

clearly not formalize them the same way – unlike, say, (GP) and (PG) above. Indeed, if the propositions of arithmetic could be seen to have the same ‘content’ straight off, there would be little point in seeking to *demonstrate* their ‘logical equivalence’.

The differences between arithmetical propositions are therefore as important as what they have in common. Basing a criterion merely on ‘logical equivalence’, then, even if captured by (CC*) rather than (CC#), fails to do justice to all that seems to be involved in talk of ‘conceptual content’. So let us formulate a third version of (CC):

(CC†) Two propositions have the same *cognitive content* iff they are *cognitively equivalent*.

Unlike the previous two types of equivalence, however, ‘cognitive equivalence’ is not itself defined, although it may be possible to illustrate what is meant, as in the example just used, in particular cases. If it is ‘cognitive content’ rather than ‘logical content’ that becomes ‘Sinn’, then it is precisely (CC†) that needs clarification.³⁶ Unfortunately, as we shall see in the second half of this book, Frege’s own characterizations of ‘Sinn’ are not entirely consistent, and the issues involved are controversial. But before discussing this, we must first provide an account of Frege’s logicist project, which was responsible not only for motivating the distinction between ‘Sinn’ and ‘Bedeutung’, but also for the tensions that nevertheless remain.

3. Frege’s Critique of Mathematical Reason

... all the things that are known have a number – for without this nothing could be thought of or known. (Philolaus, 5th century BC.)¹

As we have seen, the main purpose of Frege’s ‘Begriffsschrift’ was ‘to test in the most reliable way the validity of a chain of inference and to reveal every presupposition that tends to slip in unnoticed, so that its origin can be investigated’ (BS, ‘Preface’). Frege’s aim, more specifically, was to examine *arithmetical* chains of reasoning; and it was this that prompted the expansion of logic that allowed satisfying analyses of statements of multiple generality, statements which are clearly prevalent in mathematics (e.g. ‘Every number has a successor’, ‘Every even number is the sum of two primes’). But, as the remark just quoted indicates, the aim was not just to test the *validity* of arithmetical chains of reasoning; it was also to identify the *assumptions* upon which those chains rested, so that their status could be ascertained. More specifically still, the aim was to reveal their *logical* status – in short, to demonstrate that arithmetic was *reducible* to logic; and it is this logicist project that I am concerned to elucidate in this and the following chapter.

In this chapter I focus on Frege’s motivations and on his critique of rival conceptions of mathematics, clearing the way for an exposition of the more positive part of his programme in the next chapter. I start by sketching the developments in mathematics that form the background to Frege’s thought (§3.1), and then consider his own early mathematical work (§3.2), and the analysis of mathematical induction that he provided in his *Begriffsschrift* (§3.3). In the final two sections I offer an account of the first half of Frege’s *Grundlagen*, discussing, firstly, his criticisms of Kant’s and Mill’s views on the supposedly synthetic character of arithmetical truths (§3.4), and secondly, his attack on certain positions regarding the nature of number – empiricism, psychologism, and what he calls ‘set theory’ (§3.5).

3.1 The Mathematical Background

Pythagoras, reputedly, was the first to suggest that the world was governed by number and could be explained in mathematical terms. According to Aristotle’s account in the *Metaphysics* (985b23-986a22), the

Pythagoreans (who flourished in the 5th century BC) believed that numbers were the basic elements of the universe. Given that numbers were conceived as the *units* of existence, this may not be as absurd a metaphysical view as one might think, and it was not until Frege that the notion of a 'unit' received the critique it required (see §§3.5 and 4.1 below). But as the quote from Philolaus at the beginning of this chapter suggests, Pythagoreanism also had an epistemological dimension – that we can have no *knowledge* of anything without ascribing it a number; and this fundamental Pythagorean belief was certainly endorsed by Frege, and given a more powerful expression by equating the laws of number which were regarded as lying at the base of our understanding of the world with the laws of logic.

Numerical relationships were seen by the Pythagoreans as underlying both music and astronomy, the latter mediated by geometry – the study of spatial properties. This might suggest that arithmetic was viewed as more fundamental than geometry, but if such a belief was ever articulated by the early Pythagoreans, it was certainly not held by the later philosophers of antiquity. Geometry, just because of its central role in the explanation of the physical universe, was regarded as the fundamental science; and it was geometry that Euclid axiomatized around 300 BC. Numbers, as the *measure* of things, were given a geometrical interpretation; and arithmetic was generally taken as playing an auxiliary role. The classical Greeks, for example, believed that no more than three numbers could be multiplied together, since space had only three dimensions; and our own talk of ' x^2 ' as ' x squared' and ' x^3 ' as ' x cubed' still reflects their original geometrical interpretation.²

If Pythagoreanism is right that we can have no knowledge of anything without ascribing it a number, how is it that we have knowledge of number itself? If numbers are themselves things, is there not a danger of an infinite regress? In its general form, this question greatly occupied Plato: if knowledge requires reasoning (it cannot be mere true belief), then must we not already know how to reason? Since, according to Plato, the sensible world cannot supply the forms of reasoning, due to its changing character, then we must already be in touch with an eternal world of Ideas. Such a view might seem plausible in the case of mathematics. For we seem to know what the properties of geometrical figures are, for example, *independently* of sense-experience; 'real' circles and triangles, after all, do not exist in the physical world. If epistemological Pythagoreanism is an attractive position to adopt, then there would seem to be an inevitable progression into metaphysical Platonism. As we shall see, just such a progression occurred in Frege's thought.

The ancient Greeks undoubtedly believed in the power of human reason, and as we saw in chapter 1, it was Aristotle who recognized that if knowledge is arranged in a deductive system, knowledge of the axioms must be assumed as given. The model for scientific knowledge, as pro-

pounded by Aristotle and exemplified, most famously, in Euclid's work, was thus clear: we *intuit* the axioms and *deduce* the theorems. But what deserves note is that this model was only applied, in mathematics, to *geometry*; the development of arithmetic proceeded in a more *ad hoc* and less rigorous way, governed largely by considerations of utility.

The history of the development of our conception of number is both fascinating and philosophically instructive. Take, for example, the case of irrational numbers. The Pythagoreans had discovered, and indeed proved, that there were some ratios, such as that of the hypotenuse of a right-angled isosceles triangle to one of its other sides, i.e. $\sqrt{2}$, that were incommensurable with the natural numbers, i.e. could not be represented as a ratio of natural numbers. But they were nevertheless unhappy with such irrational numbers, and it was only when Eudoxus, a younger contemporary of Plato, provided a geometrical interpretation that they began to be accepted. No purely arithmetical justification was offered. Although after the demise of Greek civilization, the Hindus and Arabs kept the torch of arithmetic alight, the logical structure of arithmetic remained unexplored until the Renaissance. The Hindus, for example, not only used irrational numbers but also introduced negative numbers, but their interest was pragmatic.³

During the 16th and 17th centuries, the nature and status of the various types of number began to be debated. Descartes, for example, was uneasy about negative numbers; and Pascal notoriously remarked: 'too much truth bewilders us. I know people who cannot understand that 4 from 0 leaves 0' (*Pensées*, p. 92). Complex numbers also emerged, as the roots of quadratic equations were derived; and these too were controversial. Descartes termed them 'imaginary', and Newton referred to them as 'impossible', because they lacked a physical interpretation; and once again, it was only when it had been shown how to represent them geometrically (on a plane) that their use became accepted.⁴

One of the most important developments in this period, however, was the rise of algebra, although its origins can be traced back, as in other fields, to the ancient Greeks. As we saw in chapter 1, in inventing logic, Aristotle had introduced schematic letters, or 'term variables' as they are sometimes also called, to enable the general form of a proposition to be represented (as in 'All A's are B'); and this device was carried over into mathematics, appearing first in Euclid's *Elements* ('AB', for example, being used to represent a line). In the 3rd century AD, Diophantus took an important step in introducing numerical variables, corresponding to our modern symbolism of ' x ', ' x^2 ', ' x^3 ', etc.⁵ But it was only in the 16th century that algebra finally got off the ground, with the publication of Cardan's *Ars Magna* in 1545, presenting various results, and Vieta's *In Artem Analyticam Isagoge* in 1591. Vieta added schematic letters to the numerical variables already being used in algebra, so that quadratic equations, for example, could be represented in the form ' $ax^2 + bx + c = 0$ ', permitting

greater generality. Vieta also distinguished between *logistica numerosa* (calculation with numbers) and *logistica speciosa* (calculation with types), the latter constituting his new algebra, concerned with the (schematized) types of equations.⁶

Although algebra was seen as a method of analysis rather than a discipline in itself (an 'art' rather than a 'science', as the titles of Cardan's and Vieta's books suggest), its significance should not be underestimated. Its potential was realized with the creation by Fermat and Descartes of analytic or coordinate geometry, which provided algebraic description of geometric figures. Lines could be represented in the form ' $y = ax + b$ ', and circles of radius r in the form ' $y^2 = r^2 - x^2$ ', for example. Since investigations could then be carried out in a more abstract and formal way, this freed mathematicians of the need to visualize everything geometrically. Euclidean geometry was still regarded as providing the fundamental framework of interpretation, but it did eventually allow the exploration of non-Euclidean geometries.

