + 2, \ldots \omega 2, \omega 2 + 1, \omega 2 + 2, \ldots$, thinking of them initially as steps in a transfinite iteration and later as order-types of well-ordered sets. A *well-ordered set* is a set equipped with an ordering relation under which every non-empty subset has a least element. Two well-ordered sets are said to be *similar*, or *of the same order-type*, iff there exists a similarity between them (a similarity is an order-preserving bijection). Similarities are the measuring stick for ordinal numbers, just as bijections are the measuring stick for cardinal numbers.

Cantor defined operations of addition, subtraction, multiplication, division and exponentiation for ordinal numbers and investigated their unique prime factorisation. He also defined addition, multiplication and exponentiation for cardinal numbers, and used ordinal numbers to index the sequence of cardinal numbers: the sequence of infinite cardinal numbers could be listed as $\aleph_0, \aleph_1, \aleph_2, \ldots, \aleph_\omega, \aleph_{\omega+1}, \ldots$, with every infinite cardinal number occurring in the list as $\aleph_\alpha$ for a unique ordinal number α. He developed a distinctive concept of *set*, under which every set had a cardinal number and every well-ordered set had an ordinal number.

These achievements made an immense impression on Cantor's successors. By the simple expedients of taking actual infinity seriously and adopting bijections and similarities as measures of size, Cantor had replaced the mediaeval metaphysical morass of infinity with determinate mathematics. The fact that one can do *calculations* with infinities, one can show that $5(\omega + 1)(\omega^2 3 + 4) = \omega^3 3 + \omega 4 + 5$ and $\aleph_{37} \times \aleph_{20} = \aleph_{37}$, convinced mathematicians that transfinite numbers were just as real as finite numbers and that Cantor's theory represented a genuine advance over all previous thinking about infinity. Mathematicians have a strong sense of the *reality* and *concreteness* of anything they can calculate with. A few mathematicians (Kronecker, Baire, Brouwer, Poincaré, Weyl) resisted the move to actual infinity; the great majority applauded it. (However, it might be argued that if they had known at the time how intractable the evaluation of cardinal exponentials such as $2^{\aleph_0}$ was they might have been less willing to embrace transfinite arithmetic.)

2.3 The paradoxes of set theory

Even as set theory was finding its feet and actual infinity was establishing itself as mathematical orthodoxy, serious contradictions were discovered at the heart of set theory, which were seen at the time as a 'crisis' in the foundations of mathematics. Set theorists at this time (the beginning of the twentieth century) assumed a *comprehension principle*:

$$\exists S\, \forall x\, (x \in S \Leftrightarrow P(x))$$

which says that, given a well-defined property P applicable to objects of any kind, there exists a set S consisting of all the objects x for which $P(x)$ holds. The motivation for this was the commonly held belief that consistency implies existence. The view was that, if one defines a mathematical system by specifying the types of elements it contains, the relations and functions that apply within the system, and the axioms that characterise them, and if one does so in a rigorous, unambiguous, consistent and complete manner (that is, if the axioms determine unambiguously whether each relation holds between any two elements and the value of each function on any argument), then the system is guaranteed to exist. (This doctrine is nowadays known as 'plenitudinous platonism' (Balaguer, 1998).) Now, a set is a mathematical system of a very simple kind, having only one relation, $\in$; and so if we specify unambiguously what it means for an arbitrary object x to belong to the set then we have done enough to guarantee the set's existence. This is the justification for the comprehension principle.

This belief that consistency implies existence arose from the successes of nineteenth-century mathematics in generalising algebra to ever-larger number systems and establishing Euclidean and non-Euclidean geometry on a firm axiomatic footing. Dedekind believed that mathematical objects were 'free creations of the human mind', that human beings possess 'divine' powers to create any well-defined mathematical system (1888a, b). Cantor believed that any consistently and precisely defined mathematical system was guaranteed to occur in the external world (1883, §8). Poincaré (1905) and Hilbert (1900) believed that, in mathematics, existence simply *is* consistency. Frege provided an explicit articulation of this idea, in the form of his 'context principle' (1884, §60; 1919), which states that the meaning of a word consists in the contribution it makes to the meanings of the sentences in which it may occur; if this contribution can be specified adequately then the word is guaranteed to have reference. He applied the context principle in his *Grundgesetze* (1893, §§3,10) to fix the meaning of the course-of-values of a function; he argued that to do this it sufficed to provide an equality criterion for courses-of-values; this was his Basic Law V, his equivalent of the comprehension principle.

The set-theoretic paradoxes arise from the comprehension principle. Burali-Forti's paradox appeared in 1897 (in a disguised form). Let P be the property of being an ordinal number; S is therefore the set of all ordinal numbers. Then it is clear that S is well-ordered, by the usual ordering of ordinals. Every well-ordered set has an ordinal number as its order-type, so let Ω be the order-type of S. Now, Ω must be greater than all the ordinal numbers in S; but this contradicts the fact that S contains all ordinal numbers.

Around 1900–2, Zermelo and Russell independently discovered what is generally known as 'Russell's paradox'. Let P be the property that holds of x iff x is a set and $x \notin x$. Then S is the set of all sets that do not belong to themselves. The definition immediately gives $S \in S \Leftrightarrow S \notin S$, a logical contradiction (even in intuitionistic logic).

'Cantor's paradox' (essentially the argument in Cantor (1899c)) arises by taking P as the property of being a set. Thus S is the set of all sets. Let $\mathcal{P}(S)$ be the power set of S, i.e., the set of all subsets of S. Then S is smaller in cardinality than $\mathcal{P}(S)$, by Cantor's diagonal argument (1891); but this contradicts the obvious fact that $\mathcal{P}(S) \subseteq S$.

After much confusion and controversy, it came to be generally agreed that the fallacy in each of these arguments was the application of the comprehension principle: there is no set of all ordinal numbers, no set of all sets that are not members of themselves, and no set of all sets. It was concluded that the comprehension principle was unsound, at least in its full generality. Yet some principle of this sort is necessary if we are ever to be able to claim the existence of any set or other mathematical system. So, lacking a clear understanding of what was wrong with the comprehension principle, set theorists took the line of least resistance and assembled lists of weaker principles that would allow them to carry out all the normal operations of mathematics without reproducing the three paradoxes. This led to an axiomatic theory called ZFC (Zermelo-Fraenkel set theory with the axiom of choice), which is nowadays considered the standard version of set theory. The axioms are as follows.

AXIOMS FOR BASIC SETS

Axiom of the empty set ($\emptyset$ exists): $\exists y \forall z\, z \notin y$

Axiom of infinity (there exists an infinite set): $\exists y\, (\emptyset \in y \wedge \forall z \in y\; z \cup \{z\} \in y)$

SET-BUILDING AXIOMS

Axiom of pairing ($\{x_1, x_2\}$ exists): $\forall x_1, x_2 \exists y \forall z\, (z \in y \Leftrightarrow (z = x_1 \vee z = x_2))$

Axiom of union ($\bigcup x$ exists): $\forall x \exists y \forall z\, (z \in y \Leftrightarrow \exists w \in x\; z \in w)$

Axiom of power set ($\mathcal{P}(x)$ exists): $\forall x \exists y \forall z\, (z \in y \Leftrightarrow z \subseteq x)$

Axiom of separation ($\{z \in x \mid P(z)\}$ exists): $\forall x \exists y \forall z\, (z \in y \Leftrightarrow (z \in x \wedge P(z)))$

Axiom of replacement ($\{F(u) \mid u \in x\}$ exists): $\forall x \exists y \forall z\, (z \in y \Leftrightarrow \exists u \in x\; z = F(u))$

GENERAL AXIOMS ABOUT SETS

Axiom of extensionality ($x = y$ iff x and y have the same elements):
$\forall x, y\, (x = y \Leftrightarrow \forall z\, (z \in x \Leftrightarrow z \in y))$

Axiom of foundation (there are no infinite membership chains $a_0 \ni a_1 \ni a_2 \ni \cdots$):

$\forall x \neq \emptyset\ \exists y \in x\ x \cap y = \emptyset$

Axiom of choice (for any set x of non-empty disjoint sets, there is a set w containing *one* element from each set in x):

$\forall x\ ([\forall y, z \in x\ y = z \Leftrightarrow y \cap z \neq \emptyset] \Rightarrow \exists w \subseteq \bigcup x\, \forall y \in x\, \exists u\ w \cap y = \{u\})$

(In the axiom of separation P is any property of sets; in the axiom of replacement F is any operation that transforms sets to sets; for the sake of definiteness it is common to assume that P and F are expressible by means of formulae in the first-order language of ZFC.) Most of these axioms were introduced by Zermelo (1908); the axioms of replacement and foundation were added later by others.

I have grouped the axioms into three classes to suggest a quasi-constructive way of 'generating' the ZFC universe of sets. We start with the basic sets (the empty set and the given infinite set), and apply the set-building axioms repeatedly to construct further sets. All the set-building axioms are of the form 'given a set x (or two sets x_1, x_2) we can construct another set y from it'. The construction process is subject to the conditions imposed by the 'general' axioms: two sets are considered equal iff they have the same elements; no set may involve an infinite regress or a vicious circle of membership; and it is always possible to make simultaneous choices of elements from any number of non-empty sets. This quasi-constructive view is known as the *iterative conception of sets*; it will be discussed further in §2.5.

2.4 Cantor and absolute infinity

The paradoxes of set theory were less paradoxical to Cantor than to his contemporaries. When Cantor was made aware of the paradoxes he was able to point out that he had anticipated something of this sort and made allowance for it in his (1883) paper. Indeed, he had drawn a distinction between the *transfinite* and *absolute infinity*. Infinite sets are transfinite: they 'can be determined by well-defined and distinguishable *numbers*' (1883, §5). The sequence of all ordinal numbers is absolutely infinite, and exceeds all rational grasp: 'the absolute can only be acknowledged but never known – and not even approximately known' (1883, endnote 2). In later years he expressed this in terms of a distinction between 'consistent multiplicities' (of transfinite size) and 'inconsistent multiplicities' (of absolutely infinite size). As I have already said, Cantor shared the widespread assumption that any consistent mathematical system exists; but he had a novel conception of consistency. He believed that for a system to be consistent it is necessary not merely that its elements and their mutual relations be consistently specified but that the elements be able to coexist consistently as a whole.

> If we start from the notion of a definite multiplicity (a system, a totality) of things, it is necessary, as I discovered, to distinguish two kinds of multiplicities (by this I always mean *definite* multiplicities).
>
> For a multiplicity can be such that 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'. Such multiplicities I call *absolutely infinite* or *inconsistent multiplicities*.
>
> As we can readily see, the 'totality of everything thinkable', for example, is such a multiplicity; later still other examples will turn up.

> If on the other hand the totality of the elements of a multiplicity can be thought of without contradiction as 'being together', so that they can be gathered together into '*one* thing', I call it a *consistent multiplicity* or a 'set'. (Cantor, 1899a)

> *Thus there exist* determinate multiplicities that are *not also unities* – i.e. multiplicities such that a real 'coexistence of all their elements' is *im*possible. These are the ones I call 'inconsistent systems'; the others I call 'sets'. (Cantor, 1899c)

It sounds from this as if a set is a special kind of multiplicity; to understand sets we first need a theory of multiplicities, then we need a dynamical theory explaining how elements are 'gathered' into multiplicities and how they sometimes form stable wholes and sometimes fall apart under the mutual repulsion of their elements (like an unstable atomic nucleus). But this, I think, would be to take the talk of multiplicities and gathering too literally. 'Multiplicities', 'systems' and 'totalities' are merely figures of speech; sets are the only kind of collective object that Cantor believes in. A set is simply a single object containing other objects. When Cantor says that 'The system Ω of all [ordinal] numbers is an inconsistent, absolutely infinite multiplicity', he is saying that the ordinal numbers do not form any sort of collective object at all: they are just too numerous.

Thus Cantor replaced the traditional distinction between the finite and the infinite by a distinction between the finite, the transfinite and the absolutely infinite. The transfinite includes all the infinite sets commonly used in mathematics (such as $\mathbf{N}$, $\mathbf{R}$ and separable Hilbert spaces), but it also shares many of the properties of the finite: it is numerically determinate, humanly graspable, 'limited', 'increasable' and 'finished'. The absolutely infinite is beyond all rational determination and is not itself an object of mathematical study. In Cantorian set theory the transfinite/absolute distinction assumes central importance and plays a similar role to that traditionally played by the finite/infinite distinction; whereas the finite/transfinite distinction fades into relative unimportance. The theory of cardinal and ordinal arithmetic is developed in a uniform way for all sets, and the finite part of it emerges merely as a special case, not receiving a different foundational treatment from the transfinite part (Hallett calls this point of view 'Cantorian finitism' (1984)). Mayberry emphasises the point further by applying the word 'finite' to all finite and transfinite sets (2000). In Mayberry's terminology, Cantor did not develop a theory of infinite sets but *extended the finite* to cover sets such as $\mathbf{N}$ and $\mathbf{R}$ that had traditionally been conceived as infinite. (I shall not follow this terminology, however, as I wish to retain the use of the word 'finite' in the usual sense of 'smaller than $\aleph_0$'.)

It is clear from this how fundamental Cantor's distinction between the transfinite and absolute infinity is to his project; it is not just a device for avoiding the paradoxes. But how soundly based is the distinction? In particular, what prevents us from treating an absolutely infinite 'multiplicity' as a definite object? Cantor does after all describe an absolutely infinite multiplicity as 'fully-determinate, well-defined' (1899c), meaning I think that it has a well-defined criterion for membership. But he believes it is not *numerically* determinate, meaning that it is too big to have a cardinality. Yet an obvious objection arises: why can we not compare absolutely infinite multiplicities with respect to cardinality, just as we do with transfinite sets? Cantor showed that there was a bijection $\alpha \mapsto \aleph_\alpha$ between the ordinal numbers and the infinite cardinal numbers; does this not establish that the multiplicity of ordinal numbers is of the same size as the multiplicity of infinite cardinal numbers? Moreover, in modern set theory with the global axiom of choice we can show that all absolutely infinite multiplicities are of equal cardinality to V, the multiplicity of all sets. Hence there seems to be a well-defined cardinality common to all absolutely infinite multiplicities.

Now, Cantor had defended the introduction of actual infinity against the objections of Aristotle by saying that 'determinate countings can be carried out just as well for infinite sets as for finite ones, provided that one gives the sets a determinate law that turns them into *well-ordered* sets' (1883, §4). In other words, the fact that one can carry out cardinal and ordinal comparisons in a mathematically crisp and unambiguous way, free of any arbitrariness, demonstrates the validity of infinite sets and transfinite arithmetic. But the very same considerations suggest that there is a determinate cardinality of all sets and an order-type of all ordinal numbers, which implies a higher realm of 'super-sets' of absolutely infinite size. A Cantorian must reject this conclusion, for it subverts the whole idea of absolute infinity as exceeding all mathematical determination. Yet the Cantorian seems to be without resources to resist the argument.

Dummett sets out the difficulty in the following terms. The idea of a transfinite number seems self-contradictory when one first meets it, he says, as one is used to thinking of a number as something that can be arrived at by counting. However, the beginner can be persuaded that it makes sense to speak of infinite sets of different sizes.

> When he [the beginner] has become accustomed to this idea, he is extremely likely to ask, "How many transfinite cardinals are there?" How should he be answered? He is very likely to be answered by being told, "You must not ask that question." But why should he not? If it was, after all, all right to ask, "How many numbers are there", in the sense in which "number" meant "finite cardinal", how can it be wrong to ask the same question when "number" means "finite or transfinite cardinal"? A mere prohibition leaves the matter a mystery. It gives no help to say that there are some totalities so large that no number can be assigned to them. We can gain some grasp of the idea of a totality too big to be counted, even at the stage when we think that, if it cannot be counted, it does not have a number; but, once we have accepted that totalities too big to be counted may yet have numbers, the idea of one too big even to have a number conveys nothing at all. And merely to say, "If you persist in talking about the number of all cardinal numbers, you will run into contradiction" is to wield the big stick, not to offer an explanation. (Dummett, 1994)

Clearly we need a further account of the difference between consistent and inconsistent multiplicities to extricate ourselves from this difficulty. I shall turn to this in the next subsection.

