SCIENCE WITHOUT NUMBERS

A Defense of Nominalism

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Preface

This monograph represents what I believe to be a new approach to the philosophy of mathematics. Most of the literature in the philosophy of mathematics takes the following three questions as central:

(a) How much of standard mathematics is true? For example, are conclusions arrived at using impredicative set theory true?
(b) What entities do we have to postulate to account for the truth of (this part of) mathematics?
(c) What sort of account can we give of our knowledge of these truths?

A fourth question is also sometimes discussed, though usually quite cursorily:

(d) What sort of account is possible of how mathematics is applied to the physical world?

Now, my view is that question (d) is the really fundamental one. And by focussing on the question of application, I was led to a surprising result: that to explain even very complex applications of mathematics to the physical world (for instance, the use of differential equations in the axiomatization of physics) it is not necessary to assume that the mathematics that is applied is true, it is necessary to assume little more than that mathematics is consistent. This conclusion is not based on any general instrumentalist stratagem: rather, it is based on a very special feature of mathematics that other disciplines do not share.

The fact that the application of mathematics doesn't require that the mathematics that is applied be true has important implications for the philosophy of mathematics. For what good argument is there for regarding standard mathematics as a body of truths? The fact that standard mathematics is logically derived from an apparently consistent body of axioms isn't enough; the question is, why regard the axioms as *truths*, rather than as fictions that for a variety of reasons mathematicians have become interested in? The only non-question-begging arguments I have ever heard for the view that mathematics is a body of truths all rest ultimately on the applicability of mathematics to the physical world; so if applicability to the physical world isn't a good argument either, then there is no reason to regard any part of mathematics as true. This is not of course to say that there is something wrong with mathematics; it's simply to say that mathematics isn't the sort of thing that can be appropriately evaluated in terms of truth and falsehood. Questions (a)–(c) are thus trivially answered: *no* part of mathematics is true (but you can use impredicative reasoning and other controversial reasoning all you like in mathematics as long as you're pretty sure it's consistent); consequently no entities have to be postulated to account for mathematical truth, and the problem of accounting for the knowledge of mathematical truths vanishes. (Of course, the problem of accounting for our knowledge of what mathematical conclusions follow from what mathematical premises still remains. But that is *logical* knowledge, not *mathematical* knowledge: it isn't knowledge of any special realm of mathematical entities.)*

The hardest part of showing that the application of mathematics doesn't require that the mathematics that is applied be true is to show that mathematical entities are theoretically dispensable in a way that theoretical entities in science are not: that is, that one can always re-axiomatize scientific theories so that there is no reference to or quantification over mathematical entities in the reaxiomatization (and one can do this in such a way that the resulting axiomatization is fairly simple and attractive). To show convincingly that such nominalistic reaxiomatizations of serious physical theories are possible requires a

* In these first two paragraphs I have used the term 'mathematics' a bit more narrowly than in the text: in these paragraphs, only sentences containing terms referring to mathematical entities or variables ranging over mathematical entities count as part of mathematics. (Compare note 1 of the text.)

rather detailed technical argument. In this monograph I have in fact given such an argument (in the case of one physical theory I judge to be fairly typical). But I have tried to make the main ideas of my approach accessible to those without the background or the patience to follow all of the technical details.

The motivation for this project did not come solely from considerations about the philosophy of mathematics or about ontology: certain ideas in the philosophy of science (such as the desirability of what I call 'intrinsic explanations' and the desirability of eliminating certain sorts of 'arbitrariness' or 'conventional choice' from our ultimate formulation of theories) also played a key role. These ideas from the philosophy of science are touched on in Chapter 5; they yield support, independent of ontological considerations, for the account of the application of mathematics being suggested here. I also discuss (mostly in Chapter 9 but to some extent also in Chapter 4) some issues about logic and about ontological commitment: in particular, the relativity of ontological commitment to the underlying logic, i.e. the fact that one can often reduce one's ontological commitments by expanding one's logic. This is a fact about ontological commitment that has not been sufficiently discussed by philosophers writing on ontological questions, and one of the issues I address myself to in the final chapter is under what circumstances if any it is reasonable to expand one's logic in order to reduce one's ontology.