The 17th century also witnessed the rise of mathematical physics, and algebra played a further role in the formulation of the emerging laws. Galileo, for example, showed how the distance s travelled by a falling object varies according to the square of the time t taken, as given by the equation ' $s = kt^2$ ', where k is some constant. Certain variables of a physical system were thus revealed as *functions* of one another; and the concept of a *function* was to prove of critical significance. The theory of functions became a whole branch of mathematics, and as we saw in the last chapter, it was the extension of function-argument analysis to ordinary language that instigated Frege's revolution in logic. But the most immediate development, in the 17th century, was the creation of the differential and integral calculus; and this provoked enormous controversy.

To gain a sense of the dispute, let us use the example just given. Whilst we can readily speak of the *average speed* of a falling object over a certain time ($= s/t$), we also want to know its speed at any instant t_n . To determine this, we consider the average speed in the interval immediately following t_n , i.e. between time t_n and time $t_n + \delta t$, when the object has fallen from point s_n to point $s_n + \delta s$, where δt and δs are the relevant increments.⁷ Since $s = kt^2$,

$$\begin{aligned}(s_n + \delta s) &= k.(t_n + \delta t)^2, \\ &= k.t_n^2 + 2k.t_n.\delta t + k.(\delta t)^2.\end{aligned}$$

Subtracting $s_n (= k.t_n^2)$ from both sides, we obtain the formula for δs :

$$\delta s = 2k.t_n.\delta t + k.(\delta t)^2.$$

The average speed over the interval is thus given as follows:

$$\delta s/\delta t = 2k.t_n + k.\delta t.$$

As δt approaches 0, $\delta s/\delta t$ tends to $2k.t_n$, the *derivative* of kt^2 at $t = t_n$. ds/dt , the result of *differentiating* the original function $s = kt^2$, is thus $2kt$.

The problem can now be seen. For if δs and δt were indeed 0, then $\delta s/\delta t$ would be undefined, so how could its limit (ds/dt) be any number at all? And if they are not 0, but some finite quantity (however small), then it would seem that there can be no such thing, after all, as the speed of an object at an instant. Newton, who, with Leibniz, is credited with (independent) invention of the calculus, talked of 'fluxions' and 'evanescent quantities', but gave no clear account of what these were.⁸ Leibniz spoke of 'infinitesimals' – magnitudes that are smaller than any given quantity, but not zero. On the supposition that 'infinitesimals' exist, we clearly arrive at a contradiction; and although Leibniz himself explicitly rejected the supposition, remarking that infinitesimals were simply 'fictions of the mind useful for calculations, of the same order as imaginary roots in algebra', he still failed to distinguish clearly enough between the differential ds and the finite variable quantity δs (to use the terminology adopted above).⁹ Berkeley, in particular, was highly scornful of both Newtonian and Leibnizian conceptions, arguing that something of which one could have no idea at all (an 'evanescent quantity' or 'infinitesimal') was simply incoherent.¹⁰

The main problem in understanding the calculus (as Berkeley's criticisms highlighted) lay in attempting to conceive it empirically. But as Leibniz himself realized, more clearly than his contemporaries, the task was to *free* the calculus from any foundation in geometry, and to explain it in purely arithmetical and algebraic terms. This project was furthered by Euler, Lagrange and Lacroix in the late 18th century, and eventually taken up by some of the British mathematicians, such as Babbage and Peacock, having finally liberated themselves from the restrictive Newtonian tradition of seeking geometrical justifications, in the early 19th century. However, difficulties and complications kept on emerging. One assumption that had been made, for example, was that any continuous function has a derivative at each point. But this was discovered to be simply false. It might be true for functions that could be represented geometrically as 'smooth' curves, but was not true for functions with 'corners'; and in 1872 Weierstrass made public an example of a continuous function that had no derivative at any point.¹¹

The concept that held the key to the interpretation of the calculus was the concept of a 'limit'. For the derivative, dy/dx , of a function relating y to x is the value that the quotient $\delta y/\delta x$ *tends towards* as δx approaches 0. Since δy and δx do not approach 0 *independently* of one another, their quotient does not itself approach an undefined 0/0 but rather the *limit* of the relevant series. D'Alembert was one of the few who had recognized, early on, the importance of the idea of a limit, remarking in the *Encyclopédie* that the theory of limits is the basis of the true metaphysics of the calculus.¹² But it was only Cauchy's *Cours d'Analyse*, published in 1821,

that finally provided the framework for the necessary rigorization of the calculus – or *analysis*, as it came to be called. Cauchy recognized that the existence of a *limit* presupposes a *convergent* series, and he clarified the distinction between convergent and divergent series. Using the concept of a limit, he provided definitions of both the continuity and the derivative of a function.

However, Cauchy not only continued to provide geometrical interpretations but also retained the talk of *infinitesimals*, which he defined as variables whose values are less than any given quantity and which have zero for their limit. Whilst this can be interpreted harmlessly enough (as it arguably can in Leibniz's work), it is easy to slip into construing them not as *variables* but as *quantities*; and Cauchy himself did in fact trade on the ambiguity in one of his central arguments. It was only with the more refined notation (utilizing ' δ ' and ' ε ') introduced by Weierstrass, who is generally regarded as the founder of modern analysis, that misleading talk of infinitesimals was finally purged.¹³

Weierstrass was quite clear that if progress was to be made in areas where geometrical intuition gives out, then analysis needed to be grounded in number theory. Of course, this then meant that a rigorous understanding of *number* was required. But here too, as we have seen, there was no lack of controversy. Even in the 19th century, there were still disputes about negative numbers and complex numbers; and in 1843 Hamilton added to the problems by inventing *quaternions*, numbers of the form ' $a + bi + cj + dk$ ', where $i^2 = j^2 = k^2 = -1$, and $jk = i$, $kj = -i$, $ki = j$, $ik = -j$, $ij = k$ and $ji = -k$. In the case of quaternions, multiplication no longer obeyed the law of commutativity. When Cayley introduced *matrices*, a further type of *hypernumber*, the moral was clear: one cannot just assume that the basic properties of one number system automatically carry over to any enlargement of that system. This raised doubts about the possibility of a *general* algebra of number, refuting Peacock's 'principle of the permanence of forms', propounded in 1833 as part of an attempt at axiomatizing algebra, which had simply *stipulated* that an algebra that covered the natural numbers also encompassed other types of number.¹⁴

The demands of analysis made the lack of clarity about numbers particularly pressing, since to demonstrate that a certain series has a limit, one must show that a number exists that is the value of that limit. Mathematicians had for long known that irrational numbers could be represented as sums of infinite series of rational numbers. Leibniz, for example, had shown that $\pi/4$ was equivalent to $1 - 1/3 + 1/5 - 1/7 + \dots$ ¹⁵ But no general proof had been offered that *any* convergent series of numbers has a real number as its limit. Cauchy had merely proved that convergence was *necessary* for the existence of a limit, not that it was *sufficient*. Clearly, a theory of the real numbers was called for; and the first arithmetical attempt to do so was made by Weierstrass in the 1860s, and developed, most notably, by Cantor in the 1870s. The essential strategy

was to 'assign' the irrationals to convergent sequences of rationals (Cantor's 'fundamental sequences'), and then to show how the familiar properties of the real numbers could be derived. Since convergent sequences of *real numbers* (i.e. comprising the irrationals as well as the rationals) could now be constructed, one of Cantor's proofs consisted in demonstrating that the limits of these sequences were themselves real numbers (rather than yet a further type of number in turn requiring definition).¹⁶