2.5 Three views of sets

We are seeking an account of how a 'multiplicity' of objects can be joined together into a single object, a 'set'. There are three main approaches:

- sets as *classes*;
- the *limitation of size* view;
- the *iterative conception of sets*.

By a *class* I mean a property, viewed extensionally. For example, the property of being a subset of $\mathbf{R}$ is (let us suppose) fully determinate: it is determined which objects possess the property and which don't. We can say that x belongs to the class $\mathcal{P}(\mathbf{R})$ iff x satisfies this property; talk of classes is just an extensional idiom for talk of satisfying properties. We refer to two classes as 'equal' iff they have the same elements, even though the two properties may be different. By forming the property 'subset of $\mathbf{R}$' we have enabled ourselves to refer, in a single breath, to infinitely many things (all the subsets of $\mathbf{R}$); we have gathered them, mentally, into a unity, which is the essential requirement for a set.

But what is a property? Ambiguity on this point vitiated Russell's account of the paradoxes in terms of 'propositional functions' (1906). If our explanation of sets in terms of properties is to achieve any real reduction, we must understand properties syntactically, as given by sentences containing a single free variable, e.g., 'x is a subset of $\mathbf{R}$'. There is no restriction on the language; any meaningful declarative sentence with one free variable is considered to denote a property. This was Frege's notion of a *concept* (1891).

How do we escape the paradoxes, on this view? The properties expressed by the sentences 'x is an ordinal number', 'x does not belong to x', and 'x is a class' must somehow be rejected. It is not plausible to say that the sentences are not meaningful: given a particular object, we *do* know what it means to say that it is an ordinal number, or that it does not belong to itself, or that it is a class. We could, however, say, with Russell, that 'a statement about x cannot in general be analysed into two parts, x and what is said about x' (1906). That is, if one takes a sentence of the form 'A does not belong to A', and tries to analyse it into a part A and a schema 'x does not belong to x', this works syntactically (i.e., the sentence is recoverable by substituting A back into the schema), but it somehow fails semantically. The proposition expressed by the sentence just cannot be carved up that way. This is all rather mysterious.

A more hopeful line is to assimilate the set-theoretic paradoxes to the semantic paradoxes. The semantic paradoxes are diagonalisation arguments involving sentences and properties, which work in a similar self-referential way to the set-theoretic paradoxes. I shall just outline the three main semantic paradoxes.

- (Grelling's paradox) Call a property *heterological* iff it does not satisfy itself. Is heterological itself heterological?
- (Berry's paradox) Let N be the least natural number not definable in English in less than one hundred words. Then N is definable in English in less than one hundred words (we have just done so in the previous sentence!).
- (Richard's paradox) Let E be the set of every real number whose decimal expansion is definable in English. Enumerate the elements of E by lexicographic ordering of their English definitions. By diagonalisation on the decimal expansions we can construct a real number not in E: we have thus defined an indefinable number.

These paradoxes need to be resolved by tightening up our language in some way; either that or we need to live with the fact that our language is powerful enough to generate nonsense. Whatever resolution we adopt for the semantic paradoxes is likely also to cover the set-theoretic paradoxes (construed in terms of properties). Hence the mere presence of the paradoxes, troublesome though they are, is not an objection to the class view of sets.

A set theory based on this approach would take account of Poincaré's (1906b, §IX; 1910; 1913, chapter IV) and Weyl's (1921, 1925–7) considerations of the 'vicious circle' principle and 'predicative' definitions. It would probably be similar to Whitehead & Russell's type theory (1910) or Quine's set theory (1937).

The *limitation of size* view rejects any such intensional origin of sets. A set is defined as a multiplicity or plurality of objects that is *quantitatively determinate* and hence may be thought of as a single thing. There is no doubt that this is in line with Cantor's own thinking: see especially his critique of Frege (Cantor, 1885). On this view, the difference between consistent and inconsistent multiplicities is that the latter are simply too big to have a cardinality. Neither Cantor nor any of his successors has offered any explanation of why cardinality should be the decisive factor. Indeed, there are alternative versions of set theory in which a set can have a non-set as a subclass (Vopěnka, 1979; Sochor, 1984).

Mayberry (2000) gives a lucid and vigorous modern presentation of set theory from the 'limitation of size' viewpoint. He addresses a *logical* question that twentieth-century set theorists were very slow to take up: given that absolute infinity 'can only be acknowledged but never known', how can it be legitimate to quantify over all sets? The difference between transfinite and absolutely infinite multiplicities is so fundamental that one would expect it to be reflected in a different semantics and logic for 'bounded' quantifiers ($\forall x \in S$, $\exists x \in S$, ranging over a set) than for 'unbounded' quantifiers ($\forall x$, $\exists x$, ranging over the whole universe of sets). Yet ZFC set theory allows unbounded quantifiers to be used freely, and combined with the propositional connectives in arbitrary ways, without any restrictions or precautions, as if the set universe were a domain like any other.

Mayberry gives the following reason for being suspicious of unbounded quantification (§§3.5, 7.2). If unbounded quantification were allowed without restriction then we could use it to define an identity criterion for classes of sets (or 'species', as Mayberry calls classes):

$$S = T \quad \text{iff} \quad \forall x \, (x \in S \Leftrightarrow x \in T).$$

If unbounded quantifiers were subject to classical logic then any classes S and T would determinately be either equal or unequal. Given this firm criterion of identity, it would be hard to resist the conclusion that classes were *objects*, and from them we could form pluralities of classes, and pluralities of pluralities of classes, and so on. This subverts the Cantorian definition of 'set': a set was defined as a multiplicity that is also a unity (an object), so we cannot admit any other kind of collective object. Hence a Cantorian *must* restrict unbounded quantification in some way. Mayberry discusses various possible theories involving first- or second-order quantification over sets or classes, and concludes that it is better to allow only Π_1 and Σ_1 propositions (i.e., $\forall x \cdots \forall y \; A$ and $\exists x \cdots \exists y \; A$, where the initial quantifiers range over all sets and A contains only bounded quantifiers), and to give them a quasi-intuitionistic semantics and logic.

Aside from this logical question, there is a persistent doubt (first raised by Russell (1906)) about the meaningfulness of the fundamental notion of 'limited in size'. What is it exactly that determines whether a multiplicity is too large to have a cardinality? How big is too big?

Imagine a mathematician who whole-heartedly accepts the thesis that mathematics is based on sets, that a set is a plurality of limited size, and that size is measured by bijections, but who nevertheless refuses to accept set theory in its conventional form, preferring instead to adopt one of the following positions.

Position 1: all sets are finite (in the usual sense of being smaller than $\aleph_0$); their cardinalities can be measured by natural numbers; absolute infinity coincides with infinity.

Position 2: only finite and countably infinite sets exist; the process of constructing ordinal numbers can never reach ω_1 (the first uncountable ordinal number); ω_1 represents absolute infinity.

Position 3: finite and countably infinity sets exist, and the possibility of uncountable sets is left open, but the power-set axiom is rejected; the power 'set' of any infinite set is absolutely infinite.

The problem here is that all three unorthodox positions are compatible with Cantor's thesis of limitation of size and they can all be well motivated.

As regards position 1, it is generally accepted that the existence of infinite sets is merely a hypothesis, albeit a very useful one for mathematics. Mayberry develops two alternative set theories, called 'Cantorian' and 'Euclidean', based on affirming and denying that a countable infinity is 'limited', respectively (2000). Cantor shrugs off the question with the comment that we cannot even prove that *finite* multiplicities are sets (1899b). His casualness on this point is disconcerting. Is it really the case that we cannot tell whether $\{a,b,c,d,e\}$ is absolutely infinite? If so, is that because the relevant facts are somehow hidden from us, or because the concept of limited size is, after all, meaningless?

Pollard suggests the following criterion: 'things are limited in number whenever they are less numerous than other things'; in other words, a plurality is limited iff it is *increasable* (1996). By Cantor's diagonal argument, the natural numbers are less numerous than the sets of natural numbers. Hence the plurality of natural numbers is limited. The fallacy of this argument is that it assumes the two pluralities involved *have* sizes, i.e., that they are sets. The diagonal argument shows that, *if* the plurality of natural numbers and the plurality of sets of natural numbers have sizes, then the former size is less than the latter size. This is no help in establishing that any infinite plurality has a size.

Position 2 is naturally suggested by reading Cantor's account of the generation of the ordinal numbers (1883, §§1,11). Cantor considered the ordinals as produced in a quasi-constructive way by applying two generating principles repeatedly, starting from 0:

(1) given an ordinal α we can form its successor $\alpha + 1$;

(2) given any succession of ordinals $\alpha, \beta, \dots$ already generated, with no greatest member, we can form the *limit* of them, the first ordinal greater than all of them.

This process, known as transfinite iteration, generates the ordinals

$$0, 1, 2, 3, \dots, \omega, \omega+1, \omega+2, \dots, \omega 2, \omega 2+1, \omega 2+2, \dots, \omega 3, \omega 3+1, \dots, \omega 4, \dots, \omega^2, \omega^2+1, \dots$$

where $\omega, \omega 2, \omega 3, \omega 4, \omega^2$ are generated by the second principle and the others shown (apart from 0) are generated by the first principle.

It seems clear that by applying the two generating principles we can form a long (but always countable) succession of countable ordinal numbers. But, as Lake asks, how are we ever to reach the uncountable ordinals (1979, §1.3)? It seems that we would need to generate uncountably many countable ordinals to justify the leap to ω_1; but the possibility of doing this is itself in question. How can we pull ourselves up by our countable bootstraps into the realm of the uncountable? Cantor tries to accomplish this step by defining the second number class, (II), as the aggregate of all countable ordinal numbers formable by the two generating principles; he says that we can continue from here to form further number classes of ordinal numbers (1883, §12), so he seems to believe that in forming (II) we have thereby formed ω_1.

(Cantor appeals to a third principle, a 'restricting or limiting principle', whose function seems to be to help us in delimiting the number classes.) But if it is acceptable to generate ω_1 as the limit of uncountably many countable ordinals, why is it not acceptable to generate Ω, the absolutely infinite order-type of all transfinite ordinals, as the limit of the succession of all transfinite ordinals? It would be extremely natural to identify ω_1 with Ω and to regard Cantor's generating process as inherently limited to the countable.

Position 3 is naturally suggested by Cantor's diagonal argument. It is conventional to say that the diagonal argument shows that the power set of any set S is strictly larger than S. But this formulation is question-begging, as I pointed out above. The conclusion of the diagonal argument is really that, given any set A of subsets of S, with $|A| = |S|$, we can always construct another subset of S outside A. It does not establish that there is a set of *all* subsets of S. If one couples the diagonal argument with Russell's, Poincaré's and Weyl's arguments about the vicious circle principle and impredicativity, one may easily be led to the conclusion that $\mathcal{P}(\mathbf{N})$ is an inexhaustible multiplicity of sets, so big that when we try to grasp it in full we always fall short. In short, $\mathcal{P}(\mathbf{N})$ is an excellent candidate for absolute infinity. (Compare this with the argument of Cohen (1966, p. 151).)

Power sets were used implicitly by Cantor (1891); the axiom asserting the existence of power sets was first stated by Zermelo (1908), without any justification. Lake asserts the axiom as obvious:

> The sum and power set axioms follow as it is inconceivable that an inconsistent multiplicity could be obtained from a set by one of these visualizable operations. This is even clearer if we assume that all inconsistent multiplicities are the same size, for then the power set axiom, for instance, says that there is no set for which the collection of all its subcollections is the same size as the Absolute. (Lake, 1979)

Mayberry (1994) regards the power-set axiom as 'obviously true', from a limitation of size viewpoint. In (2000) he is more tentative, describing it as 'the most powerful, and least self-evident, of the finiteness principles' (p. 117), but still 'highly plausible' (p. 123) (recall that Mayberry uses the word 'finite' to mean limited in size).

> The subsets of a plurality of determinate size are simply "there", in whatever multitude, definite or indefinite, they may compose. Surely it is implausible to suppose that multitude to be absolutely infinite. Surely we cannot conceive that the absolutely infinite could be rooted in, or could emerge from, a particular instance of the finite in such a manner. (p. 124)

Yet set theorists do believe that the absolutely infinite universe of sets emerges from nothing (or from two or more urelements in Mayberry's case), in the manner described by the cumulative hierarchy of sets (see below for a definition of the cumulative hierarchy). This is just as astonishing and counter-intuitive, when one first meets it, as the idea that a set could have an absolute infinity of subsets. We are all so *familiar* with ZFC set theory that we become insensible to its bizarre aspects and we tend to mistake familiarity for self-evidence. We have a vivid diagram in our minds of the cumulative hierarchy, in which $\mathcal{P}(S)$ lives one level above S, while absolute infinity is represented by an arrow pointing up towards heaven. This diagram is so entrenched in our thinking that it obstructs us from considering with an open mind the possibility that $\mathcal{P}(S)$ is absolutely infinite. Hallett (1984, chapter 5) examines in detail the attempts to justify the power-set axiom from a limitation of size viewpoint and concludes that they cannot succeed.

The problem revealed by each of these three unorthodox positions I have sketched is that our notion of limitation of size seems insufficient to justify the axioms supposedly based on it. The axioms that I called 'set-building axioms' in §2.3, together with the axiom of infinity, are, from the limitation of size viewpoint, intended to articulate the meaning of 'limited size'. Indeed, one would hope that the meaning of 'limited size' would be clarified in the course of arguing for the axioms (one usually understands a mathematical concept better when one has seen it used in arguments). And indeed it can be said that the separation and replacement axioms arise naturally from the fundamental principles of cardinality, (1′) and (2), with which we began in §2.2. However, the same cannot be said for the other axioms, particularly the power-set and infinity axioms (and possibly also the union axiom); the justifications given by set theorists for these axioms have a circular, or even vacuous, character; one could reject power sets or infinite sets and still claim to be true to the principle of limitation of size. I am suspicious of mathematical concepts that do no work; it is the axioms that do the work in set theory, not the principle of limitation of size, and there seems to be an unbridgeable gap between the principle and the power-set and infinity axioms. These two axioms are central to Cantor's project, for we need them to show the existence of infinite sets of different sizes.

Let us turn to the third approach to set theory, the *iterative* conception of sets. As we have seen, Cantor viewed the ordinal numbers as generated by a transfinite iteration, using his two principles of generation. A similar transfinite iteration process can be used to generate the entire universe of sets. We think of sets as generated in *stages*, indexed by ordinal numbers. The collection of sets formed at or before stage α is called V_α (where α is any ordinal number), and is defined by

(i) $V_0 = \emptyset$,

(ii) $V_{\alpha+1} = \mathcal{P}(V_\alpha)$,

(iii) if α is the limit of a succession of lesser ordinals then $V_\alpha = \bigcup_{\beta<\alpha} V_\beta$.

Clause (i) says that we start with no sets. Clause (ii) is analogous to Cantor's first principle of generation: at stage $\alpha + 1$ we form all sets consisting of sets available at stage α. Clause (iii) is analogous to Cantor's second principle of generation: if α is formed by taking the limit of a succession of pre-existing ordinals, then no new sets are formed at stage α; the only sets available are those that were already available at some previous stage.

It is a consequence of this definition that if $\beta < \alpha$ then $V_\beta \subset V_\alpha$; hence this procedure gives a *cumulative hierarchy* of sets. Each set is formed at a certain stage α; it will therefore belong to V_α and all V_γ for $\gamma > \alpha$. Indeed, a set is *defined* as something produced by this process at some stage. Notice that Cantor's 'inconsistent multiplicities' are never produced at any stage.

This iterative conception of sets emerged from the work of Mirimanoff (1917) and Zermelo (1930), and is followed by Gödel (1983), Boolos (1971), Wang (1974), Shoenfield (1977) and Maddy (1990). Popular though the iterative conception is today, many authors warn us in footnotes against taking it too literally. Maddy says 'Of course, the temporal and constructive imagery is only metaphorical; sets are understood as objective entities, existing in their own right' (p. 102). Likewise Shoenfield says 'We should interpret "before" here in a logical rather than a temporal sense' (p. 323). The point is that a set formed at stage α is logically dependent on sets formed at lesser stages.