I would like to thank the University of Southern California, the National Science Foundation and the Guggenheim Foundation for their generous support that provided me with the time needed for research and writing of this project. At a less material level, I would like to thank John Burgess and especially Scott Weinstein for helping me to get straight the relation between the consistency of mathematics and its conservativeness (cf. the Appendix to Chapter 1); and to Burgess, Tony Martin, and Yiannis Moschovakis for helpfully answering various questions that arose when I attempted to prove a false claim about the system N_0 that is discussed in Chapter 9. Several readers of an earlier draft made helpful comments that enabled me to

clarify and improve my arguement: among them I would like especially to mention Solomon Feferman, Michael Friedman, David Hills, Janet Levin, Colin McGinn, and Charles Parsons. Finally, I would like to express a general indebtedness to Hilary Putnam: in philosophy of mathematics as in much else, his work has deeply influenced the way I think about things, even where (as here) the conclusions we have reached are very different.

Here is a chapter-by-chapter description of what follows:

Preliminary Remarks. (a) States the doctrine to be advocated (and to be called 'nominalism'), namely the view that there are no mathematical entities; (b) sketches the most serious objection that has been made to this doctrine: roughly, that mathematical entities are indispensable to practical affairs and to science; (c) describes the strategy *most* nominalists have adopted for trying to get around this objection; and (d) describes an *alternative* strategy for overcoming the objection, which is the strategy to be employed in this book.

1 *Why the Utility of Mathematical Entities is Unlike the Utility of Theoretical Entities.* In this chapter I argue that it is legitimate to use mathematics to draw nominalistic conclusions (i.e. conclusions statable without reference to mathematical entities) from nominalistic premises, without assuming that the mathematics used in this way is true, but assuming little more than that it is consistent. More precisely, what one assumes about mathematics (and the relationship of this assumption to the assumption that mathematics is consistent is discussed in the Appendix to the chapter) is that mathematics is *conservative:* any inference from nominalistic premises to a nominalistic conclusion that can be made with the help of mathematics could be made (usually more long-windedly) without it. This is a fundamental difference between the use of mathematical entities and the use of the theoretical entities of science: no such conservativeness property holds for the latter. The utility of theoretical entities in science is due solely to their *theoretical indispensability:* without theoretical entities, no (sufficiently attractive) theory is possible. At first blush, it appears that mathematical

entities are theoretically indispensable too, for they seem to be needed in axiomatizing science; it appears, then, that the conservativeness of mathematics accounts for only part of its utility. In later chapters however I argue that mathematical entities are not theoretically indispensable, and that the entire utility of mathematics can be accounted for by its conservativeness, without assuming its truth.

2 *First Illustration of Why Mathematical Entities are Useful: Arithmetic* This chapter and the next provide elementary illustrations of the kind of application of mathematics that can be accounted for by the conservativeness of mathematics alone, without invoking the assumption that the mathematics being applied is true. This chapter concerns the application of the arithmetic of natural numbers.

3 *Second Illustration of Why Mathematical Entities are Useful: Geometry and Distance.* Here I show that the use of real numbers in geometry can be accounted for by the conservativeness of mathematics, without assuming the truth of the theory of real numbers. This illustration of the ideas of Chapter 1 will play a major role in ensuing chapters. To give a bit more detail: I discuss Hilbert's axiomatization of Euclidean geometry, which, since it doesn't involve real numbers, shows that real numbers are theoretically dispensable in geometry; than I discuss two theorems that Hilbert proved about his axiomatization of geometry, namely his representation and uniqueness theorems, and show how the representation theorem explains the utility of real numbers in geometric reasoning (without requiring that the theory of real numbers be true) while the uniqueness theorem establishes that the axiomatization without numbers has certain quite desirable properties.

4 *Nominalism and the Structure of Physical Space.* Here it is argued that the Hilbert theory of the previous chapter not only dispenses with real numbers, but is (or can be made with a little rewriting) a genuinely nominalistic theory of the structure of physical space. Arguing this involves a brief discussion of some questions in the philosophy of space and time, and an issue in the philosophy of logic that arises again in Chapter 9.

5 *My Strategy for Nominalizing Physics, and its Advantages.* Here I suggest that the Hilbert theory of geometry, and its representation and uniqueness theorems, provide a general model of how physical theories are to be nominalized. Several features of Hilbert's version of geometry are cited; it is argued that these features are highly advantageous ones, and a decision is made to require of an adequate nominalization of physics that it have analogous advantages. It is also pointed out that the other nominalistic approaches which were contrasted to my approach in the Preliminary Remarks do not lead to physical theories with these advantageous features.

6 *A Nominalistic Treatment of Newtonian Space-time.* This chapter extends the Hilbert treatment of space to space-time, emphasizing the advantages of the resulting theory over more usual approaches to space-time. (The key advantages of my approach, aside from its being nominalistic, are that it is more thoroughly 'intrinsic' and (closely related) that it avoids use of a certain kind of 'arbitrary choice' of scale, rest frame, coordinate system, etc.) This is the first of the chapters that have a fairly technical subject matter, but it is written in an informal enough way so that most readers should be able to get the main idea of the approach I am following and its advantages.