A similar theory was developed by Dedekind in his *Continuity and Irrational Numbers*, published in 1872. Since the continuity of the real numbers had traditionally been represented geometrically, Dedekind started by considering what was meant by geometrical continuity, with the aim of developing an arithmetical alternative. What he noted was that if all the points of a straight line are divided into two classes such that every point in one class lies to the left of every point in the other class, then there exists one and only one point that effects this division (1872: p. 11). With this idea in mind, he then introduced the notion of a *cut* (*Schnitt*) in the series of rational numbers, dividing them into two classes A_1 and A_2 such that every number in A_1 is less than every number in A_2 . For any cut (A_1, A_2) , he remarked, there is a unique real number that 'corresponds to' or 'brings about' this cut (pp. 12-15).¹⁷ Where a cut results in A_1 having a greatest number or A_2 having a smallest number, we have a rational number; where it results in neither, we have an irrational number. Like Cantor, Dedekind went on to derive the familiar properties of the real numbers, and he showed that any cut in the *real numbers* produces one and only one such number. The similarity between Dedekind's and Cantor's approaches should be clear, since any (monotonic) convergent sequence determines a unique Dedekind cut, and for any Dedekind cut one can specify a convergent sequence.¹⁸

These theories generate as many problems as they resolve, however, not least at the philosophical level; and I shall return to this later, since Frege's own approach involves an analogous move, identifying numbers with extensions of concepts. But here we need only note one immediate result. For both Cantor's and Dedekind's accounts involve the notion of an *infinite set* of numbers (indeed, a set that is *actually* rather than merely *potentially* infinite); and this naturally leads to questions about the properties of such sets. How do we determine, for example, whether two sets have the same number or not? Cantor suggested that the key concept here was that of one-one correspondence: if two sets, whether finite or infinite, could be put in one-one correspondence, then they should be assigned the same number. On this conception, the set of natural numbers turns out to have the same cardinality as the set of rational numbers, and Cantor denoted this by ' \aleph_0 ' (aleph zero). The set of real numbers, on the other hand, proves to be larger than the set of natural numbers, and this number Cantor denoted by ' c ' (the number of the continuum). Having shown that

the set of all subsets of a given set is larger than the original set, since $2^n > n$, Cantor proved that c was equal to 2^c , and hypothesized that this was the next transfinite cardinal, \aleph_1 (aleph one) – Cantor's 'continuum hypothesis', which has still not been proved.¹⁹ Needless to say, transfinite numbers provoked even greater controversy than other types of number; and when Cantor discovered a paradox in 1895, a chain of events was sparked off that was eventually to lead to the devastation of Frege's own work and the sophistications of modern set theory.

Controversies and further developments aside, however, by the latter half of the 19th century number theory did seem to have finally freed itself from dependence on geometry, and was generally looking in better deductive shape. But in the meantime geometry too had undergone a transformation. Euclid's work, for so long the paradigm of a rigorously axiomatized and definitive system of truths, had increasingly been the target of criticism. In particular, attention had been focussed on Euclid's notorious fifth axiom, the 'parallel postulate'. Even Euclid had been unsure about its status, and attempts to improve its formulation or derive it from the other axioms had all failed. During the 18th century it gradually dawned on mathematicians that the parallel postulate was indeed independent of the other axioms, and that a consistent geometry could be developed that denied it. During the first half of the 19th century Gauss, Lobatchevsky and Bolyai constructed the first non-Euclidean system, hyperbolic geometry, in which the angles of a triangle add up to *less* than 180° ; and from the mid 19th century Riemann developed another non-Euclidean system, double elliptic geometry, in which the angles of a triangle add up to *more* than 180° and there are *no* parallel lines.

Mathematicians remained sceptical, though, as to the significance of non-Euclidean geometries, and many assumed that contradictions would sooner or later be discovered. But when double elliptic geometry was shown to be applicable to the surface of a sphere, 'lines' being interpreted as great circles, it became clear that non-Euclidean geometries were consistent if Euclidean geometry was consistent (since the configurations on which they could be mapped were Euclidean). Whilst this suggested that non-Euclidean geometries could be treated as models *within* Euclidean geometry, many mathematicians, from Gauss onwards, felt obliged to abandon the belief that Euclidean geometry was a system of absolute truths. By the end of the 19th century, Hilbert, in his own work on the foundations of geometry, was arguing that the axioms of geometry were simply formal schemata allowing a range of interpretations, but since he also proved that Euclidean geometry was consistent if arithmetic was consistent, its rigour was left untarnished.²⁰

By the latter half of the 19th century, then, the Pythagorean belief in the fundamental status of the laws of number had been reinvigorated, and the traditional (classical Greek) view of the relationship between geometry and arithmetic had been inverted. Very crudely, the situation might be

summarized thus. Non-Euclidean geometry could be modelled within Euclidean geometry; Euclidean geometry, through analytic geometry, could be grounded in number theory; transfinite numbers, hypernumbers, complex numbers and irrational numbers could be derived from the rational numbers; and rational numbers, both positive and negative, were definable in terms of the natural numbers. The natural numbers were still assumed as given; but with Frege, as we shall see, the final step in the reductive process was undertaken: the definition of the natural numbers in purely logical terms.

3.2 Frege's Early Work

Frege was educated at the Universities of Jena (1869-70) and Göttingen (1871-3). He took courses in mathematics and physics, but also two in philosophy (one on Kant at Jena, and the other on the philosophy of religion, taught by Lotze, at Göttingen), and submitted his doctoral dissertation, 'On a Geometrical Representation of Imaginary Forms [*Gebilde*] in the Plane' (*GR*), in late 1873.²¹ Although this work is mathematical, in the Gaussian tradition then dominant at Göttingen, its philosophical motivation is worth elucidating. Gauss, who had spent almost all his mathematical career at Göttingen (he died in 1855), had been one of those responsible for legitimating complex numbers by showing how they could be represented as points on a plane.²² In his dissertation, supervised by Schering (Gauss' official successor and editor of his works), Frege takes this a step further, by showing how imaginary forms can be represented geometrically.²³

Frege's dissertation opens with these words:

When we consider that the whole of geometry rests ultimately on axioms which derive their validity from the nature of our intuitive faculty, we seem well justified in questioning the sense of imaginary forms, since we attribute to them properties which not infrequently contradict all our intuitions. (*GR*, p. 1.)

The example that Frege gives is that of calculating the points of intersection of a straight line with a circle that lies outside that line, the equations for which yield imaginary points (*ibid.*). Such an imaginary form might strike us as simply impossible, but Frege sets out to show how the non-intuitive relations between imaginary forms can be replaced by intuitive ones (*GR*, p. 3). He considers a more familiar case to illustrate his project:

By way of comparison let us take forms at infinity, which do not occur in the space of our intuition either. Taken literally, a 'point at infinity' is even a contradiction in terms; for the point itself would be the end point of a distance which had no end. The expression is therefore an improper one, and

it designates the fact that parallel lines behave projectively like straight lines passing through the same point. 'Point at infinity' is therefore only another expression for what is common to all parallels, which is what we commonly call 'direction'. As a straight line is determined by two points, it is also given by a point and a direction. This is only an instance of the general law that, whenever we are dealing with projective relationships, a direction can represent a point. By designating the direction as a point at infinity, we forestall a difficulty which would otherwise arise because of the need to distinguish a frequently unsurveyable set of cases according to whether two or more of the straight lines in the set were parallel or not. But once the principle of the equivalence of direction and point is established, all these cases are disposed of at one blow. (*GR*, p. 1.)²⁴

Just as parallel lines, which are taken to meet at infinity, can be represented by projecting them onto the surface of a sphere, so too, Frege suggests, can a representation be found of imaginary forms (*GR*, pp. 2-3).