Cantor himself rarely considered sets of sets, so there is no sign of the cumulative hierarchy, whether construed iteratively or not, in his writings. His position on temporal imagery was rather ambiguous: officially he was against it (Hallett, 1984, p. 28; Cantor, 1885), yet he relied extensively on ideas of finite and transfinite iteration.

> By a *finite* set we understand a set *M* which arises out of an original element through the successive addition of elements in such a way that also the original element can be achieved *backwards* out of *M* through *successive removal* of elements in reverse order.

This quotation is from 1887–8 and is translated in Hallett (1984, p. 147); Hallett goes on to point out that Cantor's proof that every infinite set has a countable subset relies on successively selecting an infinite sequence of elements from the set. Moreover, Cantor's belief that every set could be well-ordered seems to have depended on the idea that transfinite iterations could be carried out and completed (1899a). Gödel also seems to take transfinite iteration literally. He points out that the kinds of set used in mathematics are always sets of integers, sets of real numbers, set of functions from $\mathbf{R}$ to $\mathbf{R}$, or sets of some other limited type, rather than sets in general; he proposes to take the operation 'set of' as primitive, and to build up the universe of sets by transfinite iteration of this operation.

The iterative conception of sets leads to similar results to the limitation of size conception, and many set theorists endorse both conceptions. Nevertheless, the two are not equivalent. Mayberry, a supporter of the limitation of size view, regards the notion of transfinite iteration (or even finite iteration) as incoherent and utterly unsuitable as a basis for mathematics (2000); his book is useful for showing how the limitation of size view can be completely disentangled from the iterative conception.

It should be noticed, however, that the iterative conception does not provide *justifications* for the axioms; rather, the axioms are *built into* it. Shoenfield tries to justify the power-set axiom from the iterative conception (1977, p. 326), and likewise Maddy claims that the iterative conception provides 'the most satisfying account' of the power-set axiom (1997, p. 53; see also 1988). But in reality the iterative conception does not provide any account at all; it just takes the power-set operation for granted, when it assumes that we can collect together *all* sets consisting of objects generated so far. The axiom of infinity is also presupposed: it is assumed that given a countably infinite sequence of stages there is a limit stage beyond them. In Wang's and Shoenfield's versions, the axiom of replacement is also presupposed in their assumptions about stages; they seem to be relying on an idea of limitation of size here, so I am inclined to agree with Boolos that the axiom of replacement is not intrinsic to the iterative conception. Maddy (1997) relies more on 'extrinsic' justifications for the axioms than on any conception of the nature of sets.

Let us try a little harder to make sense of the power-set axiom. Bernays views sets in a 'quasi-combinatorial' way, 'in the sense of an analogy of the infinite to the finite': a set is determined by deciding, for each candidate member, whether it is to be included in the set or not (1935). The decision is made independently for each candidate. The set is determined by the outcome of infinitely many decisions, rather than by any finitely expressible membership criterion. Maddy takes up this idea:

> Finite combinatorics tell us that there is a unique subcollection of a finite collection for every way of saying yes or no to each individual element. Carrying this notion into

> the infinite, subcollections are 'combinatorially' determined, one for every possible way of selecting elements, regardless of whether there is a specifiable rule for these selections. (1990, p. 102)

Obviously, this idea presupposes that, in some suitably idealised or metaphorical sense, it is possible to take infinitely many decisions. This sort of infinitistic thought experiment fits naturally with the transfinite iteration process involved in the iterative conception. The quasi-combinatorial view gives us a conception of an 'arbitrary' subset of a given set A. Perhaps (this is a further leap of faith, but a fairly natural one in this context) it makes sense to go from an *arbitrary* subset of A to *all* subsets of A, and to gather them into a set, $\mathcal{P}(A)$.

In a similar vein, Wang says that a set is a multitude for which we can form an intuitive 'overview', or can 'look through' or 'run through' or 'collect together' the elements. This leads him to the power-set axiom:

> For example, not only are the infinitely many integers taken as given, but we also take as given the process of selecting integers from this unity of all integers, and therewith all possible ways of leaving integers out in the process. So we get a new intuitive idealization (viz. the set of all sets of integers) and then one goes on. (1974, p. 182)

One very useful application of the quasi-combinatorial idea is to justify the impredicativity in the separation and replacement axioms. If a set A is available at stage α then so are all its subsets, so $\mathcal{P}(A)$ is available at stage $\alpha + 1$. In particular, $\{x \in A \mid \phi(x)\}$ is available at stage α, for any set-theoretic formula $\phi(x)$. Many authors have been worried by the fact that $\phi(x)$ may contain reference to sets that have not yet been formed; it may even quantify over all sets. If we are forming sets in successive stages, how can we claim that $\{x \in A \mid \phi(x)\}$ is formed by stage α when it depends on sets that do not yet exist? For Hallett, this undermines the iterative conception fatally (1984, §6.1). The quasi-combinatorial idea comes to the rescue here. Whenever we go from one stage to the next we form all sets consisting of sets generated previously. We do so not via expressions such as $\{x \in A \mid \phi(x)\}$ but by choosing elements individually. If this procedure is legitimate at all, it generates *all* the subsets, once and for all. As we proceed to later and later stages we can write down more expressions of the form $\{x \in A \mid \phi(x)\}$, as more sets become available to us, hence we can name more and more subsets of A, but those subsets all existed before we could name them; they were all formed by stage α at the latest. Hence the quasi-combinatorial idea provides an *axiom of reducibility* for set theory: any expression $\{x \in A \mid \phi(x)\}$, no matter what sets it refers to, is extensionally equivalent to a subset of A generated by choosing elements of A individually.

No doubt many philosophers and mathematicians would be sceptical of such infinitistic procedures (e.g., Parsons, 1977), but in the privacy of their own minds I believe they are influenced by this sort of idea. The widespread acceptance of the impredicative separation and replacement axioms, and the consensus that there are more sets than can be defined by finite conditions, are surely attributable to Bernays' quasi-combinatorial picture. I am willing to take infinitistic thought experiments seriously and discuss them without embarrassment (see §4). The snag is that the rules of the game do not seem to be very clear. If it makes sense to survey all the possible ways of making a subset of A, and to make a set out of them, why does it not also make sense to survey the entire iterative process of generating the ordinals and to make set out of them all? Indeed, Cantor himself seemed to countenance such a completed traversal of the ordinals, while simultaneously denying that the ordinals formed a set (1899a); so perhaps this is not, after all, the basis for the distinction between sets and inconsistent multiplicities.

However this may be, it is a fundamental *assumption* of the iterative conception that it is possible to 'finish' the process of generating all the subsets of A, but not possible to 'finish' the process of generating all the stages. This suggests, even more strongly than in the limitation of size conception, that it would be appropriate to use intuitionistic logic for quantifiers that range over all sets (Pozsgay, 1971; Tharp, 1971). Perhaps the moral of the paradoxes is that the concept of set is *indefinitely extensible*, in Dummett's sense (1994); our attempts to grasp it always have an incomplete and provisional character. We can never say 'for all sets', but merely 'for all sets we have managed to encompass so far'.

Absolute infinity, then, must be viewed as a potential infinity, in rather the same way that constructivists view countable infinity. This does not sit easily with some of Cantor's arguments. For Cantor claimed that every potential infinity implies an actual infinity; that is, anyone who accepts a certain domain as a potential infinity is on a slippery slope that can only lead to acceptance of it as an actual infinity (Hallett, 1984, §1.2). If we apply this argument to absolute infinity then we are forced to accept the transfinite iteration as 'completable', thus blurring the distinction between transfinite and absolute infinity.

2.6 Conclusions on actual infinity

Traditional philosophy attached great importance to the distinction between the finite and the infinite: the finite was the proper domain of human reason; the infinite eluded human understanding and could only be hinted at feebly by way of potential infinity. Cantor accepted this basic framework but refined it by adding an intermediate category, the transfinite (the infinite being rechristened 'absolute infinity'). The transfinite is like the finite in being 'actual', a 'unity', and amenable to mathematical comparison and calculation, but it includes mathematical domains previously classed as infinite. Indeed, it could be said that Cantor's aim was to shift the boundary between finite and infinite upwards so that mathematically useful systems such as $\mathbf{N}$, $\mathbf{R}$ and $\mathcal{P}(\mathbf{R})$ would lie on the finite side (Hallett, 1984, §1.3). Bijections and similarities were the instruments by which Cantor measured, carved up, and regulated the transfinite.

There is no denying Cantor's success in generating an appealing and coherent mathematical theory, in a topic where his predecessors had seen only confusion and paradox. But is his notion of transfinite ultimately sustainable? All ways of understanding the transfinite rest on an analogy with the finite. I have examined two in detail: a static view, in which some pluralities are seen as limited in size and others are seen as too big to have a size; and a dynamic view, in which sets are generated iteratively (I am leaving aside here the class view of sets, which has no transfinite/absolute distinction). Both views are plagued by a suspicion of semantic indeterminacy. On the iterative conception, we encounter embarrassing gaps in the iteration process: we have to accomplish the step from finite stages to stage ω and from countable stages to stage ω_1 by specific hypotheses. On the limitation of size view, how do we show that $\mathbf{N}$ and $\mathcal{P}(\mathbf{N})$ are limited rather than absolutely infinite? The problem is not that the full facts are not available to us (we know exactly what $\mathbf{N}$ and $\mathcal{P}(\mathbf{N})$ are supposed to be; no information is hidden), or that the required proofs have yet to be discovered, but that our notion of transfinite iteration or limited size does not seem substantial enough to determine an answer. Our inability to evaluate cardinal exponentials, $2^{\aleph_\alpha}$ or $\aleph_\alpha^{\aleph_\beta}$, may simply be a symptom of an underdetermination of meaning.

The problem of distinguishing between transfinite and absolute infinity is really an instance of what I termed the 'horizon problem' in §1. To give a global account of sets we need to say something about absolute infinity; our account will, almost irresistibly, lead to our treating absolute infinity as an *actual* infinity, and hence will run the risk of conflating it with the transfinite. The horizon problem is especially acute for Cantorians as they must justify converting the potential infinity $0, 1, 2, 3, \ldots$ into ω, and accepting ω as an object, while resisting a similar treatment for absolute infinity.

There are other possible approaches to the question that I have not discussed. I have ignored the theological side of Cantor's thinking on infinity (Hallett, 1984, §§1.1, 1.3–4), as I find it tends to make the issue of absolute infinity (and whether it is an actual infinity) even more murky. I have also not mentioned the approach of von Neumann (1925), in which absolutely infinite multiplicities are admitted as genuine objects but are not allowed to occur as members of any multiplicity. That is, the set V of all sets exists, but we are banned from forming the set $\{V\}$. This avoids the paradoxes successfully, but it is profoundly non-Cantorian and I am unable to see any philosophical rationale for it. Nor have I discussed the recent attempts to found set theory on plural quantification and second-order logic (Pollard, 1990), as this seems merely to rephrase the problems in a different language.

Sceptics will say that the central notion of transfiniteness is ill-defined and that the attempt to understand infinity by analogy with the finite is a basic category error. Supporters of set theory will point to the large measure of agreement about the treatment of small transfinite sets such as $\mathbf{N}$, $\mathbf{R}$ and $\mathcal{P}(\mathbf{R})$ and will maintain that any haziness about absolute infinite does not compromise the ordinary practice of mathematics, where we deal only with particular 'given' transfinite sets on any particular occasion.

3. Potential Infinity

3.1 Introduction

The 'potential' view of infinity arises naturally from a constructivist philosophy of mathematics. By *constructivism* I mean the doctrine that constructions are the subject-matter of mathematics. For Brouwer, this means *mental* constructions, generated in the mind through the primal intuition of 'two-ity' (a process of successively forming ordered pairs of mental events); see van Stigt (1990). Heyting sees constructions in a more general way, as arising from 'the possibility of an indefinite repetition of the conception of entities' (1956, p. 13). Bishop sees natural numbers as the prototype of all constructions (1967, 1970). Hilbert was a constructivist in the 1920s (see his (1925) and (1927)); his constructions were configurations of concrete symbols:

> as a condition for the use of logical inferences and the performance of logical operations, something must already be given to our faculty of representation, certain extralogical concrete objects that are intuitively present as immediate experience prior to all thought. If logical inference is to be reliable, it must be possible to survey these objects completely in all their parts, and the fact that they occur, that they differ

> from one another, and that they follow each other, or are concatenated, is immediately given intuitively, together with these objects, as something that neither can be reduced to anything else nor requires reduction. (Hilbert, 1925)

For my purposes in this chapter, it does not matter whether the constructions are mental or physical; what matters is that they are built up out of discrete building blocks by applying certain construction operations repeatedly. The word 'construction' is not meant in a metaphorical sense, divorced from everyday usage; it includes things such as houses, which are constructions of bricks and mortar, but also ranges more widely over all mental or physical structured assemblages of elements.

When we speak of constructions we mean *tokens* rather than *types*: Hilbert considers physical inscriptions rather than their abstract shapes (1925); Brouwer considers mental tokens (events in particular people's minds at particular times) rather than mental types (abstract thoughts that can be instantiated in people's minds). Nevertheless, we treat construction tokens of the same type as interchangeable, so it will often *sound* as if we are talking about construction types: when we speak of 4 we are referring ambiguously to any token of type "4", and when we say '4 has a unique successor' we really mean 'all successor tokens of "4" tokens are of the same type'.

The simplest kind of construction is the natural number, constructed from 0 by applying the successor operation S repeatedly. Any repetitive process can be considered to provide a system of natural number tokens: 0 is the initial state and S is the application of one further iteration of the process. The infinity of natural numbers is understood in a modal way: given any natural number (token) n, it is *possible* to construct the successor $S(n)$. Of course, at any one time only a finite supply of natural number tokens has yet been constructed, but this does not matter: in constructive mathematics we are not concerned with the actual supply of constructions (nor even the set of all possible constructions), but with the construction *process* and the fact that it can always be continued one more step. Hence we have potential infinity without actual infinity.

Constructivists generally reject actual infinity altogether. There are three possible grounds for such a rejection.

(a) Ontological grounds, i.e., the claim that no actual infinities exist. This seems to have been Hilbert's position (1925, 1930, 1931).
(b) Epistemological grounds, i.e., the claim that we finite beings cannot know whether actual infinity exists.
(c) Semantic grounds, i.e., the claim that we finite beings cannot meaningfully refer to actual infinity. This is Dummett's view (1975b).

Notice that these grounds are mutually exclusive: if you believe that all talk of actual infinity is meaningless then you cannot even pose the question of whether actual infinity exists; if you believe that no actual infinities exist then you evidently believe the question is meaningful and answerable. Nevertheless, constructivists often try to avoid committing themselves on these fundamental philosophical matters (Heyting, 1956, chapter I; Bishop, 1967, p. 2; Troelstra & van Dalen, 1988, p. vii).

In this section I shall try to explicate the notion of potential infinity, primarily in the form of the indefinite repeatability of the successor operation on natural numbers, and to consider the objections posed by supporters of actual infinity and by feasibilists. I shall not attempt to review constructive mathematics (for which see Bishop (1967), Bridges & Richman (1987) and Beeson (1985)), but shall instead focus on the logical issues (especially the meanings of the logical constants) and the understanding of infinity.

3.2 Potential infinity versus actual infinity

It should be clear from the brief description in the previous subsection that the key idea underlying potential infinity is that of *indefinite iteration* of an operation such as successor. This idea has come under attack from supporters of actual infinity, who believe that it is ill-defined or that it presupposes actual infinity. If iteration cannot be made sense of without relying on actual infinity then the distinction between potential and actual infinity becomes blurred and the case for constructivism is seriously undermined. I shall consider these objections in this subsection.

Let us consider three possible ways of defining the concept of natural number.

(1) Define **N**, the set of natural numbers, using second-order logic or set theory. There are various ways of doing this, but the general idea is to define **N** as the intersection of all sets that contain 0 and are closed under S.
(2) Define **N** as the set of all things that can be produced by applying S repeatedly to 0.
(3) Define the counting algorithm (the algorithm that enumerates 0, $S(0)$, $S(S(0))$, $S(S(S(0)))$, $\ldots$, without end – see the flowchart in figure 1), and interpret all statements about the concept of natural number as being really about the counting algorithm.