7 *A Nominalistic Treatment of Quantities, and a Preview of a Nominalistic Treatment of the Laws involving them.* Here I discuss very briefly how quantities like temperature are to be dealt with nominalistically. I also outline the strategy that is to be used in the next chapter for dealing nominalistically with laws involving these quantities, such as differential equations. This chapter, like the last, deals with technical material, but is informal enough so that most readers should get the general idea.

8 *Newtonian Gravitational Theory Nominalized.* This chapter is quite technical: it is a detailed sketch of how one particular theory is to be formulated nominalistically, and how the adequacy of this formulation is to be proved. I suspect that many readers will not be interested in going through all the details, but I recommend that they read at least Section A: This gives a relatively simple illustration of the same strategy of nominalization that is used in more complicated contexts later on in the chapter.

9 *Logic and Ontology.* There are two respects in which the treatment of physics in the foregoing chapters goes beyond first-order logic, and this final chapter discusses what morals are to be drawn from this. It is argued first that this extra logic does not violate nominalism; second, that use of this extra logic is preferable to use of set theoretic surrogates for the logic (which *would* violate nominalism); third, that use of this extra logic is probably dispensable anyway. The first two of these points involve issues about ontological commitment that are of interest independently of the theory being presented in this monograph.

Preliminary Remarks

Nominalism is the doctrine that there are no abstract entities. The term 'abstract entity' may not be entirely clear, but one thing that does seem clear is that such alleged entities as numbers, functions, and sets are abstract—that is, they would be abstract if they existed. In defending nominalism therefore I am denying that numbers, functions, sets, or any similar entities exist.

Since I deny that numbers, functions, sets, etc. exist, I deny that it is legitimate to use terms that purport to refer to such entities, or variables that purport to range over such entities, in our ultimate account of what the world is really like.

This appears to raise a problem: for our ultimate account of what the world is really like must surely include a physical theory; and in developing physical theories one needs to use mathematics; and mathematics is full of such references to and quantifications over numbers, functions, sets, and the like. It would appear then that nominalism is not a position that can reasonably be maintained.

There are a number of *prima facie* possible ways to try to resolve this problem. The way that has proved most popular among nominalistically inclined philosophers is to try to *reinterpret* mathematics—reinterpret it so that its terms and quantifiers don't make reference to abstract entities (numbers, functions, etc.) but only to entities of other sorts, say physical objects, or linguistic expressions, or mental constructions.

My approach is different: I do not propose to reinterpret any part of classical mathematics; instead, I propose to show that the mathematics needed for application to the physical world does not include anything which even *prima facie* contains references to (or quantifications over) abstract entities like numbers, functions, or sets. Towards that part of mathematics which does contain references to (or quantifications over) abstract entities—and this includes virtually all of conventional mathematics—I adopt a fictionalist attitude: that is, I see no reason to regard this part of mathematics as *true.*[1]

Most recent philosophers have been hostile to fictionalist interpretations of mathematics, and for good reason. If one *just* advocates fictionalism about a portion of mathematics, without showing how that part of mathematics is dispensable in applications, then one is engaging in intellectual doublethink: one is merely taking back in one's philosophical moments what one asserts in doing science, without proposing an alternative formulation of science that accords with one's philosophy. This (Quinean) objection to fictionalism about mathematics can only be undercut by showing that there is an alternative formulation of science that does not require the use of any part of mathematics that refers to or quantifies over abstract entities. I believe that such a formulation is possible; consequently, without intellectual doublethink, I can deny that there are abstract entities.

The task of showing that one can reformulate all of science so that it does not refer to or quantify over abstract entities is obviously a very large one; my aim in this monograph is only to illustrate what I believe to be a new strategy toward realizing this goal, and to make both the goal and the strategy look attractive and promising. My attempt to make the strategy look promising ultimately takes the following form: I show, in Chapter 8, how in the context of certain physical theories (field theories in flat space-time[2]) one can develop an analogue of the calculus of several real variables that does not quantify over real numbers or functions or any such thing. Although I do not develop this analogue of calculus completely (e.g. I do not discuss integration), I do sketch enough of it to show how a nominalistic version of the Newtonian theory of gravitation could be given. This nominalistic version of gravitational theory has all the nominalistically-statable consequences of the usual platonistic (i.e. non-nominalistical) versions

of the theory. Moreover, I believe that the nominalistic reformulation is mathematically attractive, and that there are considerations other than ontological ones that favour it over the usual platonistic formulations.