But if analytic geometry, utilizing the real number system, can handle standard Euclidean configurations, can it be extended by the incorporation of complex numbers to represent imaginary forms? As Frege puts it, 'It is now of the greatest importance to find out when a proposition which holds for real forms can be carried over to imaginary ones' (*GR*, p. 2). However, as Frege goes on to say, 'with few exceptions all the operations and concepts that occur in the case of real numbers can indeed be carried over unchanged to complex ones', and this 'justifies the introduction of imaginary forms into geometry' (*ibid.*). One concept Frege considers is that of distance. The distance r between two points (x_0, y_0) and (x_1, y_1) is given by the equation $r = \sqrt{[(x_1 - x_0)^2 + (y_1 - y_0)^2]}$. Since the use of such equations within analytic geometry involves 'operations and inferences which are equally applicable to complex numbers', they are also valid when there are complex coordinates. 'If we now take the view that what is essential to the concept of distance is not the intuitive character of a straight line but conformity to the laws of algebraic analysis, then we can apply the name "distance" also where the end points are imaginary' (*GR*, pp. 9-10). In this fashion, Frege shows, the traditional concepts of geometry can be extended to encompass imaginary forms.

Despite being a work in pure geometry, Frege's dissertation nevertheless hints at the subsequent direction of his thought. For he is clearly concerned with how results in one area can be extended into another (cf. *GR*, p. 3). What allows this is the underlying arithmetic, which can encompass the non-intuitable as well as the intuitable. That our ability to use arithmetic outstrips our powers of intuition is something that Frege emphasizes throughout his later work. Indeed, it provides the starting-point for Frege's next piece of work, his *Habilitationsschrift*, his dissertation for the *Venia docendi*, submitted in early 1874 to the University of Jena as part of his application for a teaching post.²⁵ Entitled 'Methods of Calculation based on an Extension of the Concept of Magni-

tude' (*MC*), it is in this work that Frege's foundationalist interests emerge.²⁶

Frege's *Habilitationsschrift* opens with a clear indication of how it developed out of his earlier work. 'When we consider complex numbers and their geometrical representation, we leave the field of the original concept of magnitude, as contained especially in the magnitudes of Euclidean geometry: its lines, surfaces and volumes' (*MC*, p. 56). The physical interpretation of the addition of such magnitudes, in terms of the space filled, is abandoned. 'All that has remained is certain general properties of addition, which now emerge as the essential characteristic marks of magnitude. The concept has thus gradually freed itself from intuition and made itself independent. This is quite unobjectionable, especially since its earlier intuitive character was at bottom mere appearance' (*ibid.*). Frege suggests, for example, that someone learns what an angle is, not by being presented with one, but by being shown how to *add* angles. 'And it is clear that a concept as comprehensive and abstract as the concept of magnitude cannot be an intuition' (*ibid.*).

According to Frege, there is thus an important difference between arithmetic and geometry as far as their foundations are concerned:

The elements of all geometrical constructions are intuitions, and geometry refers to intuitions as the source of its axioms. Since the object of arithmetic does not have an intuitive character, its fundamental propositions cannot stem from intuition either. And how could intuition guarantee propositions which hold for all such heterogeneous magnitudes, some species of which may still be unknown to us? (*MC*, pp. 56-7.)

Gauss, as a result of his work on non-Euclidean geometry, had departed from the traditional and Kantian view, and drawn a fundamental distinction between arithmetic and geometry, arithmetic being seen as a system of absolute truths, and geometry as an empirical discipline based on intuitions; and it might seem that Frege is endorsing that Gaussian view here. But this passage, like his remark at the beginning of his doctoral dissertation, leaves it unclear whether he thought that geometry was based on *empirical* intuitions, as Gauss himself had thought, or *a priori* intuitions, as Kant had thought.²⁷ Nevertheless, it is clear that by the time of the *Begriffsschrift*, Frege held a Kantian view of geometry, but, like Gauss, insisted on a separation between geometry and arithmetic, maintaining a Leibnizian view of the analytic *a priori* character of arithmetic. Only at the very end of his life did Frege propose a revision of these views.²⁸

Given that the concept of magnitude is not found in intuition but created by ourselves, Frege goes on, 'we are justified in trying to formulate its definition so as to permit as manifold an application as possible, in order to extend the domain that is subject to arithmetic as far as possible' (*MC*, p. 57). The concept of magnitude cannot be grasped without the concept of addition, Frege remarks, and it is addition that is 'the subject

of those fundamental propositions from which the whole of arithmetic grows as from a seed' (ibid.). All other methods of calculation derive from addition, repeated addition, for example, yielding multiplication. What is involved here, Frege suggests, is the iterative application of an operation, which can be represented by an appropriate function, such that the value of the function for a given argument can itself become the argument of that function (MC, p. 58). Adding 1, for example, can be represented by the successor function, $f(x) = x + 1$, adding 2 by $ff(x)$, and so on; doubling by $g(x) = x + x$, quadrupling by $gg(x)$, and so on.

What Frege is concerned with, in particular, is the magnitude that can be ascribed to operations. According to Frege, $fff(x)$, for example, has triple the magnitude of $f(x)$ (MC, pp. 58-9), and the main body of his *Habilitationschrift* consists in an investigation of the relationships that hold between various types of mathematical function. But the key point to note here is the central role that the notion of a function is suggested as playing in the required theory of magnitude, making possible the connection between different areas of arithmetic (MC, p. 58). As we saw in the last chapter, it was Frege's development of function theory that resulted in the logic of the *Begriffsschrift*, and the envisaged unification of arithmetic that provided the nucleus of Frege's Leibnizian aspirations.

On the completion of his *Habilitationschrift*, and on the necessary oral examination, public disputation and trial lecture, in May 1874 Frege was appointed *Privatdozent* at the University of Jena, where he stayed for the rest of his career.²⁹ In the first few years, his teaching load was heavy, and he only published four short pieces, three of them reviews, before the appearance of the *Begriffsschrift* in 1879.³⁰ In one of these, his review of H. Seeger's *Die Elemente der Arithmetik*, he notes the 'paucity of fundamental considerations', remarking that 'wherever proofs and justifications are most needed, they are found to be wanting' (RS, pp. 93-4); but otherwise there is no further indication of the revolution in logic and philosophy that the *Begriffsschrift* was to inaugurate.

3.3 The Analysis of Mathematical Induction

During the period between the completion of his *Habilitationschrift* and the publication of the *Begriffsschrift*, it is almost certain that Frege read (at least some of) Hermann Lotze's *Logik*, the first edition of which was published in 1874. Lotze had taught Frege at Göttingen, and although the course Frege had followed had been on the philosophy of religion rather than logic, Frege's developing interest in logic, his earlier acquaintance with Lotze, and Lotze's own reputation, would have made Lotze's *Logik* an obvious work to have looked at.³¹ It also now seems clear that one of the fragments published in his *Posthumous Writings* consists of a series of remarks inspired by the introduction to Lotze's *Logik*, the evidence suggesting that it was written sometime around 1876 or 1877.³² Exactly what

aspects of Lotze's thought influenced Frege, however, remains a matter of conjecture, but two features may be mentioned here that would have struck a sympathetic chord as Frege considered how to provide firmer foundations for arithmetic. The first is Lotze's suggestion that concepts should be treated as *functions*, rather than merely conjunctions, of component concepts.³³ Lotze developed no formal logic on the basis of this suggestion, and given that Frege himself had already appreciated the unifying potential of the notion of a function, there is no need to suppose that Lotze exerted any direct influence on Frege; but it does indicate that there was nothing unnatural, in the late 19th century, about the construal of concepts as functions.³⁴

Extending the notion of a function to encompass concepts, and hence treating mathematical functions as just a specific type of function, suggests a more intimate relationship between logic and mathematics than had traditionally been conceived, and this found expression in the second feature of Lotze's thought – his view that mathematics was grounded in logic.³⁵ Once again there is no reason to suppose that, without Lotze, Frege would not have formulated his logicist project; and since Lotze denied that the *superstructure* of mathematics could be incorporated into general logic, there remains a difference between the two thinkers.³⁶ We might perhaps see Lotze as issuing a challenge to show that the whole of arithmetic was reducible to logic, and Frege as responding to this, but Frege's earlier dissertations clearly reveal that he already believed that our understanding of certain fundamental mathematical notions was conceptually (and hence logically) based rather than grounded in intuition (whether empirical or *a priori*).

Whatever the influences on Frege may have been, however, by 1879 Frege was sure what his ultimate goal was – to provide arithmetic with the strongest possible foundations. In his 'Preface' to the *Begriffsschrift*, he states that 'The firmest proof is obviously the purely logical, which, prescind from the particularity of things, is based solely on the laws on which all knowledge rests'. Any particular truth may be established in a number of ways, some reflecting the history of its discovery, and some reflecting the psychology of the person propounding it. But the status of the truth depends on its 'most perfect method of proof' (ibid.), and hence the task is to find out if there is a purely logical method of proof. It was the project of showing that there are such methods in the case of arithmetic that Frege undertook.