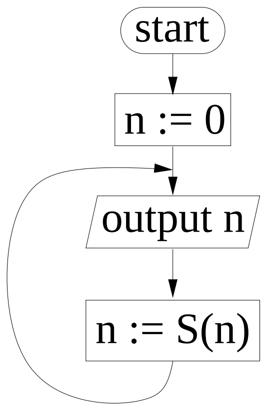

Figure 1. The counting algorithm.

Options (1) and (2) involve actual infinity, as they countenance the set of all natural numbers as a completed infinity; option (3) involves potential infinity, as it avoids reference to the totality of all natural numbers. Cantor took option (2) (see §2.5), as did Dedekind in his (1872), but by (1888b) Dedekind had developed his theory of 'chains' and switched to option (1). Frege also supported option (1): his theory of the ancestral relation (1879) is similar to Dedekind's theory of chains. Many modern platonists take option (1) in order to distance themselves as far as possible from iteration and potential infinity.

The choice between these options is the key point of contention between platonists and constructivists (it is much more important than the question of whether to accept the principle of excluded middle). The platonist view in favour of option (1) is supported by the following five arguments.

(i) The circularity argument. Dedekind (1890) claims that options (2) and (3) involve a vicious circle. In option (2) we are defining a natural number as anything that can be reached from 0 by *a finite number of iterations* of S; but it is clearly circular to define 'natural number' in terms of 'finite number'. Dedekind continues:

> The mere words "finally get there at some time", of course, will not do either; they would be of no more use than, say, the words "karam sipo tatura", which I invent at this instant without giving them any clearly defined meaning.

This argument is repeated by later platonists, such as Tait (1983) and Mayberry (2000). The argument clearly applies against option (3) as well. Understanding the flowchart in figure 1 involves realising that one is supposed to start at the 'start' box and follow the arrows *for ever*, which presupposes infinite time and an infinite number of steps, indexed by natural numbers; hence option (3) presupposes both actual infinity and the general concept of natural number. Mayberry denounces 'operationalism' (i.e., option (3)) at length; finally he exclaims 'But what does "for ever" mean? *That* is the very point at issue!' (p. 385)

This argument utterly fails to impress constructivists. For them, the flowchart is a finite object (four boxes linked by four arrows). The instructions are clear: start at the 'start' box and keep following the arrows. The instructions do not contain the phrases 'finally', 'finite number' or 'for ever'. The flowchart is unusual in having a 'start' box but no 'stop' box. This absence of a 'stop' box is the true meaning of infinity; unfortunately, platonists have inflated this humble notion and made it into something mysterious. Constructivists insist that this idea of *keeping going* is immediately intelligible, does not presuppose any grasp of eternity, and is all the notion of infinity we have or need (for arithmetic). There is something about this view that seems to fit the human condition. We know how to put one foot in front of the other, even though we have no inkling of our final goal. As Bob Dylan says, the only thing we know how to do is to *keep on keepin' on*. Brouwer has attempted to explain this in terms of temporal intuition (1907), but such explanations have been generally found to be opaque and unhelpful. Perhaps the best tactic for constructivists is simply to insist that we understand algorithms, as evidenced by our ability to execute them, compare them, and express them in various programming languages, and to take this understanding as primitive.

(ii) The reducibility argument. The circularity argument usually occurs coupled with a second argument, that because natural number *can* be defined in terms of second-order logic or set theory, it *should* be so defined. Mayberry makes this claim, on the grounds that a mathematical analysis of a concept is generally preferable to a philosophical explanation of it (2000, p. 271). Nevertheless, many twentieth-century authors have felt that to define natural number in terms of second-order logic or set theory is to misrepresent the real conceptual relations between them; as Poincaré puts it, 'This method is evidently contrary to all sane psychology' (1905). Poincaré also accused the early logicists and formalists of circularity, as their metamathematical methods presupposed the intuition of iteration and the principle of induction, which they were claiming to prove (1905, 1906a, 1906b). Similar reassertions of the fundamental character of iteration have been made frequently since then (Weyl, 1921; Skolem, 1922; Heyting, 1956, p. 13). There is no dispute about the fact that natural numbers can be *characterised* in terms of Dedekind's chains or in terms of sets, and that the principle of induction can then be proved. We can all admire the ingenuity of this trick; the point at issue is whether it is convincing as a conceptual analysis, whether this should be taken

as the *definition* of number, whether it represents a *reduction* of number to something more fundamental. Skolem says:

> Set-theoreticians are usually of the opinion that the notion of integer should be defined and that the principle of mathematical induction should be proved. But it is clear that we cannot define or prove ad infinitum; sooner or later we come to something that is not further definable or provable. Our only concern, then, should be that the initial foundations be something immediately clear, natural, and not open to question. This condition is satisfied by the notion of integer and by inductive inferences, but it is decidedly not satisfied by set-theoretic axioms of the type of Zermelo's or anything else of that kind; if we were to accept the reduction of the former notions to the latter, the set-theoretic notions would have to be simpler than mathematical induction, and reasoning with them less open to question, but this runs entirely counter to the actual state of affairs.

Thus the controversy reduces to the question of which starting point, iteration or set theory, is the more philosophically secure and mathematically workable.

(iii) The argument from extraneous empirical circumstances. Platonists believe that counting and other algorithmic processes are unsuitable as a basis for arithmetic since they are inextricably entangled with irrelevant empirical considerations, such as who is doing the counting, what materials they are counting with, and under what circumstances (Russell, 1903, §§109, 133). Of course, platonists accept algorithms as useful methods for discovering arithmetic facts, but not as providing a foundation for arithmetic facts. Frege, discussing the concept of 'y following in a sequence after x', rejects the idea of defining it iteratively, in terms of shifting attention step by step from x to y:

> Now this [the iterative definition] describes a way of discovering that y follows, it does not define what is meant by y's following. Whether, as our attention shifts, we reach y may depend on all sorts of subjective contributory factors, for example on the amount of time at our disposal or on the extent of our familiarity with the things concerned. Whether y follows in the ϕ-sequence after x has in general absolutely nothing to do with our attention and the circumstances under which we transfer it ... My definition [option (1)] lifts the matter onto a new plane; it is no longer a question of what is subjectively possible but of what is objectively definite. (1884, §80)

(iv) The feasibility argument. An obvious objection to options (2) and (3) is that we cannot, in fact, continue an iteration indefinitely. We shall sooner or later drop dead, or run out of paper, or encounter some other physical obstacle. For example, we cannot count up to 10^{100}; yet 10^{100} is certainly a natural number. Hence number cannot be based on counting. This argument originates with Cantor (Hallett, 1984, p. 27), and has been repeated with great frequency ever since (Bernays, 1935; Tait, 1986; George, 1988).

(v) The argument from error. Another obvious objection, though surprisingly less popular, is that any counting process is subject to error. An error is a discrepancy between the count and the true sequence of numbers. The fact that such discrepancies occur, or are even conceivable, demonstrates that number is founded on something other than counting.

I am not including any constructivist counter-arguments to the last three objections here, as they are better postponed to the subsection on feasibilism (§3.5). For the moment I shall just content myself with the bland response that constructivists base arithmetic on an 'idealised' counting process, free of empirical considerations, practical limitations and errors.

3.3 Mathematics as a process of construction

Let us suppose that we are persuaded by the constructivist side of the argument in §3.2. Like Baire (1905), we reject the idea of an infinite set, conceived as like a bag of marbles, and replace it with the idea of a construction process (i.e., option (3)). How then are we to understand statements involving infinite quantifiers? For example, consider the statement $\exists n\ OP(n)$, where the variable n ranges over the natural numbers and $OP(n)$ means that n is an odd perfect number (I shall use the word 'statement' for a formula in a mathematical language based on predicate calculus, or for its informal equivalent). There is an algorithm for testing whether a given natural number is odd and perfect, so the meaning of the predicate OP is clear. The problem is the quantifier $\exists n$, which ranges over an infinite set. A platonist would understand $\exists n\ OP(n)$ as like an infinite disjunction $OP(0) \vee OP(1) \vee OP(2) \vee \cdots$ and would say that it is true iff at least one of the disjuncts is true. This explanation is not open to us. Constructivists regard $\exists n\ OP(n)$ not as *asserting* that there is an odd perfect number, but as expressing the *intention* to find an odd perfect number or the *problem* of finding an odd perfect number (Heyting, 1931; Kolmogorov, 1932). If N is an odd perfect number then we may say 'N achieves the intention $\exists n\ OP(n)$' or 'N solves the problem $\exists n\ OP(n)$', or 'N is a proof of $\exists n\ OP(n)$'; these are three ways of expressing the same judgement, which I shall also write using the notation

$$N \vdash \exists n\ OP(n).$$

(This is a special sense of the word 'proof' and the symbol '$\vdash$', peculiar to constructivism and separate from the way they are used in platonistic mathematics.) The meaning of $\exists n\ OP(n)$ is given by specifying the conditions under which a given construction N counts as a 'proof' of it. It is easy to specify these conditions in this example: even though we do not know any odd perfect number, we certainly do know how to test whether a given number is odd and perfect.

Essentially the same idea is expressed by Weyl (1921) and Hilbert (1925). Weyl calls an existential statement a 'judgement abstract', and Hilbert calls it a 'partial proposition'. They regard $\exists n\ OP(n)$ as an incomplete judgement (one could write it as $OP(-)$), which needs to be supplemented by a construction, N, to make a complete judgement, $OP(N)$. We can say that a 'proof' of a statement is a construction that, when inserted into the statement, makes a true judgement.

Mathematics, then, is simply an activity of finding constructions that prove statements. A typical mathematical judgement is either of the form 'P is a proof of A' (i.e., $P \vdash A$) or 'given any construction x of a certain kind, $f(x)$ is a proof of $A(x)$'. Note that the latter judgement involves a kind of universal quantifier: one is tempted to write it as $\forall x\ f(x) \vdash A(x)$. In fact, even the simple judgement $P \vdash A$ involves an element of generality: it means that if any mathematician repeats the construction P, at any time, without error, then they must reach the same conclusion, that it proves A. Hence a constructive mathematical judgement is not merely an empirical report of a particular construction episode (as sometimes suggested by Brouwer (Placek, 1999) and Heyting (1956, pp. 3, 8)), but involves an aspect of generality, timelessness, objectivity and necessity.

How is this generality to be understood? Weyl and Hilbert say that we can understand a simple universal statement 'for any n, $A(n)$' (where n ranges over $\mathbf{N}$) as a kind of schema, that *becomes* a judgement $A(N)$ when a numeral N is substituted into it. We can interpret this in terms of Russell's distinction between 'for all' and 'for any' (1908, II). 'For all n' is the universal quantifier $\forall n$ of predicate calculus; $\forall n\ A(n)$ is understood by platonists as an infinite conjunction $A(0) \wedge A(1) \wedge A(2) \wedge \cdots$. 'For any n, $A(n)$' is a singular schematic judgement, meaning that $A(n)$ holds *regardless* of the value of n. It seems best to say, therefore, not '$\forall x\ f(x) \vdash A(x)$', but 'for any x, $f(x) \vdash A(x)$' (this is consistent with the characterisation of intuitionistic argument given by Herbrand (1931, footnote 3)).

To summarise, we hope to interpret constructivist mathematics in terms of two ideas:

- that a mathematical statement, A, is an incomplete judgement, something needing to be supplemented by a construction, P, to form a complete judgement, $P \vdash A$;
- that generality in mathematical judgements is to be understood in terms of 'for any' rather than 'for all'.

3.4 The meaning of the logical constants

So far we have only considered a very simple example of a mathematical statement, $\exists n\ OP(n)$. What happens when other logical constants are used, and when the logical constants occur in combination? Hilbert's approach was to interpret each mathematical theory as a whole: he regarded formal mathematical theories as devices for deriving true decidable statements (1925, 1927). Other constructivists, however, have sought to attach a meaning to individual statements, in line with the idea that the meaning of a statement is given by specifying what type of construction counts as a proof of it. Let us confine our attention to formulae in the language of Peano arithmetic, for the sake of simplicity. The atomic formulae are decidable: they can be evaluated by a computation to produce the result *true* or *false*. Hence, for an atomic formula, we may consider a trivial construction, such as 0, as a proof if the formula evaluates to *true*, and no construction as a proof if the formula evaluates to *false*.

The meaning of the logical constants is determined as follows.

(a) To prove $A \wedge B$ one must prove A and prove B.
(b) To prove $A \vee B$ one must select one of the two disjuncts and prove it.
(c) To prove $A \Rightarrow B$ one must show how, given a proof of A, to obtain a proof of B.
(d) To prove $\exists n\ A(n)$ one must select a natural number N and prove $A(N)$.
(e) To prove $\forall n\ A(n)$ one must show how, given a natural number N, to prove $A(N)$.

(I have not included a clause for $\neg A$ because it is convenient to define $\neg A$ as $A \Rightarrow$ *false*.) I venture to call this the *intended meaning* of the logical constants, since so many constructivists have given explanations of the logical constants approximately along these lines (Kolmogorov, 1932; Heyting, 1956, §§7.1.1, 7.2.1; Bishop, 1967, §1.3; van Dalen, 1973, §2.1; Dummett, 1977, §1.2; Beeson, 1985, §II.6; Bridges & Richman, 1987, §1.3; Troelstra & van Dalen, 1988, chapter 1, §3.1). Indeed, if this is not the intended meaning, it is hard to imagine what else could be intended. However, it is in the attempt to render this more precise that differences between constructivists emerge.

In the first place, the above account is phrased as a definition of a verb ('prove'), but in clause (c) it slips into use of the noun ('proof'). In the previous subsection we characterised a mathematical statement as an incomplete judgement, and a proof as the construction that has to be inserted in the gap to make a true judgement. In the light of this it is natural to take 'proof' rather than 'prove' as the definiendum. The above clauses need to be rewritten accordingly.

However, before doing so we need to decide what *type* of thing a proof is. In ordinary speech the word 'proof' denotes a sequence of judgements, proceeding from axioms or hypotheses to a conclusion. The formal counterpart of this is a tree of formulae, having axioms or hypotheses at the leaves and the conclusion at the root. I shall refer to these as *informal* and *formal derivations*, respectively. If a proof is a formal derivation then it is natural to rewrite clauses (a)–(e) as follows.

(a′) A proof of $A \wedge B$ consists of a proof of A, a proof of B, and a final inference $\frac{A \quad B}{A \wedge B}$.
(b′) A proof of $A \vee B$ consists of either a proof of A followed by a final inference $\frac{A}{A \vee B}$, or a proof of B followed by a final inference $\frac{B}{A \vee B}$.
(c′) A proof of $A \Rightarrow B$ consists of a proof of B from the hypothesis A.
(d′) A proof of $\exists n\, A(n)$ consists of a proof of an instance $A(N)$ followed by a final inference $\frac{A(N)}{\exists n\, A(n)}$.
(e′) A proof of $\forall n\, A(n)$ consists of a proof of $A(n)$ followed by a final inference $\frac{A(n)}{\forall n\, A(n)}$.

It appears from this that constructive proofs are formal derivations in a natural-deduction logic, using only introduction rules. But this is too restrictive: we must allow some use of elimination rules as well if formulae such as $(A \wedge B) \Rightarrow (B \wedge A)$ are to be provable. So perhaps it is better to take proofs as normal natural-deduction derivations, as suggested by Dummett (1977, p. 396).

However, there is surely something circular about defining a proof as a formal derivation when it is our intention to use the proof clauses (a)–(e) to *justify* the axioms and rules of inference used in formal derivations. Moreover, both formal and informal derivations seem very different from the special notion of proof introduced in §3.3, in which a proof is simply a construction that fills a hole in an incomplete judgement, and may be something as simple as a natural number. Dummett acknowledges this difference, referring to a proof in the sense of clauses (a)–(e) as a *canonical proof* and a mathematical derivation (whether formal or informal) as a *demonstration*; a demonstration provides the means for constructing a canonical proof (1977, pp. 391–4).