I must admit that the formulation of gravitational theory which I arrive at will not satisfy every nominalist: I use several devices which some nominalists would question. In particular, nominalists with any finitist or operationalist tendencies will not like the way I formulate physical theories, for my formulations will be no more finitist or operationalist than the usual platonistic formulations of these theories are. To illustrate the distinction I have in mind between nominalist concerns on the one hand and finitist or operationalist concerns on the other, consider an example. Someone might object to asserting that between any two points of a light ray (or an electron, if electrons have non-zero diameter) there is a third point, on the ground that this commits one to infinitely many points on the light ray (or the electron) or on the ground that it is not in any very direct sense checkable. But these grounds for objecting to the assertion are not nominalistic grounds as I am using the term 'nominalist', for they arise not from the nature of the postulated entities (viz. the parts of the light ray or of the electron) but from the structural assumptions involving them (viz. that there are infinitely many of them in a finite stretch). I am not very impressed with finitist or operationalist worries, and consequently I make no apologies for making some fairly strong structural assumptions about the basic entities of gravitational physics in what follows. It is not that I have no sympathy whatever for the program of reducing the structural assumptions made about the entities postulated in physical theories—if this can be done, it is interesting. But as far as I aware, it has not been successfully done even in platonistic formulations of physics: that is, no platonistic physics is available which uses a mathematical system less rich than the real numbers to represent the positions of the parts of a light ray or of an electron. Consequently, although I will make it a point not to make any structural assumptions about entities beyond the structural assumptions made in the usual platonistic theories about these entities, I will also feel no compulsion to reduce my structural assumptions below the platonistic level.[3] The reduction of structural assumptions is simply not my concern.

Although I feel no apologies are in order for my use of structural assumptions that would offend the finitist or operationalist, there is another device I have used which I do feel slightly apologetic about. But I try to argue in the final chapter that it is less objectionable than it might at first seem, and that it is probably eliminable anyway.

I would like to make clear at the outset that nothing in this monograph purports to be a positive argument for nominalism. My goal rather is to try to counter the most compelling arguments that have been offered against the nominalist position. It seems to me that the only non-question-begging arguments against the sort of nominalism sketched here (that is, the only non-question-begging arguments *for* the view that mathematics consists of *truths*) are all based on the applicability of mathematics to the physical world. Notice that I do not say that the only way to argue that *a given mathematical axiom* is true is on the basis of *its* application to the physical world: that would be incorrect. For instance, if one grants that the elementary axioms of set theory are true, one can with at least some plausibility argue for the truth of the axiom of inaccessible cardinals on the grounds that this axiom accords with the general conception of sets that underlies the more elementary axioms. More generally, if we assume that the concept of truth has non-trivial application in at least one part of pure mathematics (or to be more precise, if we assume that there is at least one body of pure mathematical assertions that includes existential claims and that is true), then we are assuming that there are mathematical entities. From this we can conclude that there must be some body of facts about these entities, and that not all facts about these entities are likely to be relevant to known applications to the physical world; it is then plausible to argue that considerations other than application to the physical world, for example, considerations of simplicity and coherence within mathematics, are grounds for accepting some proposed mathematical axioms as true and rejecting others as false. This is

all fine; but it is of relevance only *after* one grants the assumption that for *some* part of mathematics the concept of truth has non-trivial application, and this is an assumption that the nominalist will not grant.

There can be no doubt that the axioms of, say, real numbers are important, or that they are non-arbitrary; and an explanation of their non-arbitrariness, based on their applicability to the physical world but compatible with nominalism, will be given in Chapters 1–3. The present point is simply that from the importance and non-arbitrariness of these axioms, it doesn't obviously follow that these axioms are true, i.e. it doesn't obviously follow that there are mathematical entities that these axioms correctly describe. The existence of such entities may in the end be a reasonable conclusion to draw from the importance and non-arbitrariness of the axioms, but this needs an argument. When the debate is pushed to this level, I believe it becomes clear that there is one and only one serious argument for the existence of mathematical entities, and that is the Quinean argument that we need to postulate such entities in order to carry out ordinary inferences about the physical world and in order to do science.[4] Consequently it seems to me that if I can undercut this argument for the existence of mathematical entities, then the position that there are such entities will look like unjustifiable dogma.