Frege realized that to do this, he needed to develop a logical system that was sufficiently sophisticated to represent both arithmetical propositions and arithmetical forms of reasoning. As we saw in the last chapter, this led to the replacement of traditional subject-predicate analysis with function-argument analysis, the axiomatization of propositional logic, and the invention of the predicate calculus; and this expansion of logical theory was presented in Parts I and II of the *Begriffsschrift*. Here I shall focus on

the third and final Part, entitled 'Some Elements from a General Theory of Series', in which Frege uses his logical system to provide an analysis of mathematical induction.

Proof by mathematical induction – demonstrating that a certain property holds of all the members of a given series (or that a series of objects with a certain property can be generated) – involves showing, firstly, that the property holds for the initial member of the series, and secondly, that if the property holds for any arbitrary member of the series, then it also holds for the next member of the series (or that a further member of the series can always be generated from a given member or members). Clearly, mathematical induction constitutes an important form of reasoning within mathematics, so that establishing its logical nature would go some way towards establishing the logicist thesis. But Frege's interest here had a far deeper dimension than this. For the natural number series itself is generated inductively. One starts, for example, by defining 0 and 1, stipulating that these are numbers, and then, by defining the relation of succession, or addition, shows that if n is a number, then $n + 1$ is a number. A natural number can then be specified as a member of the series recursively generated from these initial definitions. As Frege wrote in the 'Preface' to the *Begriffsschrift*, 'The course I took was first to seek to reduce the concept of ordering in a series to that of *logical* consequence, in order then to progress to the concept of number'.

Frege starts by defining the notion of an *hereditary property*. A property F is *hereditary in the f -series* if the following condition is met (formulating it in modern notation rather than Frege's own concept-script):³⁷

(HP) $(\forall x) \{ Fx \rightarrow (\forall y) [f(x, y) \rightarrow Fy] \}$. (Cf. BS, §24, formula 69.)

' $f(x, y)$ ' is understood as symbolizing that 'y is a result of an application of the procedure f to x ' or, equivalently, that 'y bears the f -relation to x '. (HP) can then be read as follows:

(HP†) From the proposition that x has the property F , whatever x may be, it can be inferred that every result of an application of the procedure f to x has the property F . (Cf. BS, §24.)

Frege gives the following example to illustrate the idea. Let ' $f(x, y)$ ' mean that y is the child of x , and let F be the property of being a human being. Then the f -series is the series starting with x and continuing through the descendants of x , and it is clear that F is hereditary, since every child of a human being is in turn a human being (*ibid.*).

With the notion of an hereditary property, Frege proceeds to define the concept of *following in a series*, or as it would now be termed, the concept of the *proper ancestral* of a relation. Using (HP) to abbreviate the formula given above, ' b follows a in the f -series' can be defined thus:

(PA) $(\forall F) \{ [(HP) \& (\forall y) \{ f(a, y) \rightarrow Fy \}] \rightarrow Fb \}$. (Cf. BS, §26, form. 76.)

In words, this reads:

(PA†) From the two propositions that the property F is hereditary in the f -series and that every result of an application of the procedure f to a has the property F , whatever F may be, it can be inferred that b has the property F . (Cf. BS, §26.)

Clearly, if b does follow a in the f -series, then any hereditary property that a has will also be possessed by b , so that (PA) holds.³⁸ Conversely, if, whenever F is an hereditary property, and anything that is a result of an application of f to a has the property F , b also has the property F , then b must follow a in the f -series.³⁹ For consider the property of *following a in the f -series*. This clearly satisfies (HP), and hence is hereditary (cf. BS, §28, formula 97). Any result of applying f to a , of course, possesses this property (cf. BS, §28, formula 96), so that both conjuncts of the antecedent of (PA) hold. If (PA) itself holds, then (by *modus ponens*) we can deduce Fb , i.e. b has the property of following a in the f -series. So ' b follows a in the f -series' and (PA) are equivalent.

It is easy to see how a characterization of mathematical induction can now be provided. For we can immediately write down the following proposition:

(MI) $[Fa \& (HP) \& (PA)] \rightarrow Fb$. (Cf. BS, §27, formula 81.)

From Fa and (HP) we can derive $(\forall y) [f(a, y) \rightarrow Fy]$, from which, with (HP) again, by (PA) we have Fb . Expressed in words, (MI) reads:

(MI†) If a has a property F which is hereditary in the f -series, and if b follows a in the f -series, then b has the property F . (Cf. BS, §27.)

This is precisely the key step in mathematical induction. For with the additional assumption that the first member of the f -series has the hereditary property F , we can clearly show that every member of the f -series has the property F .⁴⁰

Frege goes on to define ' b belongs to the f -series beginning with a ', that is, what we would now term the *ancestral* of the f -relation, which we can formulate, very simply, thus:

(AR) $b = a \vee (PA)$. (Cf. BS, §29, formula 99.)

The reading here is equally straightforward:

(AR†) b is identical with a , or b follows a in the f -series. (Cf. BS, §29.)

In the remainder of Part III of the *Begriffsschrift*, Frege derives further properties of series, using the notions introduced, and offers one final definition, of the *many-one* relation. A procedure f is *many-one* (*eindeutig*) if the following condition obtains:

(MO) $(\forall x)(\forall y) \{ f(x, y) \rightarrow (\forall z) [f(x, z) \rightarrow z = y] \}$. (Cf. BS, §31, formula 115.)

Informally, this reads:

(MO†) From the circumstance that y is a result of an application of the procedure f to x , whatever x may be, it can be inferred that every result of an application of the procedure f to x is identical with y . (Cf. BS, §31.)

Although Frege does not do so in the *Begriffsschrift*, let us also formulate the condition that must be met for a relation to be *one-many*:

(OM) $(\forall x)(\forall y) \{ f(x, y) \rightarrow (\forall w) [f(w, y) \rightarrow w = x] \}$.

A *one-one* relation, we can then say, is a relation that is both many-one and one-many, that is, that fulfils the combined condition:⁴¹

(OO) $(\forall x)(\forall y) [f(x, y) \rightarrow \{ (\forall z) [f(x, z) \rightarrow z = y] \& (\forall w) [f(w, y) \rightarrow w = x] \}]$.

Developing Frege's earlier example, we can say that the relation of parent to eldest child is many-one, the relation of father to child (in cases where there is more than one child) one-many, and the relation of father to eldest child one-one.

Both the analysis of mathematical induction and the definition of a one-one relation played a crucial role in Frege's logicist project, as we shall see in the next chapter. But we can conclude the present section by emphasizing the important point here: what Frege had shown was that they could be given in purely logical terms.⁴² Frege expressed it colourfully in his introductory remarks to Part III of the *Begriffsschrift*:

one sees [in these examples] how pure thought, regardless of any content given through the senses or even *a priori* through an intuition, is capable of bringing forth by itself, from the content which arises from its own nature, judgements which at first sight only seem possible on the basis of some intuition. This can be compared to condensation, by means of which air, which appears to a child's mind to be nothing, can be transformed into a visible fluid that forms drops. The propositions about series developed [in this Part] far surpass in generality all similar propositions which can be derived from any intuition of series. (BS, §23.)

The success of his initial condensations no doubt convinced Frege of the possibility of providing logical definitions of *all* arithmetical concepts and forms of reasoning. The next step was to provide definitions of the numbers themselves, and this was the task he undertook in his second book, *Die Grundlagen der Arithmetik*.