Accordingly, many authors would write the explanations of the logical constants in the following form.

(a″) $P \vdash A \wedge B$ iff $P = (Q, R)$, where $Q \vdash A$ and $R \vdash B$.
(b″) $P \vdash A \vee B$ iff $P = (i, Q)$, where $i = 0$ and $Q \vdash A$, or $i = 1$ and $Q \vdash B$.
(c″) $P \vdash A \Rightarrow B$ iff P is a function such that, for any Q, if $Q \vdash A$ then $P(Q) \vdash B$.
(d″) $P \vdash \exists n\, A(n)$ iff $P = (N, Q)$, where $Q \vdash A(N)$.
(e″) $P \vdash \forall n\, A(n)$ iff P is a function such that, for any natural number N, $P(N) \vdash A(N)$.

There are many theories of constructive proof along the lines of clauses (a″)–(e″). I shall not attempt to describe them, but shall rather review the common dilemmas they face.

Theories of constructive proof may be classified into *bottom-up* theories, which start with well-understood mathematical constructions such as natural numbers and try to use them to define a concept of proof based on clauses (a″)–(e″); and *top-down* theories, which take the full pre-theoretic concept of constructive proof and try to analyse it to make it more explicit and intelligible.

The simplest bottom-up theory is Kleene's concept of realisability (1945). The proofs are natural numbers, the functions in clauses (c″) and (e″) are partial recursive functions coded as natural numbers, and the pairs are also coded as natural numbers. Gödel's (1958) 'Dialectica' interpretation, Scott's (1970) theory, and Martin-Löf's (1984) intuitionistic type theory are also bottom-up theories, though involving higher-type constructions built upon the natural numbers.

A feature of all these systems is that the $\vdash$ relation is undecidable. This should be clear from clauses (c″) and (e″), since it is undecidable whether a function satisfies the condition to be a proof of $A \Rightarrow B$ or $\forall n\, A(n)$. As Beeson points out, this deviates considerably from the usual notion of constructive proof (1985, pp. 281–2). Consider for example Goldbach's conjecture, $\forall n\, G(n)$, where $G(n)$ is the decidable statement that $2n+4$ is the sum of two prime numbers. If Goldbach's conjecture is constructively valid then we have

$$f \vdash \forall n\, G(n),$$

where f is a function mapping any natural number n to 0. (Recall that a decidable statement such as $G(n)$ has 0 as a proof iff it is true.) But no one would accept f as a 'proof' of Goldbach's conjecture. The problem of determining whether $f \vdash \forall n\, G(n)$ is as hard as the problem of solving Goldbach's conjecture; f contributes nothing to the solution of the conjecture. The statement $\forall n\, G(n)$ is an incomplete judgement, but f fails to fill the gap; it leaves the gap as wide as ever. Hence it is often proposed that a proof ought to include enough information to make it clear that it *is* a proof; in other words, the $\vdash$ relation should be decidable (Kleene, 1945, §2; Kreisel, 1962, §4; Dummett, 1977, p. 12). (See Sundholm (1983) for a contrary view, however.)

There are some bottom-up theories in which $\vdash$ is decidable, such as the early version of Martin-Löf's type theory (1975): functions are given by terms in a special language, and by reducing the term to a normal form it can be seen whether it is a proof of a given formula. Indeed, the view that proofs are formal derivations (clauses (a′)–(e′) above) may also be regarded as a theory of this type. Decidability, however, is bought at the price of a limited language for defining functions. By diagonalising out of the class of expressible functions we can define a function, not expressible in the language, that transforms any proof of a formula A to a proof of a formula B. In such a case, we are able to derive proofs of B from proofs of A, yet we do not have a proof of $A \Rightarrow B$. This defeats the whole purpose of constructive implication: the idea of $A \Rightarrow B$ was to express the problem of converting any given proof of A to a proof of B (clause (c)).

Hence all bottom-up theories have difficulties conforming to the intended meaning of proof. Top-down theories proceed in the other direction, beginning with clauses (a)–(e) or (a″)–(e″) and trying to infer the nature and structure of proofs. These theories distinguish between 'concrete' mathematical constructions (involving natural numbers, recursive functions, and similar discrete things) and 'abstract' mathematical constructions (involving mentalistic notions of proof, meaning and rules). (Beeson (1985, §§III.8, VI.10) uses the word 'rule' to mean a procedure whose application requires understanding of meaning, but not creativity or free will.) It is not assumed that abstract constructions can be reduced to concrete ones; in particular, it is not assumed that all rules mapping one natural number to another are recursive functions. It is assumed that $\vdash$ is decidable, in the sense that a human can decide whether

$P \vdash A$ by attending to the meaning of P and A; but it is not assumed that $\vdash$ is mechanically decidable, i.e., recursive.

In order to make $\vdash$ decidable, it is necessary to add 'supplementary data' or 'second clauses' into the clauses (c$''$) and (e$''$):

(c$'''$) $P \vdash A \Rightarrow B$ iff $P = (E, F)$, where F is a function and E is evidence that, for any Q, if $Q \vdash A$ then $F(Q) \vdash B$.

(e$'''$) $P \vdash \forall n\ A(n)$ iff $P = (E, F)$, where F is a function and E is evidence that, for any natural number N, $F(N) \vdash A(N)$.

Thus every proof carries its own evidence that it really is a proof. But what is evidence, exactly? Beeson (1985, p. 39) and Díez (2000) believe that evidence is simply proof. Thus the definition of $\vdash$ is circular, rather like Tarski's platonistic definition of truth of a formula in a model. Beeson finds this circularity untroubling, since constructive proof is a fundamental concept and needs no explaining. Nevertheless, this leaves us with a rather obscure notion of proof, with the proof relation characterised in terms of an ill-delimited totality of all proofs (clause (c$'''$)). As Beeson admits (p. 402), this disrupts the attempt to define $P \vdash A$ by structural induction on the formula A.

Most authors prefer to construe evidence as something simpler and more fundamental than proof. Note that the 'for any' quantifier used in clauses (c$'''$) and (e$'''$) may be understood in the sense explained in §3.3, rather than as $\forall$. All we need to assume is that it is decidable whether any given E is evidence for a judgement of the form 'for any x, $C(x)$', where $C(x)$ is a decidable statement or a conditional involving decidable statements; then $\vdash$ becomes decidable. The definition of $\vdash$ becomes non-circular, since we are defining proof of a formula in terms of proof of its syntactic components, using a predefined concept of evidence. All we need to complete the story is a philosophical account of evidence.

Theories of this sort are provided by Kreisel (1962, 1965) and Goodman (1970, 1973). Unfortunately, neither tells us anything about what evidence is. Worse, it turns out that the assumption that evidence is decidable and sound leads to a self-referential paradox (Goodman, 1970, §9). Goodman attributes this to a vagueness in the description of how constructions are built up (§10), and responds by stratifying the universe of constructions into levels. The bottom level consists of constructive rules, the next level consists of rules and proofs about rules, and so on. Goodman restricts the scope of each item of evidence to apply to constructions of a particular level. He then finds that he needs to posit a reducibility operator (§11), acting rather like the reducibility axiom in Russell's ramified type theory (1908), and suffering of course from the same implausibility.

This unexpected need for stratification and reducibility is related to a perceived impredicativity in the definition of constructive implication. If one believes that proofs are composed of formulae (as they would be if they were natural-deduction derivations), then to understand $A \Rightarrow B$ one is required to survey all proofs of A, some of which may contain $A \Rightarrow B$. This is clearly an impredicativity. To avoid this, Dummett proposes that, for any statement A, there should be an upper bound on the complexity of the proofs of A that need be considered, a bound that increases with the syntactic complexity of A (1977, §7.2, especially pp. 394–6). The impredicativity does not arise if proofs are not composed of formulae, but the complexity bound is still desirable as it makes the task of surveying all proofs of A more tractable. Goodman also makes the proposal of a complexity bound; and he points out that if one applies it to the case where A is itself an implication $C \Rightarrow D$, then it implies that all functions mapping proofs of C to proofs of D may be assumed to be of bounded complexity (1970, §11). This itself is a sort of axiom of reducibility.

The foregoing argument suggests that stratification and reducibility are necessary to the coherence of constructive implication; unfortunately it does nothing to make them plausible.

It should be clear now that constructivism is in serious difficulties in its attempt to make sense of its fundamental concept of proof. Bottom-up theories, appropriate to constructivists such as Markov (1968) and Bishop (1970), fail to do justice to the intended meaning of proof (i.e., clauses (a)–(e)). Top-down theories, which are more appropriate to intuitionism, rely on our supposed ability to grasp and quantify over an ill-defined universe of proofs. They appeal to *ad hoc* assumptions of stratification and reducibility to make constructive implication manageable. They make heavy use of mentalistic notions such as 'understanding of meaning' and 'humanly computable rule', which seem to entangle them with intractable problems in the philosophy of mind.

Díez (2000) offers a hybrid solution that uses a concept of realisability similar to Kleene's, but also uses 'second clauses' at the outermost level:

$P \vdash A$ iff $P = (E,N)$, where $E \vdash$ 'N realises A'.

This gives rise to an odd notion of implication: there will be cases in which we know how to transform a proof (E,N) of A to a proof (E',N') of B, yet N' may depend on both E and N, hence there is no function transforming N to N'. In this case, $A \Rightarrow B$ is not realised and hence is not proved. Thus we have a violation of clause (c): we know how to convert a proof of A into a proof of B, yet we have not proved $A \Rightarrow B$. As with the decidable bottom-up theories, this undermines the very purpose of implication. Díez is well aware of this phenomenon but does not seem to see it as a problem. In addition, Díez's proposal suffers from the same risk of circularity as the top-down theories, but in an especially clear form, since 'N realises A' may be of arbitrary logical complexity.

My own theory of proof can be found in Fletcher (1998); I shall not attempt to describe it here, except to say that its aim is to combine the explicitness of the bottom-up approach with full adherence to the intended meaning (clauses (a)–(e)), using 'second clauses' and a theory of evidence.

3.5 Feasibility

It is time to return to a problem left over from §3.2. A constructivist believes that the subject matter of arithmetic is iterative processes in general, of which counting is a typical example. In §3.2 I stated an obvious objection to this view: that all iterative processes are subject to practical limitations; it is not feasible to count up to 10^{100}. This objection is used by platonists to expose the incoherence of constructivism, but it can also be made by those who genuinely believe that arithmetic should be limited to feasible numbers, numbers one can actually count up to. Such people are known as strict finitists, ultra-finitists, ultra-intuitionists or feasibilists (Yessenin-Volpin, 1970; Wright, 1982).

The feasibilists accuse the constructivists of making an unwarranted and ill-defined idealisation from small finite examples to arbitrary finite examples. This accusation is rather reminiscent of the charge brought by the constructivists against the platonists, that of making an unwarranted and ill-defined extrapolation from the finite case to actual infinity. Some authors have claimed that there is a very close analogy between the feasible-to-finite idealisation and the finite-to-infinite extrapolation (Wright, 1982; George, 1988). If so then constructivism is in trouble.

Strangely, constructivists have given little attention to this serious objection. Troelstra & van Dalen admit that it is a genuine problem (1988, p. 851). Dummett has argued that feasibilism is incoherent, because the predicate 'feasible' is vague and hence prone to the ancient Greek paradox of the heap (1975a). Constructivism, of course, is not similarly dependent on any vague predicate. This certainly disrupts the analogy that Wright and George are attempting to draw, but it does nothing to justify the constructivist's idealisation.

Let me try to provide the missing justification. Imagine that we are setting out to develop a theory of some counting process, such as the writing of a sequence of strokes $|||||||| \cdots$; we intend to take account of all the practical factors that might affect this process, such as running out of paper or being struck by a meteorite. We first note that our theory will contain both necessary and contingent propositions. Examples of necessary propositions are:

> 'To write ten strokes entails writing five strokes, twice.'
> 'To write two strokes, followed by two more, is to write four strokes.'

Examples of contingent propositions are:

> 'It is practically feasible to write 500 strokes.'
> 'The likelihood of a meteorite strike while one is writing 500 strokes is very low.'
> 'If one attempts to copy a sequence of 500 strokes one is very likely to write too many or too few by mistake.'

Note that the contingent propositions involve vague predicates and miscellaneous empirical considerations, whereas the necessary ones do not. Note further that a necessary proposition is never logically dependent on contingent propositions. Hence the necessary propositions form a self-contained logical system: their logical inter-relationships can be studied without reference to the contingent propositions. The necessary propositions all have a conditional character: they do not contain any outright assertion that any sequence of strokes can be written (after all, even writing a single stroke might be practically very difficult in some circumstances).

Our theory of stroke-writing, therefore, falls apart neatly into two parts: a self-contained necessary theory, and a contingent theory that surrounds it and links it to our general knowledge about the physical world. The necessary theory is, of course, arithmetic. An important advantage of this decomposition is that the necessary theory can be transferred to other iterative situations. For example, if we wanted a theory of, say, building towers of cardboard boxes we could use the same necessary theory and couple it to a new contingent theory. In short, arithmetic is a theory of the necessary aspects of all iterative processes. The application of arithmetic to a particular iterative situation is a two-step process: first develop the arithmetic facts, and then take account of contingent factors. From this point of view the proposal to insert feasibility considerations into arithmetic is completely misconceived.

This account answers two objections left over from §3.2: the argument from extraneous empirical circumstances and the feasibility argument. It can also be extended to answer the remaining objection, the argument from error. The fact that we are able to repeat a calculation, compare the two versions, identify discrepancies, and determine which version is erroneous, gives an operational meaning to the concept of error. It gives us a grip on the question of objectivity, the distinction between a calculation's being correct and its merely seeming to be correct.

The feasibility issue is so important that I think it worthwhile to make the same point again by an analogy from a different field. In theoretical linguistics, a distinction is drawn between competence and performance (Chomsky, 1972, chapter 5). The *competence* of a speaker of a language is their knowledge of its vocabulary and grammatical constructions. The *performance* of a speaker is their actual linguistic behaviour, the sentences they are able to produce or understand under various circumstances, and the errors they make. A highly convoluted sentence, containing many nested subordinate clauses, might be within a speaker's linguistic competence, in the sense that they know all the words and grammatical constructions used in it, but beyond their linguistic performance, in the sense that it is too complicated for them to take in. Modelling a speaker's language abilities is a two-step process. First we model their competence, perhaps by means of a formal grammar; the grammar will usually include recursive production rules, which can be applied any number of times in a single sentence, and hence the grammar generates an infinite set of sentences. The second step is to model the speaker's performance, which is the result of an interaction between their infinite competence and finite resource constraints of short-term memory and attention.

It may seem paradoxical that, in order to do justice to a speaker's real linguistic behaviour, we have to begin by constructing an idealised competence theory in which we pretend that the speaker can handle sentences of arbitrary complexity. Indeed, many theorists have rejected this two-step analysis and have attempted to model performance directly. The best discussion of the controversy I have seen is by Fodor & Pylyshyn (1988, especially §3.1). They point out the explanatory value of the two-step analysis: 'a speaker/hearer's performance can often be improved by relaxing time constraints, increasing motivation, or supplying pencil and paper. It seems very natural to treat such manipulations as affecting the transient state of the speaker's memory and attention rather than what he knows about – or how he represents – his language.'

These considerations generalise to other types of cognitive function, particularly arithmetic. A person's performance in an arithmetic test can be accounted for as an interaction between their arithmetic competence (the algorithms they know) and the incidental circumstances (calculating aids, distractions, frame of mind, etc.). Someone who has been taught to calculate square roots in their head can calculate *more* square roots when given pen and paper – even though they have not been taught any more mathematics. Hence it is natural to infer that their knowledge of extracting square roots exceeds what they are actually able to demonstrate with particular materials.

Nevertheless, in arithmetic as in linguistics, there are those who maintain that we should develop a theory of performance directly, without going via competence; this view is feasibilism.