The fact that what I am trying to do is not to provide a positive argument for nominalism but to undercut the only available argument for platonism must be borne in mind in considering an important methodological issue. Although in this monograph I will be espousing nominalism, I am going to be using platonistic methods of argument: I will for instance be proving *platonistically*, not nominalistically, that a certain nominalistic theory of gravitation has all of the nominalistically-statable consequences that the usual platonistic formulation of the Newtonian theory of gravitation has. It might be thought that there was something wrong about using platonistic methods of proof in an argument for nominalism. But there is really little difficulty here: if I am successful in proving *platonistically* that abstract entities are not needed for ordinary inferences about the physical world or for science, then anyone who wants to *argue* for platonism will be unable to rely on the Quinean argument that the existence of abstract entities is an indispensable assumption. The monograph shows that any such argument would be inconsistent with the platonistic position that is being argued for. The would-be platonist, then, will be forced into either accepting abstract objects without argument or else relying on other arguments for platonism, arguments which in my opinion are quite unpersuasive. The upshot then (if I am right in my negative appraisal of alternative arguments for platonism) is that platonism is left in an unstable position: it entails its own unjustifiability.[5]

It may be of course that my negative appraisal of alternative arguments for platonism is wrong. Interestingly enough, the platonist who bases his case for platonism on some such alternative argument may even find what I have to say welcome; for independently of nominalistic considerations, I believe that what I do here gives an attractive account of how mathematics is applied to the physical world. This is I think in sharp contrast to many other nominalistic doctrines, e.g. doctrines which reinterpret mathematical statements as statements about linguistic entities or about mental constructions. Such nominalistic doctrines do nothing toward illuminating the way in which mathematics is applied to the physical world. (I will return to this point in Chapter 5.)

1

Why the Utility of Mathematical Entities is Unlike the Utility of Theoretical Entities

No one can sensibly deny that the invocation of mathematical entities in some contexts is useful. The question arises as to whether the utility of mathematical existence-assertions gives us any grounds for believing that such existence-assertions are true. I claim that in answering this question one has to distinguish two different ways in which mathematical existence-assertions might be useful; I grant that if such assertions are useful in a certain respect, then that would indeed be evidence that they are true; but the most obvious respect in which mathematical existence assertions are useful is, I claim, quite a different one, and I will argue that the utility of such assertions in this respect gives no grounds whatever for believing the assertions to be true.

To be more explicit, I will argue that the utility of mathematical entities is structurally disanalogous to the utility of theoretical entities in physics. The utility of theoretical entities lies in two facts:

(a) they play a role in powerful theories from which we can deduce a wide range of phenomena; and

(b) no alternative theories are known or seem at all likely which explain these phenomena without similar entities.

[The unsympathetic reader may dispute (b): if any body of sentences counts as a 'theory' and any deduction from such a 'theory' counts as an explanation, then there clearly are alternatives to the usual theories of subatomic particles: e.g., take as your 'theory' the set T* all of the consequences of T that don't contain reference to subatomic particles (where T is one of the usual theories that does contain reference to subatomic particles); or if you want a recursively axiomatized 'theory', let T** be the Craigian reaxiomatization of the theory T* just described. Since I don't know any formal conditions to impose which would rule out such bizarre trickery, let me simply say that by 'theory' I mean *reasonably attractive theory;* 'theories' like T* and T** are obviously uninteresting, since they do nothing whatever toward explaining the phenomena in question in terms of a small number of basic principles.] The upshot of (a) and (b) is that subatomic particles are *theoretically indispensable;* and I believe that that is as good an argument for their existence as we need. Now, later on in the monograph I will argue that mathematical entities are not theoretically indispensable: although they do play a role in the powerful theories of modern physics, we can give attractive reformulations of such theories in which mathematical entities play no role. If this is right, then we can safely adhere to a fictionalist view of mathematics, for adhering to such a view will not involve depriving ourselves of a theory that explains physical phenomena and which we can regard as literally true.

But the task of arguing for the theoretical dispensability of mathematical entities is a matter for later. What I want to do now is to give an account, *consistent with* the theoretical dispensability of mathematical entities, of why it is useful to make mathematical existence-assertions in certain contexts.

The explanation of why mathematical entities are useful involves a feature of mathematics that is not shared by physical theories that

postulate unobservables. To put it a bit vaguely for the moment: if you take any body of nominalistically stated assertions N, and supplement it with a mathematical theory S, you don't get any nominalistically-statable conclusions that you wouldn't get from N alone. The analog for theories postulating subatomic particles is of course not true: if T is a theory that involves subatomic particles and is at all interesting, then there are going to be lots of cases of bodies P of wholly macroscopic assertions which in conjunction with T yield macroscopic conclusions that they don't yield in absence of T; if this were not so, theories about subatomic particles could never be tested.