3.4 The Analyticity of Arithmetic

On the publication of the *Begriffsschrift*, and with five years of conscientious teaching behind him, Frege was promoted to *ausserordentlicher Professor* on the recommendation of Ernst Abbe, his mentor at Jena.⁴³ In his report on Frege's book, Abbe spoke of its 'very original cluster of ideas', but noted that 'it will probably be understood and appreciated by only a few'. Mathematics, Abbe suggested, 'will be affected, perhaps very considerably, but immediately only very little, by the inclination of the author and the content of the book'.⁴⁴ Abbe's remarks were prophetic. As we noted in chapter 2, the reviews of Frege's book were discouraging, his 'concept-script' being judged inferior to the Boolean logic of his leading contemporaries. This induced Frege to examine carefully the work of Boole, and to write several papers demonstrating the greater power of his own theory (see §2.2 above). This task occupied Frege in the three years that followed the publication of the *Begriffsschrift*, and the work analysing the concept of number, which had been heralded in the closing sentence of the 'Preface' to the *Begriffsschrift*, was consequently delayed.⁴⁵

The criticism of the *Begriffsschrift* did, however, have one positive effect: it made Frege aware of the need to explain his ideas and sketch his proposed reduction of arithmetic to logic informally, *before* embarking on the programme of providing rigorous proofs using his 'concept-script'. He read up on the work of philosophers who had written about mathematics, and realized the importance of locating his own views against the background of the traditional disputes. The result was *Die Grundlagen der Arithmetik*, published in 1884 and recognized now, though not at the time, as a philosophical masterpiece, containing a penetrating critique of rival conceptions of arithmetic and an original account of his own. In these final two sections I shall focus on his critique of rival conceptions, and leave an exposition of his positive project until the next chapter.

Against the mathematical background sketched in §3.1, the opening passage of Frege's *Grundlagen* can be quoted without further explanation:

After departing for a long time from Euclidean rigour, mathematics is now returning to it, and even striving to take it further. In arithmetic, simply as a result of the origin in India of many of its methods and concepts, reasoning has traditionally been less strict than in geometry, which had mainly been developed by the Greeks. This was only reinforced by the discovery of higher analysis; since considerable, almost insuperable difficulties stood in the way of a rigorous treatment of this subject, whilst at the same time there seemed little profit in the expenditure of effort in overcoming them. Later developments, however, have shown more and more clearly that in mathematics a mere moral conviction, based on many successful applications, is insufficient. A proof is now demanded of many things that previously counted as self-evident. It is only in this way that the limits to their validity have in many cases been determined. The concepts of function, continuity, limit and infinity have been shown to require sharper definition. Negative and irra-

tional numbers, which have long been accepted in science, have had to submit to a more exacting test of their legitimacy.

Thus everywhere efforts are being made to provide rigorous proofs, precise determinations of the limits of validity and, as a means to this, sharp definitions of concepts. (GL, §1.)

Eventually, Frege goes on, we realize that the concept of number itself requires definition, and that proofs are needed of even the simplest arithmetical propositions (§§ 2, 4).

The demand for rigour has as much a philosophical as a mathematical motivation. For in understanding how a particular truth is *justified*, we appreciate its logical status, that is, whether it is analytic or synthetic, *a priori* or *a posteriori*. I shall say much more about these distinctions in chapter 5, but let us simply note here the characterizations that Frege provides of these notions (§3):

- (AN) A truth is *analytic* if its proof depends only on general logical laws and definitions.
- (SN) A truth is *synthetic* if its proof cannot be given without relying on truths from a particular science.
- (PR) A truth is *a priori* if its proof can be given from completely general laws, which themselves neither need nor admit of proof.
- (PS) A truth is *a posteriori* if its proof cannot be given without appealing to facts, that is, to unprovable and non-general truths that contain assertions about particular objects.

These characterizations immediately rule out there being any *analytic a posteriori* truths (§12), since general logical laws and definitions are assumed to 'neither need nor admit of proof'. Analyticity implies *apriority*, in other words, but not vice versa.

There are thus three positions concerning the status of arithmetical propositions to consider. Arithmetic may be seen as a system of *analytic a priori* truths, as Leibniz can be regarded as advocating; a system of *synthetic a priori* truths, as Kant explicitly proposed; or a system of *synthetic a posteriori* truths, as Mill argued.⁴⁶ Frege endorses the Leibnizian position, though he raises some objections to Leibniz's own account. He agrees with Leibniz, for example, that numerical formulae are provable, that is, reducible via axioms and definitions to 'identities', but criticizes Leibniz's own proof that $2 + 2 = 4$ for missing out the associative law (§6). More importantly, whilst characterizing Leibniz as holding that arithmetical truths are both analytic and *a priori*, Frege recognizes that Leibniz believed that *all* truths were provable – contingent as well as necessary, threatening to collapse the distinction between necessary and contingent propositions that had been so usefully drawn (§15).⁴⁷ Frege applauds, however, Leibniz's fundamental insight: that every number can be defined in terms of its predecessor, which enables us to *conceive* very

large numbers even though we can have no *idea* (*Vorstellung*) of them.⁴⁸ 'Through such definitions we reduce the whole infinite set of numbers to the number one and increase by one, and every one of the infinitely many numerical formulae can be proved from a few general propositions.' (FA, §6.)

What, then, is wrong with the other two positions? Frege's criticism of Kant's position is directed against what he takes as the main motivation for construing arithmetical propositions as *synthetic* truths, namely, that since they are not, typically, immediately self-evident, appeal must be made to *intuition* in any apprehension of their truth. Frege agrees that complex numerical formulae, such as $135664 + 37863 = 173527$, are not self-evident, but argues that this shows, not that they are synthetic, but that they are *provable* (§5). Kant, though, talks not of proof but of *intuition* as the basis of any apprehension of truth. Kant's favourite example is $7 + 5 = 12$. No amount of analysis of the concept of the sum of 7 and 5, he argues, gets us to the concept of 12; rather, we must have an *intuition* of, say, fingers or points in order to work out that 5 units added to 7 units makes 12 units.⁴⁹ Of course, appealing to fingers involves *empirical* intuition, which, as Frege recognizes, runs counter to Kant's official view. But even allowing that there could be such a thing as *pure* intuition, it is hard to see how we could have intuitions of very large numbers: 'have we, in fact, an intuition of 135664 fingers or points at all? If we had, and if we had another of 37863 fingers and a third of 173527 fingers, then the correctness of our formula, if it were unprovable, would have to be evident right away, at least as applying to fingers; but it is not.' (FA, §5.)

Of course, there is no reason why Kant would deny that complex numerical formulae are provable, but he would presumably still insist that the apprehension of the truth of the simplest formulae depends upon intuition. Frege's response is to point out that 'it is awkward to make a fundamental distinction between small and large numbers, especially as it would scarcely be possible to draw any sharp boundary between them'. Why should we not treat even the simplest formulae as provable? (Ibid.)⁵⁰ As Leibniz showed, even $2 + 2 = 4$ is provable, so that, in Kant's own paradigm cases, there is no need to appeal to intuition. 'It is all too easy to appeal to inner intuition', Frege remarks, 'when other grounds cannot be found' (GL, §12).

As we have seen, Frege was adamant that the domain of arithmetic extends beyond the realm of the merely intuitable. In a letter written in 1882, after noting his departure from Kant, Frege goes on:

The field of geometry is the field of possible spatial intuition; arithmetic recognizes no such limitation. Everything is enumerable, not just what is juxtaposed in space, not just what is successive in time, not just external phenomena, but also inner mental processes and events and even concepts, which stand neither in temporal nor in spatial but only in logical relations to one another. The only barrier to enumerability is to be found in the

imperfection of concepts. Bald people for example cannot be enumerated as long as the concept of baldness is not defined so precisely that for any individual there can be no doubt whether he falls under it. Thus the area of the enumerable is as wide as that of conceptual thought, and a source of knowledge more restricted in scope, like spatial intuition or sense perception, would not suffice to guarantee the general validity of arithmetical propositions. (*PMC*, p. 100.)

Frege's Pythagorean argument here is worth spelling out. Genuine thought, according to Frege, requires sharply defined concepts, which implies a determinate number of objects falling under each concept, which shows that enumerability is part of the essence of conceptual thought and hence is a purely logical matter. Since each concept has a number associated with it, Frege's subsequent definition of number in terms of the *extensions* of concepts comes as a natural move. Frege's Pythagoreanism, then, can be seen as motivating his logicist project. In §14 of the *Grundlagen* he writes: 'The truths of arithmetic govern the realm of the numerable. This realm is the broadest; for to it belongs not only the actual, not only the intuitable, but everything thinkable. Should not the laws of number, then, stand in the most intimate connection with those of thought?'

Frege's critique of Kant's position applies *a fortiori* to Mill's position, since, in also regarding arithmetic as a body of synthetic propositions, Mill narrowed its domain even further, to the realm of the merely empirical. But, as Frege points out, we can number more than physical things. We can talk, for example, of four ideas or four concepts, or the four syllogistic figures (cf. §24). But in construing arithmetical propositions not only as synthetic, but also as *a posteriori*, Mill laid himself open to an additional barrage of criticism.