Let us attempt to develop a feasible theory of numbers, based on the principle that a number is feasible iff we can write that many strokes. (The word 'we' may be construed as referring to a particular person or the whole mathematical community at a certain time.) The first difficulty is that the obstacles that limit our stroke-writing are so various and variable. If our pencil point breaks then we are suddenly prevented from proceeding; there is a number n such that n is feasible but $S(n)$ is infeasible. Other obstacles, such as fatigue, boredom, or a shrinking piece of chalk, operate in a more gradual way. Sazonov (1995, §1) considers a theory of the former type, in which there is a largest feasible number; he claims that 'recursion theory relativized to such a finite row of natural numbers is essentially the theory of polynomial-time computability'. Most theories of feasibility, however, are of the latter type, satisfying the axiom

$$F(n) \Rightarrow F(S(n)),$$

where $F(x)$ means that x is feasible (Yessenin-Volpin, 1970; Parikh, 1971). In this case the feasible numbers should satisfy Peano's axioms. One may view F as a vague predicate if one wishes. A popular way of obtaining a theory of feasibility is by adapting some system of nonstandard arithmetic. We can set up a theory of nonstandard arithmetic very simply by introducing into first-order Peano arithmetic a constant 'c', with a list of axioms '$N < c$' for every numeral N. Such a theory is consistent, since every finite subset of the axioms is consistent, so it has a model, containing an infinite natural number c as well as the standard numbers $0, 1, 2, \ldots$. Now, if we alter the terminology, substituting the word 'feasible' for 'standard', then we can re-construe this as a theory of all the natural numbers, containing the feasible numbers as a subsystem. The full natural numbers satisfy the principle of induction

$$(A(0) \wedge \forall n\, (A(n) \Rightarrow A(S(n)))) \Rightarrow \forall n\; A(n)$$

for any formula $A(x)$ not containing the feasibility predicate F. If we extend the original theory to include sets, the nonstandard theory will have *standard*, *internal* and *external* sets (in the usual sense of nonstandard set theory). When we transfer this to a theory of feasibility, the internal sets are simply called 'sets' and the external sets are called 'classes'. For example, there is a (proper) class of all feasible numbers, which is a subclass of the set of all numbers less than c.

The most comprehensive attempt at providing a foundation for mathematics along these lines is *alternative set theory* (AST), developed by Vopěnka (1979) and Sochor (1984). For readers unfamiliar with AST, a helpful ultrapower model of AST in conventional set theory is provided by Pudlák & Sochor (1984). It turns out that in AST the feasibility predicate can be defined in terms of classes: a natural number n is feasible iff all subclasses of $\{0, 1, \ldots n\}$ are sets. AST provides a useful new view of the relation between the continuum and discrete structures, in terms of infinitesimals and indiscernibility relations (in fact, this can be done in any theory containing two models of arithmetic, one nested inside the other).

Feasibility theories of this type may be considered unsatisfying, as they say nothing about the size of infeasible numbers such as c; indeed, they allow us to prove that numbers such as 10^{1000} are feasible (by applying $F(n) \Rightarrow F(S(n))$ repeatedly 10^{1000} times). A bolder type of theory inserts a specific axiom $\neg F(\theta)$, where θ is a particular closed primitive-recursive term such as '10^{1000}'. This of course renders the theory inconsistent. The usual way round this, introduced by Parikh (1971), is to impose feasibility restrictions in the meta-theory, i.e., to limit the derivations to those with a feasible number of steps. It is also necessary to limit the syntactic complexity of the formulae in the derivations. For example, in AST the induction axiom is limited to formulae $A(x)$ of feasible length; otherwise one could apply induction with the formula $x = 0 \vee x = 1 \vee x = 2 \vee \cdots$ (a disjunction over all feasible numbers) to show that all numbers were feasible.

A theory of this sort will assume that the feasible numbers are closed under some arithmetic operations but not others: for example, it may contain as axioms $F(m) \wedge F(n) \Rightarrow F(m+n)$

and $F(m) \wedge F(n) \Rightarrow F(m \cdot n)$, but not $F(m) \wedge F(n) \Rightarrow F(m^n)$. Parikh shows (§2) that if a suitable upper bound is imposed on the complexity of derivations then an axiom $\neg F(\theta)$ can be added without enabling any new contradictions to be derived. Hence the theory is consistent from a feasibilist point of view.

The snag is that the value of θ has to be very large in comparison with the bound on the complexity of derivations. For example, a fairly short derivation will give:

$$F(10), \text{ hence } F(10^2), \text{ hence } F(10^4), \text{ hence } F(10^8), \ \ldots \text{ hence } F(10^{1024}).$$

Thus the concept of feasibility embodied in the theory far outruns the concept of feasibility being applied in the meta-theory. This presumably makes the theory implausible to a feasibilist.

It is possible to bring the feasibility restrictions in the theory and meta-theory into harmony by restricting the theory rather drastically in other ways (Sazonov, 1995, §§3–4). In fact, one has to reject the law of modus ponens and accept only normal natural-deduction derivations; and one has to reject the doubling function as infeasible.

The underlying trouble here can be stated quite simply without meta-mathematics. One would like to include the following principles in a theory of feasible arithmetic.

(i) If n is a feasible number and f is a feasible function then it is feasible to apply f n times.
(ii) The doubling function is feasible.
(iii) 1000 is feasible.
(iv) 2^{1000} is infeasible.

But of course these are inconsistent. One could make a case for rejecting any of them; but giving up (i) makes the feasible number system less self-contained and the concept of feasibility less coherent; giving up (ii) makes feasible arithmetic seem very impoverished; giving up (iii) or (iv) is contrary to our informal ideas of what is feasible.

In the end we may have to accept that feasibility is a context-dependent notion (it is feasible to write 1000 strokes, but not if each stroke is required to be twice the length of the previous one). Hence the idea of axiomatising feasibility with a single unary F predicate may be too simple-minded and may only capture some aspects of real-life feasibility constraints. Perhaps it would be fair to say that feasible arithmetic does not remove idealisation from arithmetic (as the feasibilists intended) but simply shifts the boundary of the idealisation so that the abstract notion of a bound to counting is incorporated in arithmetic.

It is significant that these formal theories of feasible arithmetic have difficulty quantifying over the feasible domain. Either the quantifiers range over all natural numbers (feasible and infeasible), or they are bounded by arithmetic terms (see §2 and §4 of Parikh (1971), respectively). This is reminiscent of the dilemma in Cantorian set theory, where one either finds oneself referring to proper classes (which officially one does not believe in) or restricting the range of each quantifier to a set (with consequent difficulties in making general statements about sets). This is simply a re-emergence of what in §1 I termed the *horizon problem*. A feasibilist needs a theory of infeasible quantities, just as a finitist needs a theory of the infinite and a Cantorian set theorist needs a theory of proper classes. The rejection of idealisation beyond actual human capabilities does not do away with the problem of infinity; it merely shifts the frontier.

3.6 Conclusions on potential infinity

The view of infinity developed in §3.2 and the concept of proof developed in §§3.3–4 require a reform of the whole structure of mathematics. Kronecker was the first to see that this required rejection of the logical principle of excluded middle, $A \vee \neg A$, in cases where A is undecidable. It also requires a modified concept of real number, based on Cauchy sequences given either by constructive rules (Bishop, 1967) or by free choice sequences (Troelstra, 1977); the former option leads to results consistent with 'classical' platonistic analysis, but the latter option leads to powerful new theorems having no platonistic counterpart. The more general consequences for mathematics, and set theory in particular, are just beginning to be elaborated; Beeson (1985) provides a good summary. Brouwer deserves the credit for almost singlehandedly establishing the philosophical basis of constructive mathematics and carrying out the required reconstruction of arithmetic and analysis (van Stigt, 1990).

Since Brouwer, constructivism has split into many schools (Bridges & Richman, 1987). A large part of constructive mathematics, however, is common to all schools and is developed by Bishop (1967). Moreover, proof-theoretic investigations have shown that almost all scientifically applicable mathematics (e.g., the theory of separable Hilbert spaces) can be formalised in theories that are proof-theoretically reducible to constructive number theory (Feferman, 1998). It might be argued, therefore, that the reasons that led to the acceptance of actual infinity at the end of the nineteenth century (see §2.1) have now been superseded. Potential infinity works just as well.

The feasibility objection to potential infinity, which seems at first sight so damaging, turns out to be untroubling. Feasible arithmetic can coexist peacefully with constructive arithmetic: the two are simply based on different idealisations.

On the other hand, we have seen in §3.4 the serious obscurities surrounding the fundamental concept of constructive proof. The meaning of any formula of the form $A \Rightarrow B$ depends on a quantification over a murky and ill-delimited totality of proofs of A. By comparison, the universe of transfinite sets seems clear and definite, and our worries about it in §2 seem to pale into insignificance. The motivation for constructivism was to avoid reliance on ungraspable infinite totalities, but we seem to have ended up worse off than we were under Cantor's regime. The difficulty of reasoning constructively about the totality of constructive proofs of a formula is the constructivist's equivalent of the horizon problem.

The more perceptive critics of constructivism have pointed out this objection (Bernays, 1935, p. 266; Gödel, 1933, 1938, 1941). Constructivists themselves have emphasised the importance and difficulty of the problem (Dummett, 1977, §7.2; Weinstein, 1983).

Clearly, the clarification of these issues is the most pressing problem facing constructivism today.

4. Physical Infinity

4.1 Introduction

The topic for this section is the nature of infinity as it occurs in applied mathematics and physics, in both countable form (an infinite number of particles) and uncountable form (the continua of space and time and other real-valued physical quantities such as mass and charge).

We are also concerned with the relation between mathematical infinity and physical infinity. There are two obvious ways in which they could be related. One could start with mathematical infinity and use it as a model for describing physical reality. Thus one would provide a foundation for the system of real numbers on pure mathematical grounds and then hypothesise that the structure of space and time is isomorphic to a four-dimensional manifold. The relation between the pure mathematical theory and the physical application is often explained in terms of 'bridge principles' (Peressini, 1999).

Alternatively, one could start with physical infinity and use it as a foundation for mathematical infinity. An example of this is Hellman's modal structuralism. Hellman (1989) interprets propositions of number theory as modal statements about all ω-sequences (an ω-sequence is a model of second-order Peano arithmetic, and may consist of elements of any kind, including physical objects). A proposition of number theory is defined to be true iff it is necessarily the case that it holds in all ω-sequences. Using this interpretation, Hellman (1998) has argued that if one accepts even the possibility of a physical ω-sequence then one is committed to the coherence of actual infinity in pure mathematics.

It is more common, however, to argue by analogy or thought experiment from the meaningfulness and possibility of physical infinity to the meaningfulness and possibility of mathematical infinity. To platonists it seems obvious that there could be an infinity of stars: 'It is fatuous to suppose that we know *a priori* that the stars in the heavens cannot possibly go on and on forever but that at some point in space they must come to an end' (Benardete, 1964, p. 31). Thus the idea of actual infinity is surely coherent. Hence the idea of an actual infinity of mathematical objects also makes sense. This seems to undermine fatally the *semantic* objection to actual infinity (see §3.1). Furthermore, this line of thought can be used to defend the principle of excluded middle in pure mathematics. Wittgenstein and the intuitionists have questioned whether it is justified to say that the sequence 7777 must either occur or not occur in the decimal expansion of π. Benardete (1964, pp. 129–130) imagines an infinite row of stars, colour-coded according to the digits of π (white represents 0, black represents 1, etc., with scarlet representing 7). He asserts *a priori* that either a sequence of four consecutive scarlet stars exists or it doesn't; the conclusion then transfers to π.

A second common platonist argument is that their truth-conditional view of infinite quantifiers (see §2.1) makes sense because one could, in principle, evaluate a quantified statement by examining each instance separately. Imagine, for example, testing Goldbach's conjecture (the statement that every even number greater than 2 is the sum of two prime numbers) by testing the even numbers $4, 6, 8, \ldots$ in succession. If one proceeded at ever-doubling speed one could complete all the tests in a finite time and so give a definite truth-value to the conjecture. This sort of infinitistic thought experiment is nowadays known as a *supertask*. Weyl questioned whether such a supertask was possible and related it to Zeno's paradox of Achilles and the tortoise (1949, p. 42).

In a similar vein, Russell believed that our inability to run through all the digits of π is not '*logically* impossible' but merely '*medically* impossible' (1935–6). This distinction indicates that we should disregard contingent limitations in this context as we are only concerned with conceptual coherence. It would not be appropriate to rule out the supertask by citing the limited human lifespan, the impossibility of travelling faster than light, or the atomic structure of matter, as these contingent factors do not undermine the conceptual coherence of the example and hence do not affect its validity as a thought experiment to elucidate the meaning of infinity.

Let us therefore make the following idealising assumptions:

(i) an actual infinity of bodies is possible, occupying arbitrary positions (provided they do not overlap);

(ii) any physical quantity (e.g., duration, velocity, mass, density) can take on arbitrarily large finite values or values arbitrarily close to zero;

(iii) bodies are perfectly rigid and have well-defined boundaries (this assumption can be relaxed in many cases).

This idealised world-picture can be used to probe the structure of the spatial continuum. The two key characteristics of the continuum are: *divisibility* (any line segment can be divided into two line segments), and *homogeneity* (all line segments are qualitatively alike, differing only in length). By enquiring about the ultimate parts of a line segment we can identify a number of options for the structure of the continuum. The first question to ask is, what happens if we try to divide a line segment into smaller and smaller pieces by repeated halving?

Option 1: a line segment cannot be halved more than a certain finite number of times. In this case the continuum has a discrete structure.

Option 2: a line segment can be halved any finite number of times. In this case the next question is, can we think of this endless subdivision process as completable? To state it more precisely, can we survey the whole collection of parts produced by this subdivision?

Option 2.1: the process of repeated subdivision is completable. Then we can take the intersection of a nested infinite sequence of parts, $P_1 \supset P_2 \supset P_3 \supset \cdots$, produced by subdivision, thus forming a smaller part, $\bigcap_{n=1}^{\infty} P_n$, which I shall call an ω-*part*. Since the continuum is homogeneous, the ω-parts are all alike and the original line segment is the union of all its ω-parts. The next question is, is an ω-part itself divisible?

Option 2.1.1: the ω-parts are indivisible and hence are the smallest parts of the continuum. The next question is, what is their length?

Option 2.1.1.1: the ω-parts are of zero length. This is the view taken by conventional modern analysis: the continuum is considered as a set of points, and an ω-part is a singleton set containing one point. It is conventional to add a completeness axiom, stating that any criterion for dividing a line segment into two connected parts determines a point where the division occurs.

Option 2.1.1.2: the ω-parts are of positive length, though still indivisible.

Option 2.1.2: the ω-parts are divisible, so we can continue the process of subdivision. On this view the continuum is represented by a non-archimedean field; an ω-part consists of parts separated by infinitesimal distances. This view was taken by Peirce; he regarded the continuum as an inexhaustible source of parts, exceeding in cardinality any transfinite set (Zink, 2001).

Option 2.2: the process of repeated subdivision is not completable. However many times one has divided one can always divide one step further, but it is illegitimate to consider this unending process as a finished whole. This was Aristotle's view (Lear, 1979–80). It is also the view that underlies modern 'pointless' topology, in which a space is considered as a lattice of open neighbourhoods rather than as a set of points (Tarski, 1927; Johnstone, 1983; Clarke, 1981, 1985), and also Dummett's (2000) intuitionistic view of physical continua. From this point of view, one admits that a line segment contains points, but only in the 'potential' sense in which a block of marble may be said to contain a statue; a line segment certainly does not *consist of* all its points, any more than a block of marble consists of all its statues.

Zeno's paradoxes were designed to refute each of these options (see Owen (1957–8)), and twentieth-century authors have devised further geometric, kinematic and dynamic paradoxes of infinity (Benardete, 1964; Grünbaum, 1967; Salmon, 1970). I shall review these paradoxes with a view to discovering their implications for the concept of infinity in the physical world, and in particular for the idea of grounding mathematical infinity in physical models.

4.2 Geometric paradoxes

First let us consider paradoxes relating purely to the structure of space (and matter extended in space), without involving motion. Zeno's paradox of extension was intended to refute the hypothesis that space is infinitely divisible and has indivisible parts (option 2.1.1 above). Under this hypothesis, a line segment consists of infinitely many indivisible parts, all of the same magnitude (by homogeneity). If the parts are of zero magnitude, then the sum of the parts must be zero (however many there are of them); whereas if the parts are of positive magnitude then the sum of them must be infinite. Yet the original line segment was of finite positive magnitude. This seems to rule out options 2.1.1.1 and 2.1.1.2.