I'll state these claims more precisely in a moment, but first I should say that the claim about mathematics would be almost totally trivial if mathematics consisted only of theories like number theory or *pure* set theory, i.e. set theory in which no allowance is made for sets with members that are not themselves sets. But these theories are by themselves of no interest from the point of view of applied mathematics, for there is no way to apply them to the physical world. That is, there is no way in which they are even *prima facie* helpful in enabling us to deduce nominalistically-statable consequences from nominalistically-statable premises. In order to be able to apply any postulated abstract entities to the physical world, we need *impure* abstract entities, e.g. functions that map physical objects into pure abstract entities. Such impure abstract entities serve as a bridge between the pure abstract entities and the physical objects; without the bridge, the pure objects would be idle. Consequently, if we regard functions as sets of a certain sort, then the mathematical theories we should be considering must include at least a minimal amount of set theory with urelements (a urelement being a non-set which can be the member of sets). In fact, in order to be sufficiently powerful for most purposes, the mathematical theory must differ from pure set theory not only in allowing for the possibility of urelements, it must also allow for non-mathematical vocabulary to appear in the comprehension axioms (i.e. in the instances of the axiom schema of separation or of replacement). So the 'bridge laws' must include laws that involve the mathematical vocabulary and the physical vocabulary together.

Something rather analogous is true of the theory of subatomic particles. One can artificially formulate such a theory so that none of the non-logical[6] vocabulary that is applied to observable physical objects is applied to the subatomic particles; in general it seems to me pointless to formulate physical theories in this way, but to press the analogy with the mathematical case as far as it will go, let us suppose it done. If it is done, and if we suppose that T is a physical theory stated entirely in this vocabulary, then of course, it *will* be the case that if we add T to a bunch of macroscopic assertions P, we will be able to derive no results about observables that weren't derivable already. But that is for a wholly uninteresting reason: it is because the theory T by itself is not even *prima facie* helpful in deducing claims about observables from other claims about observables. In order to make it even *prima facie* helpful, we have to add 'bridge laws', laws which connect up the entities and/or the vocabulary of the (artificially formulated) physical theory with observables and the properties by which we describe them. So far, then, like the mathematical case. *But there is a fundamental difference between the two cases, and that difference lies in the nature of the bridge laws.* In the case of subatomic particles, the theory T, interpreted now so as to include the bridge laws (and perhaps also some assumptions about initial conditions), can be applied to bodies of premises about observables in such a way as to yield genuinely new claims about observables, claims that would not be derivable without T. But in the mathematical case the situation is very different: here, if we take a mathematical theory that includes bridge laws (i.e. includes assertions of the existence of functions from physical objects into 'pure' abstract objects, including perhaps assertions obtained via a comprehension principle that uses mathematical and physical vocabulary in the same breath), then that mathematics is applicable to the world, i.e. it is useful in enabling us to draw nominalistically-statable conclusions from nominalistically-statable premises; *but here, unlike in the case of physics, the conclusions we arrive at by these means are not genuinely*

new, they are already derivable in a more long-winded fashion from the premises, without recourse to the mathematical entities.

This claim, unlike the one I will make later about the theoretical dispensability of mathematical entities, is pretty much of an incontrovertible fact, but one very much worth emphasizing. So first let me state the point more precisely than I have done.

A first stab at putting the point precisely would be to say that for any mathematical theory S and any body of nominalistic assertions N, N + S is a conservative extension of N. However, this formulation isn't quite right, and it is worth taking the trouble to put the point accurately. The problem with this formulation is that since N is a nominalistic theory, it may say things that *rule out* the existence of abstract entities, and so N + S may well be inconsistent. But it is clear how to deal with this: first, introduce a 1-place predicate 'M(x)', meaning intuitively 'x is a mathematical entity'; second, for any nominalistically-stated assertion A, let A* be the assertion that results by restricting each quantifier of A with the formula 'not $M(x_i)$' (for the appropriate variable 'x_i');[7] and third, for any nominalistically-stated body of assertions N, let N* consist of all assertions A* for A in N. N* is then an 'agnostic' version of N: for instance, if N says that all objects obey Newton's laws, then N* says that all *non-mathematical* objects obey Newton's laws, but it allows for the possibility that there are mathematical objects that don't. (Actually N* is in one respect *too* agnostic: in ordinary logic we assume for convenience that there is at least one thing in the universe, and in the context of a theory like N this means that there is at least one non-mathematical thing. So it is really N* + '$\exists x - M(x)$' that gives the agnostic content of N). Whether a similar point needs to be made for our mathematical theory S depends on what we take S to be. If S is simply set theory with urelements, no restriction on the variables is needed, since the theory already purports to be about non-sets as well as sets: we merely need to connect up the notion of set that occurs in it with our predicate 'M', by adding the axiom '$\forall x(\text{Set}(x) \rightarrow M(x))$'. If in addition the mathematical theory includes portions like number theory, considered as independent disciplines unreduced to set theory, then we must restrict all variables in them by a new predicate 'Number', and add the axioms '∀x(Number (x) → M(x))' and '∃x(Number (x))'. Presumably, however, everyone agrees that mathematical theories really ought to be written in this way (that is, presumably no one believes that all entities are mathematical), so I will not introduce a special notation for the modified version of S, I'll assume that S is written in this form from the start. (The analogous assumption for N would be inappropriate: the nominalist wants to assert not N*, but the stronger claim N.)