Frege again takes the case of large numbers to ridicule Mill's position. Mill accepts that we can provide definitions of natural numbers in terms of their predecessors, but remarks that 'they are definitions in the geometrical sense, not the logical; asserting not the meaning of a term only, but along with it an observed matter of fact' (*SL*, II vi 2, p. 257). But, responds Frege, 'what in the world can be the observed or physical fact, as Mill also calls it, that is asserted in the definition of the number 777864?' (§7.)⁵¹ Of course, as in the case of Kant, we can be more charitable here and interpret Mill as allowing Leibnizian definitions of large numbers, but as still insisting on the *empirical* basis of the simplest formulae. Mill writes:

we may call "Three is two and one" a definition of three; but the calculations which depend on that proposition do not follow from the definition itself, but from an arithmetical theorem presupposed in it, namely, that collections of objects exist, which while they impress the senses thus, $^{\circ}o^{\circ}$, may be separated into two parts, thus, $^{\circ}o^{\circ}$. This proposition being granted, we term all such parcels Threes, after which the enunciation of the above mentioned physical fact will serve also for a definition of the word Three. (*SL*, II vi 2, p. 257.)

In response to the assumed separability of the objects, Frege comments: 'It is just as well that not everything in the world is nailed down; for otherwise this separation could not be achieved, and $2 + 1$ would not be 3!' (*GL*, §7.) Even granting that collections of objects are rearrangeable, there remains the problem as to how such a definition applies in other cases, say, when we speak of a clock striking three, or of three sensations of taste, or of three methods of solving an equation: 'for in none of these cases is there a sense impression as of $^{\circ}o^{\circ}$ ' (ibid.).

Frege accepts that we may require experience to *learn* the truths of arithmetic, but argues that this does not make those truths *empirical* as the term 'empirical' is used in opposition to 'a priori' (§8). As Frege has already explained, the issue concerns the *justification* of arithmetical truths; and the task is to see if proofs can be found that depend solely on general laws (§3; cf. (PR) above). Whilst empirical elucidations may help us acquire knowledge of arithmetic, they are irrelevant to the question of its status. Mill always confuses, Frege remarks, the *applications* of an arithmetical proposition with the pure proposition itself. ' $5 + 2 = 7$ does not mean that if 2 unit volumes of liquid are poured into 5 unit volumes of liquid, 7 unit volumes of liquid are obtained, but the latter is an application of the proposition, which is only admissible if no change in volume occurs as a result, say, of a chemical reaction.' And '+', for example, does not refer to a process of heaping up, since it can be applied in quite different situations. (*GL*, §9.)

However, all these considerations still leave us with the underlying issue untouched. We can allow, and Kant and Mill can also allow, that all (non-primitive) numerical formulae can be proved, and that Leibnizian definitions can be given of all the natural numbers. But for this to show that arithmetical propositions are analytic, the analyticity of the primitive truths themselves must be established. According to (AN), a truth is analytic if its proof depends only on general logical laws and definitions, and it is plainly assumed that the general logical laws and definitions are themselves analytic.⁵² But the important question concerns the status of the primitive arithmetical truths. If these can be reduced to logical truths, then (with the assumption just made) the analyticity of arithmetic is assured; if they cannot, then the Kantian and Millian options remain live. The most that Part I of the *Grundlagen* establishes, then, is the conditional proposition: *if* the primitive arithmetical truths are analytic, then all arithmetical truths are analytic (since all other truths can be proved from the primitive ones).

Since Frege has already established that mathematical induction can be analysed in purely logical terms, the crucial question concerns the concept of number itself, and the definitions, in particular, of '0', '1' and 'increase by 1'. If logical definitions can also be given at this most primitive level (yielding e.g. ' $0 + 1 = 1$ '), then the logicist project is successfully launched. In Part II of the *Grundlagen* Frege criticizes the views of

previous writers on the concept of number, and in Part III he considers views on unity (*Einheit*) and one (*Eins*). As we shall see in the next chapter, by the end of Part III Frege's own view has emerged as a way of resolving the difficulties he has identified in the views of others. In the final section of this chapter, I shall simply outline Frege's criticisms.

3.5 Defective Conceptions of Number

In Part II of the *Grundlagen* Frege attacks two main positions regarding the nature of number: empiricism and psychologism. The empiricism he has in mind involves the conception of number as a property of external things. Once again, Mill is the main target, and Frege offers two reasons for not treating numbers in the same way as qualities, whether primary qualities such as solidity or secondary qualities such as colour.⁵³ Firstly, Frege argues, qualities belong to external things 'independently of any choice of ours', whereas what number we ascribe to something depends on our way of viewing it (§22). The *Iliad*, for example, can be thought of as one poem, or as 24 Books, or as some large number of verses. A pile of cards can be thought of as one pack, or as 52 cards, or as 40 points in bridge.⁵⁴ One pair of boots can be thought of as *two* boots (§25). Secondly, Frege argues, reiterating the point he has continually stressed, number is applicable over a far wider range than colour and solidity, and in particular, can be applied to what is *non-physical* (§24).

If the number that can be ascribed to something depends on our way of viewing it, then it is tempting to regard such ascriptions, and number itself, as purely subjective. Frege quotes Berkeley as someone who responded in this way to the perceived difficulty in what is essentially *empirical realism*.⁵⁵ Here is Berkeley's clearest statement of his position:

That number is entirely the creature of the mind, even though the other qualities be allowed to exist without, will be evident to whoever considers, that the same thing bears a different denomination of number, as the mind views it with different respects. Thus, the same extension is one or three or thirty-six, according as the mind considers it with reference to a yard, a foot, or an inch. Number is so visibly relative, and dependent on men's understanding, that it is strange to think how anyone should give it an absolute existence without the mind. We say one book, one page, one line; all these are equally units, though some contain several of the others. And in each instance it is plain, the unit relates to some particular combination of ideas arbitrarily put together by the mind. (*PHK*, §12.)⁵⁶

To talk of ideas, understood as mental phenomena, is to suggest a *psychologistic* conception of number; and Frege is no less critical of this alternative position:

For number is no whit more an object of psychology or a product of mental processes than, let us say, the North Sea is. The objectivity of the North Sea

is not affected by the fact that it is a matter of our arbitrary choice which part of all the water on the earth's surface we mark off and elect to call the "North Sea". This is no reason for deciding to investigate the North Sea by psychological methods. In the same way number, too, is something objective. If we say "The North Sea is 10,000 square miles in extent" then neither by "North Sea" nor by "10,000" do we refer to any state of or process in our minds: on the contrary, we assert something quite objective, which is independent of our ideas and everything of the sort. (*FA*, §26.)

Anti-psychologism is one of the most dominant features of Frege's philosophy throughout his life. The principle that 'there must be a sharp separation of the psychological from the logical, the subjective from the objective' is the first of the three 'fundamental principles' he singles out in his 'Introduction' to the *Grundlagen* (p. X).⁵⁷ For Frege, the realm of the psychological or subjective is the realm of ideas, understood as private mental entities, and his fundamental objection to psychologism is that it rules out communication and makes argument pointless. 'If the number two were an idea, then it would straightaway be mine only. Another's idea is already as such another idea. We would then have perhaps many millions of twos. One would have to say: my two, your two, one two, all twos.' (§27.) If someone were then to say that $2 \times 2 = 5$, all I could do is just register that their two has one property, and mine another. Arguing that they were wrong would be futile, since we would not be speaking of the same thing.⁵⁸ But there may not only be, in some cases, many more numbers than we would normally countenance; there may also be, in other cases, none where they would be expected. '10¹⁰', for example, might turn out to be an empty symbol, since there might be no being capable of having the appropriate idea (§27). In spelling out the implications of construing numbers as ideas, then, what Frege presents us with is a *reductio ad absurdum* of psychologism.