The modern analytic theory of the continuum is devised to avoid paradoxes of this sort. This theory adopts option 2.1.1.1, but it also makes a number of distinctions that the ancient Greeks did not. It distinguishes between a line segment and its length; it distinguishes several notions of magnitude (ordinality, cardinality and measure); and it distinguishes several notions of 'sum' (the sum of finitely many numbers, the sum of an infinite series of numbers, and the union of sets). Notice the mixture of potential and actual views of infinity involved here: an infinite sum $\sum_{n=1}^{\infty} \frac{1}{2^n} = 1$ does not mean literally the addition of infinitely many numbers but the limit of a succession of finite sums $\frac{1}{2}, \frac{1}{2}+\frac{1}{4}, \frac{1}{2}+\frac{1}{4}+\frac{1}{8}, \ldots$ (i.e., potential infinity); whereas an infinite union of sets $\bigcup_{n=1}^{\infty} \left[\frac{1}{2^n}, \frac{1}{2^{n-1}}\right] = (0, 1]$ is interpreted literally as a straightforward pooling of the contents of all the sets at once (i.e., actual infinity). Having made all these distinctions, we can say that a line segment is the set of all its points and is the union of the singleton sets of its points; however, we can deny that its length is the sum of the lengths of the singleton sets. The line segment has positive length, while the singleton sets have zero length. The length (measure) function μ is countably additive, which means that $\mu(\bigcup_{n=1}^{\infty} A_n) = \sum_{n=1}^{\infty} \mu(A_n)$ provided the sets A_n are disjoint and measurable and the infinite series converges. This additivity property does not, however, extend to uncountable sums; indeed, there is no way of adding uncountably many numbers. Hence we cannot form the sum of the lengths of all the singleton sets in a line segment. Zeno's paradox is therefore blocked.

As a piece of pure mathematics this is perfectly consistent; indeed, some authors regard it as a resolution of the paradox (Grünbaum, 1967, chapter III). However, there are grounds for dissatisfaction with this as a theory of space. Let us consider, for example, *Thomson's Cheese-Grater* (Thomson, 1970). Compare the following two processes.

(i) Take a lump of cheese and chop it into halves, then chop *one* of the pieces in half, then chop *one* of the pieces so produced in half, and so on.

(ii) Take a lump of cheese and chop it into halves, then chop *both* pieces in half, then chop *all four* pieces in half, and so on.

Process (i) produces a countable infinity of pieces of cheese of positive size, and does not seem particularly paradoxical. But what does (ii) produce? Certainly not chunks of cheese, in the conventional sense. The only thing one can say, if one accepts the conventional theory of the continuum, is that it produces an uncountable infinity of 'cheese-points', dimensionless, indivisible pieces of cheese.

Since both procedures are, in principle, reversible, we ought to be able to stick the pieces together to recover the original lump of cheese. On the other hand, notice that two lumps of cheese of different size, if both subjected to process (ii), will produce identical sets of cheese-points: that is to say, the metric, order, topological and measure properties of the two lumps are broken up by the grating process, leaving just two sets of cheese-points of equal cardinality. Hence it should be possible to grate one lump of cheese by process (ii) and then reassemble it into a lump of a different size.

This of course is reminiscent of the Banach-Tarski paradox: a ball of radius 1 (thought of as a set of points) can be decomposed into six pieces and reassembled by rigid motions into two balls of radius 1 (Banach & Tarski, 1924).

Similar anomalies arise in the attempt to represent physical boundaries (Smith, 1997). Is the space occupied by a physical body an open set or a closed set? If two bodies are in contact, are the points on the boundary between them occupied by both bodies or neither? If a disc is divided into two halves, one red and one green, what colour is the boundary? One possible answer would be that colour properties only apply to regions, not points; but if the sky varies in colour continuously from blue in the east to white in the west, then it is very natural to model this by assigning a different colour to each point. A mathematician's solution to these problems would be to represent a colour distribution using a function from points to colours, but to regard two such functions as equivalent if they differ only on a set of measure zero, and then to define a colour distribution as an equivalence class of functions rather than as a single function. A similar technique can be applied to the space occupied by bodies. It could be said that this approach amounts to not taking points very seriously.

These issues of spatial modelling are of practical concern to computer scientists in the fields of artificial intelligence, computational linguistics, qualitative spatial reasoning, geographical information systems, and spatial perception (Vieu, 1993; Smith, 1996; Varzi, 1996). These fields all face the problem of representing and reasoning about real-world objects, masses of liquid, boundaries, holes and shadows, their positions in space and their evolution through time. In a computational setting, finitist representations are forced on us: it is not helpful to represent everything as an uncountable set of points. Mereology is found to be more suitable than set theory, in the spirit of option 2.2.

These examples provide reason for doubting the suitability of the conventional theory of analysis for representing space. As Dummett (2000) complains, the conventional theory provides excess expressive possibilities: it allows one to formulate distinctions and thought experiments that one would prefer to ban on geometric grounds. One may well want to rule out the cheese-grating process (ii), while permitting (i); but on what basis can one justify treating them differently?

The difficulties we have considered in this subsection will recur more forcibly when we proceed to consider kinematic and dynamic paradoxes.

4.3 Kinematic paradoxes

Next let us consider paradoxes involving change and motion (but without considering the forces and dynamical laws that produce motion).

The options listed in §4.1 for the spatial continuum also apply to the temporal continuum. Zeno's paradox of the arrow was directed against the view that time consists of indivisible parts (options 1 and 2.1.1). (I am loosely following the interpretation of Zeno's paradoxes given by Owen (1957–8).) If there are indivisible parts of time (instants), then, during one instant, an arrow in flight cannot move. That is to say, it cannot change its position, since if it did it would be occupying different positions at different parts of the instant, and we would have conceded that the instant was divisible. On the other hand, we are also assuming that time consists entirely of instants, so if the arrow does not move in any instant then it does not move at all.

This paradox is usually resolved by saying that motion is just a matter of being at different positions at different instants of time (this is known as the 'at-at' theory). If we confine our attention to a single instant then the question of motion does not arise: the arrow just has a single position. The concept of motion only applies to intervals of time. Admittedly, we do often speak in physics of the 'instantaneous' velocity of a particle, but this just means the *limit* of its velocity over an interval as the length of the interval tends to zero. This resolution of the paradox seems mathematically and physically satisfactory, provided one has no other objection to indivisible parts of time.

Zeno's paradox of the stadium seems to be directed against the view that space and time consist of indivisible parts of positive length (options 1 and 2.1.1.2). Zeno considers two lines of chariots moving in opposite directions at a speed of one indivisible part of space per indivisible part of time. By considering the relative motion of the two lines, Zeno finds that we are more or less forced to subdivide our parts of space or time. Alternatively one could say that motion that appears smooth at the macroscopic scale consists of a jerky, non-consecutive traversal of positions at the microscopic scale. Since discrete theories of space and time have never been popular I shall not dwell on this paradox further.

More troubling are Zeno's dichotomy paradox and Achilles paradox, which are directed against option 2.2, the view that space and time are infinitely divisible but that the process of infinite subdivision cannot be surveyed as a whole. (These paradoxes also apply against option 2.1.2, at least in its Peircean form in which the process of subdivision is transfinite and incompletable; but I shall stick to option 2.2 for expository convenience.) To traverse a spatial interval $[0, 1]$ one must begin by crossing the first half, $[0, \frac{1}{2}]$; but to do so one must begin by crossing the first half of that, $[0, \frac{1}{4}]$, and to do this one must begin by crossing the first half, $[0, \frac{1}{8}]$, and so on. Thus one can never get started. This is the *regressive* version of the dichotomy paradox. The *progressive* version says that to complete the crossing of $[0, 1]$ one must cross $[\frac{1}{2}, 1]$; to do that one must cross $[\frac{3}{4}, 1]$, and so on. The Achilles paradox involves a race between Achilles and the tortoise, in which Achilles runs ten times as fast as the tortoise but the tortoise has a head start of 1 unit of length. Achilles must traverse the interval $[0, 1]$ to reach the tortoise's initial position, by which time the tortoise has reached 1.1; so Achilles must next traverse $[1, 1.1]$, by which time the tortoise has reached 1.11; so Achilles must traverse $[1.1, 1.11]$; and so on. Thus Achilles must traverse infinitely many intervals to catch up with the tortoise.

In all three cases, a smooth motion is analysed into infinitely many parts by repeated subdivision. Under option 2.2, this subdivision process cannot be thought of as a finished whole; hence the motion can never be completed.

Aristotle believed that this argument could be answered within the terms of option 2.2. The *motion* can be completed but the *subdivision process* cannot. The points of subdivision exist only in a potential sense; hence Achilles does not actually have to traverse infinitely many points (Huggett, 1999, pp. 31–6). Unfortunately, this solution cannot cope with the 'staccato' version of the paradoxes (Benardete, 1964, pp. 8–9; Grünbaum, 1970). In the staccato form of the progressive dichotomy, we traverse $[0, \frac{1}{2}]$ in $\frac{1}{4}$ second, then pause for $\frac{1}{4}$ second, then we traverse $[\frac{1}{2}, \frac{3}{4}]$ in $\frac{1}{8}$ second, then pause for $\frac{1}{8}$ second, and so on, thus reaching the point 1 after 1 second. In this case the spatial points $\frac{1}{2}, \frac{3}{4}, \ldots$ are individuated by the traversal process, so it cannot be maintained that they are merely potential points.

In fact, this is just one of a long list of modern paradoxes involving 'supertasks', i.e., infinite sets of discrete actions accomplished in a finite time; the idea is that one specifies the individual actions and then enquires what the outcome of the whole process would be. I am only concerned with the conceptual coherence of supertasks here; see Davies (2001) and Cotogno (2003) for discussion of how they might be realised in Newtonian and general relativistic dynamics. Let us first consider supertasks of order-type ω. These involve an infinite sequence of actions $a_1, a_2, a_3, \ldots$, where action a_n is carried out in the time interval $(1 - 2^{-n+1}, 1 - 2^{-n})$. We are assuming here that any given action can be carried out at an arbitrarily high speed. All the actions are completed in the time interval $(0, 1)$. The question is, what is the resulting state of affairs at time 1?

The simplest and most telling example is Thomson's lamp (1954–5). The lamp has a button that switches it from off to on or from on to off. Initially the lamp is off. Action a_n consists of pressing the button. At time 1, is the lamp on or off? There would seem to be five possible answers.

(i) The lamp is on.
(ii) The lamp is off.
(iii) The situation is indeterministic: both outcomes at time 1 are causally possible, given the course of events before time 1.
(iv) The situation is indeterminate: our physical laws break down here and are unable to tell us which outcomes at time 1 are permitted, given the course of events before time 1.
(v) The situation is underdetermined: the outcome depends on other information about the events before time 1 not provided in the description.

Answers (i) and (ii) can be rejected as arbitrary: the problem description is symmetric between on and off, and cannot imply an asymmetric outcome. Answers (iii) and (iv) are very odd, but we shall encounter examples of such unexpected indeterminism and indeterminacy shortly when we come to dynamic paradoxes. Benacerraf (1962), Grünbaum (1967, pp. 92–102) and Harrison (1996) choose answer (v): for them, the outcome depends on the internal mechanism of the lamp switch. Suppose the button controls the position of a lever; if the height of the lever is zero an electrical circuit is made and the lamp is on, whereas if the height is non-zero the circuit is broken and the lamp is off. Consider two possibilities:

(a) a 'discontinuous' version, in which the lever rises to a fixed height h whenever the lamp is switched off;
(b) a 'diminishing' version, in which, due to the increasing rapidity of switching on and off, the lever rises to decreasing heights h_n (at the nth switching off), where $h_n \to 0$ as $n \to \infty$.

Let $f(t)$ be the position of the lever at time t. The function f is said to be *continuous at 1* iff $f(1) = \lim_{t\to 1} f(t)$. In version (a), f is discontinuous at 1; in fact, the one-sided limit $\lim_{t\to 1^-} f(t)$ does not exist. Some authors (Black, 1950–1; Grünbaum, 1970) dismiss discontinuous trajectories as 'kinematically forbidden', though without giving any justification. A possible *dynamical* motive for insisting on continuity might be a belief that the evolution of a system is governed by differential equations, which would certainly impose smoothness conditions on any dynamically realisable trajectory. If one does not assume the use of differential equations, then on what other basis could the present state of a system determine its future? Is $f(1)$ causally determined by the values of f on $(-\infty, 1)$? If the answer is no then we are back to answers (iii) and (iv).

In version (b), we have a well-defined limit $\lim_{t\to 1^-} f(t) = 0$, so if f is continuous then $f(1) = 0$ and the lamp ends up on.

Many other supertasks of order-type ω also admit a 'discontinuous' version and a 'diminishing' version. The discontinuous version is either banned (if one insists on continuity), indeterministic or indeterminate. The diminishing version has a unique outcome consistent with continuity. For example, Grünbaum (1970) considers both discontinuous and diminishing versions of the staccato dichotomy paradox. Black's (1950–1) infinity machines (which transfer infinitely many marbles from one tray to another) are discontinuous cases, though Grünbaum (1967, p. 103) considers a diminishing version. Benacerraf's (1962) vanishing and shrinking genies also fit into this classification.

We have seen that we can only derive an outcome for a supertask of order-type ω if we are willing to assume continuity (or, to be precise, left-continuity, $f(1) = \lim_{t\to 1^-} f(t)$, where $f(t)$ is the state of the system at time t). Even assuming continuity, however, the outcome can be ambiguous, as for example in various paradoxes involving putting balls into urns (Allis & Koetsier, 1991; Friedman, 2002). In the Littlewood-Ross paradox, a countable infinity of balls is numbered $1, 2, 3 \ldots$; action a_n consists of putting balls number $10n - 9, \ldots 10n$ into an urn and removing ball number n. At time 1, after all the actions have been carried out, what balls are in the urn? Infinitely many or none of them? Allis & Koetsier (1995) argue, using a continuity principle, that the urn should be empty: their argument amounts to considering the fate of each ball separately on the grounds that each ball is only affected by finitely many actions. On the other hand, if one rubs off the numeric labels on the balls and simply considers the number of balls, $N(t)$, in the urn at time t, one sees that $\lim_{t\to 1^-} N(t) = \infty$, so by continuity $N(1) = \infty$. It beggars belief that the outcome can depend so drastically on the presence of labels on the balls. This discrepancy arises of course because continuity depends on a topology on the set of states; Allis & Koetsier's argument relies on one topology and the opposing argument relies on another. Different topologies are suggested by different ways of describing the supertask and by different assumptions about which aspects are relevant. Hence the outcome is indeterminate until we can justify the choice of one topology.

Let us consider the consequences of this analysis for Russell's and Weyl's notion of an infinite computation, e.g., evaluating Goldbach's conjecture by testing each even number $4, 6, 8, 10, \ldots$ to see whether it is the sum of two primes. When we describe this supertask as a 'computation', we are presupposing two things.

(i) *Determinism*: the outcome of the computation must be determined by the instructions. We must get the same answer every time we repeat it.

(ii) *Abstractness*: the outcome should not depend on whether the computer is made of electronic, mechanical, optical or hydraulic components, but only on the computation performed. An evaluation of Goldbach's conjecture should only depend on arithmetic facts about even numbers and prime numbers, not on the type of hardware, nor the details of the design of the switches and logic gates, nor the colour of the equipment, nor the weather.

If these conditions are not satisfied then we cannot speak of an infinite *computation* but merely an infinite *experiment* and it is no use in elucidating the meaning of an infinitistic statement such as Goldbach's conjecture. Unfortunately, in our analysis we have discovered that there are four possible verdicts on supertasks of order-type ω: (a) banned on kinematic grounds; (b) indeterministic; (c) underdetermined (dependent on the detailed design of switches); and (d) indeterminate (dependent on an arbitrary choice of topology). Hence we do not yet have a convincing example of an infinite computation.