Having dealt with these tedious points, I can now state accurately the claim made at the end of the next to last paragraph.

> *Principle C* (for 'conservative'): Let A be any nominalistically statable[8] assertion, and N any body of such assertions; and let S be any mathematical theory. Then A* isn't a consequence of N* + S + '∃x – M(x)' unless A is a consequence of N.

Why should we believe this principle? Well, it follows[9] from a slightly stronger principle that is perhaps a bit more obvious:

> *Principle C'*: Let A be any nominalistically-statable assertion, and N any body of such assertions. Then A* isn't a consequence of N* + S unless it is a consequence of N* alone.

This in turn is equivalent (assuming the underlying logic to be compact) to something still more obvious-sounding:

> *Principle C''* Let A be any nominalistically-statable assertion. Then A* isn't a consequence of S unless it is logically true.

Now I take it to be perfectly obvious that our mathematical theories do satisfy Principle C''. After all, these theories are commonly regarded as being 'true in all possible worlds' and as '*a priori* true'; and though these characterizations of mathematics may be contested, it is hard to see how any knowledgeable person could regard our mathematical theories in these ways if those theories implied results about concrete entities alone that were not logically true. The same argument can

be used to directly motivate Principle C', thereby obviating the need of the compactness assumption: if mathematics together with a body N* of nominalistic assertions implied an assertion A* which wasn't a logical consequence of N* alone, then the truth of the mathematical theory would hinge on the logically consistent body of assertions N* + – A* not being true. But it would seem that it must be possible, and/or not *a priori* false, that such a consistent body of assertions about concrete objects alone is true; if so, then the failure of Principle C would show that mathematics couldn't be 'true in all possible worlds' and/or '*a priori* true'. The fact that so many people think it does have these characteristics seems like some evidence that it does indeed satisfy Principle C' and therefore Principle C.

This argument isn't conclusive: standard mathematics *might* turn out not to be conservative (i.e. not to satisfy Principle C), for it might conceivably turn out to be inconsistent, and if it is inconsistent it certainly isn't conservative. We would however regard a proof that standard mathematics was inconsistent as extremely surprising, and as showing that standard mathematics needed revision. Equally, it would be extremely surprising if it were to be discovered that standard mathematics implied that there are at least 10^6 non-mathematical objects in the universe, or that the Paris Commune was defeated; and were such a discovery to be made, all but the most unregenerate rationalist would take this as showing that standard mathematics needed revision. *Good* mathematics *is* conservative; a discovery that accepted mathematics isn't conservative would be a discovery that it isn't good.

Indeed, as some of the mathematical arguments in the Appendix to this chapter show, the gap between the claim of consistency and the full claim of conservativeness is, in the case of mathematics, a very tiny one. In fact, for *pure* set theory, or for set theory that allows for impure sets but doesn't allow empirical vocabulary to appear in the comprehension axioms, the conservativeness of the theory follows from its consistency alone. For full set theory this is not quite true; but a large part of the content of the conservativeness claim for full set theory (probably the only part of the content that is important in application) follows from the consistency of set theory alone (and still more of the content follows from slightly stronger assumptions, like that full set theory is ω-consistent). These claims are demonstrated in the Appendix to this chapter. In any case, I think that the two previous paragraphs show that the same sort of quasi-inductive grounds we have for believing in the consistency of mathematics extend to its conservativeness as well. As we saw earlier, this means that there is a marked disanalogy between mathematical theories and physical theories about unobservable entities: physical theories about unobservables are certainly not conservative, they give rise to genuinely new conclusions about observables.