Succumbing to the temptation to treat the objective/subjective divide as corresponding to the physical/mental divide, it might be thought that, in rejecting both empiricism and psychologism, Frege has simply argued himself out of any coherent position at all. However, Frege distinguishes what is *objective* (*objectiv*) from what is *actual* (*wirklich*), the actual being the *handleable* (*handgreiflich*) or *spatial* (*räumlich*), such that what is actual (the world of material substance) is only part of what is objective. 'The axis of the earth is objective, so is the centre of mass of the solar system, but I should not call them actual in the way the earth itself is so.' (*FA*, §26.) We do, of course, speak of the equator as an *imaginary* line, but we do not mean by this that it is merely *imagined*: 'it is not a creature of thought, the product of a psychological process, but is only recognized or apprehended by thought' (*ibid.*). If it were *created* by thought, then we could not talk of it existing prior to its alleged creation; yet (just like gravity before Newton) the equator was around long before life on earth. What is objective, writes Frege, is 'what is subject to laws, what can be

conceived and judged, what is expressible in words', and this corresponds to 'what is independent of our sensation, intuition and imagination', but not, Frege goes on to say, 'what is independent of the reason, – for what are things independent of the reason? To answer that would be as much as to judge without judging, or to wash the fur without wetting it.' (Ibid.) This last remark needs explanation. For how can what is objective, which includes what is actual, depend on our reason? A physical object, for example, can exist independently of our sensing it; and we can talk coherently about a geometrical object such as a chiliagon even if no one can intuit or imagine it (i.e. construct some kind of mental picture of it). Yet neither of these is *inconceivable*, since we could not make true judgments about them without, in some sense, conceiving them; and this is all that Frege means by saying that they are 'dependent on reason'.⁵⁹

By the end of Part II of the *Grundlagen*, then, both empiricism and psychologism have been rejected as offering coherent accounts of arithmetic; but we are left only with the positive assertion that numbers are objective, though non-actual. In the final section of Part II, Frege mentions one further position, the set theory of number, which he understands as taking one of two forms, construing numbers either as sets of objects or as sets of units (§28). Neither view, Frege remarks, provides an account of the numbers 0 and 1; but the second view, he suggests, demands separate discussion, which he takes up in Part III. In fact, however, Frege's objections to both views are clarified in this Part; and it is useful to keep them both in mind.

Frege's critique can be presented in the form of a dilemma. Either the things of which numbers are sets are different (as they would be if they were different objects), or else they are identical. If they are different, then the same problem arises that Frege posed for psychologism: there will be as many twos, say, as there are different pairs of objects in the universe. The first view, then, is easily demolished by *reductio ad absurdum*. Of course, it is in the hope of avoiding this problem that the second view, construing numbers as sets of units, seems attractive. The idea is simple. Consider two sets of objects, both, say, with 5 members. Clearly, if numbers are *identified* with such sets, then we have two different fives. But imagine *abstracting away* from the particular characteristics of these objects, until all we are left with are objects that are identical with one another. These are the *units*, and since both sets will be composed of exactly the same units, the two sets will in fact be the same set, which can then be identified with the number 5. However, so the objection runs, if the units really are identical, then (so to speak) they merge into one, and the whole theory collapses.⁶⁰

The argument can also be run in reverse. Let us start with the fully analysed Leibnizian definition of '5':

$$(5) \quad 5 = 1 + 1 + 1 + 1 + 1.$$

According to the conception of numbers as sets of units, this is to be understood as follows:

$$(5\ddagger) \quad 5 = \{1, 1, 1, 1, 1\}.$$

But if '1' is the name of an object, and each occurrence of '1' refers to the same object, then all (5 \ddagger) amounts to is this:

$$(5\ddagger) \quad 5 = \{1\}.$$

If 'and' is used as it is in talking of constructing a set (agglomeration or collection) of things out of its members (parts), then, as Frege remarks, '1 and 1 and 1 is not 3 but 1, just as gold and gold and gold is never anything other than gold' (§38), or, we might add, just as $P \& P$ in propositional logic is equivalent to P .⁶¹ Clearly, then, '1' as it occurs in (5 \ddagger) is not to be understood as the name of a particular object, *the number one*; rather, each occurrence of '1' in (5 \ddagger) represents a *unit*, but a different one each time. Strictly speaking, then, (5 \ddagger) should be rewritten:

$$(5') \quad 5 = \{1', 1'', 1''', 1'''' , 1'''''\}.$$

But now we are back with the original problem, for this only defines *one* number five, since it differs from, say, the following definition:

$$(5\#) \quad 5 = \{1', 1'', 1''', 1'''' , 1'''''\}.$$

As Frege remarks, "The symbols 1', 1'', 1''' tell the tale of our embarrassment. We must have identity – hence the 1; but we must have difference – hence the strokes; only unfortunately, the latter undo the work of the former." (FA, §36.) Furthermore, as Frege shows, if we carry this idea back into standard arithmetic, complete nonsense ensues (§38). $3 - 2 = 1$, for example, might be written thus:

$$(1' + 1'' + 1''') - (1'' + 1''') = 1'.$$

But now consider the following (presumably equivalent) subtraction:

$$(1' + 1'' + 1''') - (1'''' + 1''''').$$

The result here seems no more likely to be 1' than any other unit, and it is hard to see how on earth to proceed.⁶² Numbers, then, cannot be construed as sets (agglomerations) of things. Whether these things are identical with one another or not, absurdity results; and calling them 'units' only serves to cover up the problem.

Frege himself offers a succinct summary of his critique:

We are faced, therefore, with the following difficulty:

If we try to produce the number by putting together different distinct objects, the result is an agglomeration in which the objects contained remain still in possession of precisely those properties which serve to distinguish them from one another; and that is not the number. But if we try to do it in

the other way, by putting together identicals, the result runs perpetually into one and we never reach a plurality.

If we use 1 to stand for each of the objects to be numbered, we make the mistake of assigning the same symbol to different things. But if we provide the 1 with differentiating strokes, it becomes unusable for arithmetic.

The word "unit" is admirably adapted to conceal this difficulty; and that is the real, though no doubt unconscious, reason why we prefer it to the words "object" and "thing". We start by calling the things to be numbered "units", without detracting from their diversity; then subsequently the concept of putting together (or collecting, or uniting, or annexing, or whatever we choose to call it) transforms itself into that of arithmetical addition, while the concept word "unit" changes unperceived into the proper name "one". And there we have our identity. ... The difficulty is so well hidden under the word "unit", that those who have any suspicion of its existence must surely be few at most. (FA, §39.)

What this suggests is that 'one' and 'unit' function in quite different ways. Whilst 'one' (i.e. '1') appears to be what Frege calls a 'proper name' ('Eigennamen'), 'unit' is a concept word. I consider the issues that this raises, as well as Frege's resolution of the difficulties he has identified in the various views he has examined, in the first section of the next chapter.

4. The Logician Project

Do not permit yourself to think you have known truth in philosophy, unless you can explain the leap in which we deduce that one, two, three, and four together make ten. (St Augustine, as quoted by Leibniz, *LS*, p. 37.)

In the last chapter I sought to clarify the background to Frege's logicist project, sketching some of the developments in the history of mathematics, revealing the motivations in his early work, and outlining his criticisms of traditional conceptions of arithmetic. In this chapter I focus on his positive programme, elucidating informally its main details. I start by explaining his construal of number statements as containing assertions about concepts, and his belief that numbers are objects (§4.1); and then provide an exposition of his argument in §§62-9 of the *Grundlagen*, which is not only fundamental to his logicism but also pivotal in the development of his philosophy, as we shall see in the next chapter. In §4.3 I clarify the moves in his logical reconstruction of the natural numbers; and in §4.4 I outline his account of other numbers, introducing his *magnum opus*, the *Grundgesetze der Arithmetik*, which presents formally what he had merely sketched informally in the *Grundlagen*. In the final section, in commenting on its plausibility, I compare Frege's own theory with the theories of Cantor and Dedekind, in particular. The developments in Frege's philosophy which the *Grundgesetze* incorporates and anticipates will be the subject of later chapters.

4.1 Numbers, Concepts and Objects

In the course of his critique of rival conceptions of arithmetic in the first half of the *Grundlagen* (§§5-44), Frege established a number of (mainly negative) preliminary points. From our exposition in the previous chapter, we can summarize these as follows:

(a) Certain traditional arguments for construing arithmetical propositions as *synthetic* truths (whether *a priori* or *a posteriori*) are flawed (§§5, 7-10, 12). The only other possible option, that they are *analytic a priori* truths, remains viable (§§3, 6, 11, 15).

(b) Numbers are not properties of external things, since ascriptions of number depend on the concepts under which the things are classified