Let us move on to consider supertasks of order-type ω^*. These consist of a reversed infinite sequence of actions $\ldots a_5, a_4, a_3, a_2, a_1$, where a_n is carried out in the time interval $(2^{-n}, 2^{-n+1})$, or possibly (if one allows an infinite past) in the time interval $(-n, -n+1)$. Consider the following puzzle, first published in *Mathematics Magazine* in 1971.

> A boy, a girl, and a dog take a walk along a path; they depart from a common starting point. The boy walks at four miles per hour and the girl walks at three miles per hour; the dog trots back and forth between them at ten miles per hour. Question: at the end of one hour, what is the position of the dog and in which direction is it facing? Answer: the dog can be at any point between the boy and the girl, and it can be facing in either direction. Pick an arbitrary point and either direction. If you time-reverse the whole process, you will find that it invariably leads back to the stated starting conditions. (Salmon, 1970, preface to 2001 reprint, p. ix)

The situation is underdetermined: it needs a boundary condition specifying the dog's position and direction at one point in time. A similar observation applies to other paradoxes of order-type ω^*; for example, in a time-reversed version of Thomson's lamp, the lamp could be on or off after the final action a_1. This is not paradoxical. In the forwards version of Thomson's lamp we expect to have to specify the initial state of the lamp, so in the reversed version we should expect to have to specify the final state.

The example most relevant to our purposes is Benardete's (1964, pp. 123–4) backwards verification of Goldbach's conjecture. The action a_n consists of checking whether $2n + 2$ is the sum of two prime numbers; if it is not then we keep a record of $2n + 2$. At the end we have a list of all counter-examples to Goldbach's conjecture. Unfortunately this procedure is also vulnerable to underdetermination: the list may contain, in addition to the genuine counter-examples we have recorded, spurious numbers that are present because they were there at every stage. Moreover, we cannot check at the end whether these numbers are really counter-examples because there may be infinitely many of them.

Worse still, it is easy to design variations of this backwards computation that have no possible outcome at all: e.g., let a_n consist of testing whether n is a prime number and halting if it is, thus yielding the greatest prime number. This is closely analogous to a series of paradoxes presented by Benardete (1964, pp. 236–239, 252–261); the general idea of all of them is that an object encounters an ω^*-sequence of barriers, each of which would be sufficient to stop it, but since there is no *first* barrier the object encounters we cannot say which barrier has stopped it. The most troubling version of the paradox involves *non-existent* barriers (pp. 259–260). A man walks along the x-axis towards 0 from the negative direction. A sequence of gods $g_1, g_2, g_3, \ldots$ awaits to put barriers in his path. God g_n resolves to place a barrier at the position 2^{-n} if the man reaches as far as $\frac{3}{4}2^{-n}$. What happens as the man tries to pass through 0? He cannot reach any position beyond 0, since that would entail passing some barrier; so he never gets further than 0. But, in that case, none of the barriers is ever placed. So he is stopped by the gods' *unrealised intentions* to place barriers!

The man's progress is not blocked by a 'strange field of force' or 'before-effect' as suggested by Benardete and some later commentators. Indeed, as Yablo points out, the paradox does not even depend on the presence of the man (2000). Let B_n be the proposition that god g_n places his barrier (at time 2^{-n}, say). Let I_n be the proposition $B_n \Leftrightarrow \forall m > n \, \neg B_m$, which expresses g_n's intention to place a barrier iff no earlier god has done so. In formal logical terms, the set of propositions $\{I_1, I_2, I_3, \ldots\}$ is consistent but not ω-consistent and not satisfiable (i.e., not satisfiable by any standard model, in which the $\forall m$ quantifier ranges over the standard natural numbers). Hence Yablo is right to say that the gods are prevented from carrying out their intentions by pure logic. No contradiction can be deduced from the gods' intentions; no obstacle prevents any god from carrying out his intention; yet no state of affairs counts as all the gods fulfilling their intentions.

We can dispel the sense of mystery by viewing the matter in the following light: whenever we set up a supertask by specifying what is to happen at each step we are defining the evolution of the system by *recursion* on the steps. As is well known, recursive definitions work perfectly on ω and other well-founded orderings, but fail on non-well-founded orderings, such as ω^*, for which there may be zero, one or more solutions of the recursion equations – exactly as we found in our examples. This makes our paradoxes look a lot less paradoxical: this failure of recursion is in fact a perfectly familiar and well-understood mathematical phenomenon. This provides a strong reason for dismissing all supertasks of non-well-founded order-type as ill-defined.

Indeed, this rather undermines the idealised world-picture we have been working with. We have assumed that we are free to imagine an infinity of objects, to assign to them arbitrary properties, to arrange them in any non-overlapping configuration, to assume that any 'god' or machine (or whatever) will carry out its instructions correctly, and then to ask what happens when the set-up is left to run. We have found that in some cases the set-up simply *cannot* evolve and hence cannot exist in a world that has time. Nevertheless I shall not abandon the idealised world-picture; I shall persevere with it, assuming that it is well-behaved as long as we do not try to set up temporally non-well-founded systems of events.

4.4 Paradoxes involving sums of conserved quantities

It is a standard result of elementary analysis that the infinite series $\sum_{n=1}^{\infty} \frac{(-1)^n}{n}$ can be made to converge to any sum, or to diverge, by reordering the terms suitably (Haggarty, 1993, §4.2). This can be turned into a physical paradox if we imagine a countably infinite set of particles $p_1, p_2, \ldots$, where p_n has an electric charge (or any other conserved quantity) of $\frac{(-1)^n}{n}$. Suppose the particles collide simultaneously and coalesce. What is the charge of the resulting particle? The problem is that infinite sums are defined only for *sequences* of numbers, whereas the physical situation contains an *unordered set* of particles.

This type of paradox was introduced by Cooke, using examples with particles of unbounded density, unbounded speed or unbounded length (2003). If we are willing to assume an upper bound ρ on (positive or negative) charge density, an upper bound v on speed, and that the resulting composite particle is bounded by a sphere of radius r, then we can exclude the paradox as follows. Let us generalise the situation to allow the particles to have any charges $q_1, q_2, \ldots$, and let us suppose that the particles coalesce within a time period T. Then at the beginning of the time period they must all have been bounded by a sphere of radius $r + vT$, and hence $\sum_{n=1}^{\infty} |q_n| \leq \frac{4}{3}\pi(r + vT)^3\rho$. Thus the sequence (q_n) is absolutely convergent, and hence (by a standard theorem) its sum $\sum_{n=1}^{\infty} q_n$ converges and is independent of the order of terms. In particular, coalescence in a finite time is impossible in the case $q_n = \frac{(-1)^n}{n}$.

Unfortunately, Cooke's paper also shows how to realise the summation $\sum_{n=1}^{\infty} \frac{(-1)^n}{n}$ as a sum of gravitational forces, using a static configuration of particles of equal mass and volume; this example cannot be excluded by limits on density, speed or size. Cooke suggests that there may be some 'intrinsic ordering' hidden in the physical situation that specifies how the terms are to be summed, but it is very hard to see where such an ordering could be found.

There is a second type of problematic infinite summation that occurs in physics, where a sum or limit of positive quantities diverges unexpectedly in what seems to be a feasible physical situation. In classical physics the electromagnetic or gravitational self-energy of a point particle diverges (that is, it tends to infinity like $\frac{1}{r}$ as the radius r tends to 0, for fixed charge or mass). There is also Olbers' paradox of conservation of flux (Wesson, 1991): if an infinite static universe is sprinkled evenly with stars of uniform luminosity then the light intensity at any point tends to infinity over time.

These paradoxes provide some motivation for abandoning the 'limit of partial sums' view of infinite summation and re-asserting a naive 'actual infinity' view, as suggested in §1, under which infinitely many quantities (of the same qualitative kind) can be simply lumped together into a total quantity, in precisely the same way that finitely many quantities can (cf Benardete, 1964, p. 26). This entails accepting the possibility of infinite quantities, of course. The $\sum_{n=1}^{\infty} \frac{(-1)^n}{n}$ problem can be dismissed by rejecting the concept of negative quantities. This view fits quite well with the way we naively think of quantities: intuitively, a quantity (e.g., the lifetime of the universe) can be finite or infinite but cannot be negative. Negative quantities are merely a figure of speech: a 'negative acceleration' is not really an acceleration at all but a deceleration; a 'negative profit' is not really a profit but a loss. Profits and losses differ qualitatively (not merely in sign), so, although it is always possible to add infinitely many profits together to give a (finite or infinite) total, we cannot be expected to add infinitely many profits and losses together. This view merits further development; notice, however, that it involves unpicking the laboriously constructed solution to Zeno's paradox of extension adopted by conventional mathematics (see §4.2).

4.5 Paradoxes of collision

Consider an infinite collection of bodies that interact only by elastic collision, according to Newton's three laws of motion. Assume that the bodies can be made as nearly rigid as required: i.e., any collision can be assumed to be completed in an arbitrarily small time. Newtonian mechanics is commonly supposed to be deterministic and to satisfy laws of conservation of mass, momentum and energy, but there are four ways in which these properties can be subverted.

The first method is by escape to infinity. Pérez Laraudogoitia (1997) describes how, by an ω-sequence of elastic collisions, a particle can be made to double its speed at ever-halving intervals and so escape to spatial infinity in a finite time (like Benardete's metaphysical rocket (1964, p. 149)). Hence the particle disappears from space (and in fact so do all the other particles involved in the collisions). This phenomenon of escape to infinity is a genuine feature of Newtonian mechanics, not an artifact of our unrealistic assumptions of infinitely many objects and perfectly elastic collisions: Xia (1992) has shown that five point-mass particles interacting via Newton's law of gravity can escape to spatial infinity in a finite time without colliding. Moreover, since Newton's laws are time-reversible, it is equally possible for particles to appear spontaneously from infinity, in an ω^*-sequence of collisions. We know from §4.3 that ω^*-sequences of actions can be indeterministic, so perhaps this is no great surprise. (There are also cases in which collision set-ups of order-type ω^* have no solutions at all, such as the one described by Angel (2001), but I shall not consider this phenomenon as it has already been dealt with in §4.3.)

The second method is by using infinite mass to evade the conservation laws. Consider infinitely many balls $B_1, B_2, B_3, \ldots$ of equal mass, where B_n has radius 2^{-n}. We can enclose them all in a spherical shell to produce a composite body of infinite mass and finite volume. Take two such bodies and collide them; what happens? The result is indeterminate, since total mass and momentum are undefined, so the law of conservation of momentum cannot be applied.

The third method involves momentum disappearing into an ω-sequence. Pérez Laraudogoitia (1996, 1999) imagines a moving particle P_0 colliding elastically with a sequence of stationary particles $P_1, P_2, P_3, \ldots$, all of equal mass. In accordance with Newton's laws, P_0 passes all its momentum on to P_1, which then passes all its momentum on to P_2, and so on. After a finite time all the collisions are complete and all the particles are stationary. The momentum has disappeared. In the time-reversed version motion appears spontaneously from stationary particles.

Pérez Laraudogoitia's (2002) example of spontaneous self-excitation exploits both the second and third methods.

The fourth method is by imitating Thomson's lamp (see §4.3): we set up an oscillating ω-sequence of collisions that has no possible outcome compatible with continuity (Earman & Norton, 1998; Pérez Laraudogoitia, 1998). Newton's laws are indeterminate in such a case. Pérez Laraudogoitia suggests that the particles simply disappear when their world-lines cannot be continued in a continuous way. He also believes that the situation is time-reversible, so that particles can appear unpredictably out of the void.

Perhaps it is time to take a closer look at conservation laws. Earman (1986) points out that they can be formulated in a variety of ways. In most of our examples momentum is conserved *locally* (at each collision) but not *globally* (the total momentum of the system is not equal at all times). The relation between the local and global conservation laws can be most easily seen from field theory (Weinberg, 1995, §7.3), since any situation involving particles can be modelled in terms of density fields. A continuous symmetry of the action implies a local conservation law

$$\frac{\partial J^\mu(x)}{\partial x^\mu} = 0$$

where J is a four-vector current field, $x = (t, \mathbf{x})$ is a point in space-time, and μ is a Lorentz index. Using Stokes' theorem we can deduce the global conservation law

$$\frac{dQ(t)}{dt} = 0, \quad \text{where } Q(t) = \int J^0(t, \mathbf{x})\, d^3\mathbf{x}.$$

This argument assumes that J vanishes sufficiently fast at large spatial distances. It also presupposes that J is differentiable. These two conditions rule out *all* the above cases. Any case involving escape to infinity or particles spread out over unbounded space violates the condition of vanishing at infinity. Any case involving infinitely many equal masses in a bounded space-time volume violates the differentiability condition, since, by the properties of compact sets, there must exist a point every neighbourhood of which contains infinite mass.

Hence, in mathematical terms, there is nothing paradoxical about the above cases: they simply demonstrate that conservation laws are subject to conditions of smoothness and vanishing at infinity. However, they are certainly paradoxical physically. After all, the purpose of conservation laws is to explain the stability of the world; if the laws are formulated in a weakened form that permits matter and motion to appear and disappear spontaneously (as advocated by Earman (1986) and Pérez Laraudogoitia (1998)) then they fail to serve their purpose and we need *another* physical principle to explain why this never seems to happen in nature.

4.6 Conclusions: does nature abhor infinities?

It is very natural to assume that the world is an infinitely divisible space-time manifold containing a possibly infinite number of non-overlapping particles with arbitrary values for speed, mass, etc.; we use this picture as the starting point for our physical theorising and as an aid to thinking about actual infinity in pure mathematics. All the physical paradoxes we have considered involve problems with this idealised picture or a mismatch between it and the mathematical systems we use in our physical theories. None of the paradoxes reveals any inconsistency or conceptual incoherence in the mathematical systems considered as pure mathematics. Equally, none of them would be troubling to a physicist. Worse things happen in physics every day. Physicists are used to calculating with infinite and infinitesimal quantities to obtain finite results, most notoriously in the divergent integrals and divergent perturbation series that occur in quantum field theory; they think nothing of treating space-time as $(4 + \varepsilon)$-dimensional, where ε is a small complex number, rather than 4-dimensional, if that is what it takes to make their integrals converge (this technique is known as dimensional regularisation (Weinberg, 1995, §11.2)). Physicists are tolerant of infinities appearing in their calculations because they regard their theories merely as low-energy approximations to some more comprehensive theory in which the calculation would converge (Weinberg, 1995, §12.3). The infinities in quantum field theory are believed to be ultimately due to the use of local field operators, which embody the idealised view that particles and their interactions can be localised to a point (Cao & Schweber, 1993). More generally, infinities arise in calculations due to unrealistic simplifying assumptions and they are dealt with by modifying the assumptions.

Nevertheless, the paradoxes are problematic from the point of view of this essay, namely the attempt to use physical models to demonstrate the conceptual coherence of actual infinity, in order to justify its use in mathematics. The geometric paradoxes cast doubt on the view that space is an infinite set of points; the kinematic paradoxes undermine the notion of an infinite computation; and the dynamic paradoxes undermine the notion of an infinity of objects. The

ω^*-sequence paradoxes discussed at the end of §4.3 are particularly troubling. If the world could contain infinitely many objects, what is to stop them from arranging themselves into a configuration in which they cannot evolve?

One possible reaction to the paradoxes is to impose boundary conditions on our idealised picture, such as banning infinite numbers of objects, imposing a bound on speed and density, or insisting that matter is represented by fields that are differentiable and vanish at infinity. This would certainly tame the paradoxes but is hard to motivate on *a priori* grounds.

A second possible reaction is to reject the idealised picture as incoherent and insist that we stick to real physics, in which paradoxes are excluded by the speed of light limitation, the atomic structure of matter, the uncertainty principle, or some other limiting principle. Note that each such limiting principle is merely contingent, but it may be a necessary truth that *some* limiting principle exist to cover each paradox.

Certainly, the idea of a world of infinitely many objects is not an innocent and straightforward extension of the idea of a world of finitely many objects. Unexpected global difficulties arise when one passes from finite to infinite, and they are different from the difficulties that arise for infinity in pure mathematics. That is not to say that these difficulties are insuperable, merely that supporters of actual infinity have a lot of explaining to do.

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