What the facts about mathematics I have been emphasizing here show is that even someone who doesn't believe in mathematical entities is free to use mathematical existence-assertions in a certain limited context: he can use them freely in deducing nominalistically-stated consequences from nominalistically-stated premises. And he can do this not because he thinks those intervening premises are true, but because he knows that they preserve truth among nominalistically-stated claims.[10]

This point is not of course intended to license the use of mathematical existence assertions in axiom systems for the particular sciences: *such* a use of mathematics remains, for the nominalist, illegitimate. (Or more accurately, a nominalist should treat such a use of mathematics as a temporary expedient that we indulge in when we don't know how to axiomatize the science properly, and that we ought to try to eliminate.) The point I am making, however, does have the consequence that *once such a nominalistic axiom system is available*, the nominalist is free to use any mathematics he likes for deducing consequences, as long as the mathematics he uses satisfies Principle C.

So if we ignore for the moment the role of mathematics in axiomatizing the sciences, then it looks as if the satisfaction of Principle C is the really essential property of mathematical theories. The fact that mathematical theories have this property is doubtless one motivation for the platonist's assertion that such theories are 'true in all possible worlds'. It does not appear to me, however, that the satisfaction of Principle C provides reason for regarding a theory as true at all (even in the actual world). Certainly such speculations, typical of extreme platonism, as to for instance whether the continuum hypothesis is 'really true', seem to lose their point once one recognizes conservativeness as the essential requirement of mathematical theories: for the usual Gödel and Cohen relative consistency proofs about set theory plus the continuum hypothesis and set theory plus its denial are easily modified into relative *conservativeness* proofs. In other words, assuming that standard set theory satisfies Principle C, so does standard set theory plus the continuum hypothesis and standard set theory plus its denial; so it follows that *either theory could be used without harm in deducing consequences about concrete entities from nominalistic theories*. The same point made about the continuum hypothesis holds as well for less *recherché* mathematical assertions. Even standard axioms of number theory can be modified without endangering Principle C; similarly for standard axioms of analysis. What makes the mathematical theories we accept better than these alternatives to them is not that they are true and the modifications not true, but rather that they are more *useful*: they are more of an aid to us in drawing consequences from those nominalistic theories that we are interested in. If the world were different, we would be interested in different nominalistic theories, and in that case some of the alternatives to some of our favorite mathematical theories might be of more use than the theories we now accept.[11] Thus mathematics is in a sense empirical, but only in the rather Pickwickian sense that is an empirical question as to which mathematical theory is useful. It is in an equally Pickwickian sense, however, that mathematical axioms are *a priori*: they are not *a priori* true, for they are not true at all.

The view put forward here has considerable resemblance to the logical positivist view of mathematics. One difference that is probably mostly verbal is that the positivists usually described pure mathematics as analytically true, whereas I have described it as not true at all; this difference is probably mostly verbal, given their construal of 'analytic' as 'lacking factual content'. A much more fundamental difference is that what worried the positivists about mathematics was not so much its postulation of entities as its apparently non-empirical character, and this was a problem not only for mathematics, but for logic as well. Hence they regarded *logic* as analytic or contentless in the same sense that *mathematics* was. I believe that this prevented them from giving any clear explanation of what the 'contentlessness' of mathematics (or of that part of mathematics that quantifies over abstract entities) consists in. The idea of calling a logical or mathematical assertion 'contentless' was supposed to be that a conclusion arrived at by a logical or mathematical argument was in some sense 'implicitly contained in' the premises: in this way, the conclusion of such an argument was 'not genuinely new'. Unfortunately, no clear explanation of the idea that the conclusion was 'implicitly contained in' the premises was ever given, and I do not believe that any clear explanation is possible. What I have tried to do in this chapter is to show how by giving up (or saving for separate explication) the claim that *logic* (and that part of math that *doesn't* make reference to abstract entities) doesn't yield genuinely new conclusions, we can give a clear and precise sense to the idea that *mathematics* doesn't yield genuinely new conclusions: more precisely, we can show that the part of math that does make reference to mathematical entities can be applied without yielding any genuinely new conclusions about non-mathematical entities.

APPENDIX: *On Conservativeness*

It may be illuminating to give two mathematical arguments for the conservativeness of mathematics. The first argument proves, from a set-theoretic perspective (more specifically, from the perspective of ordinary set theory plus the axiom of inaccessible cardinals) that ordinary set theory (and hence standard mathematics, which is

reducible to ordinary set theory) is definitely conservative. The second argument is a purely proof-theoretic one: it establishes a slightly restricted form of the conservativeness claim on the basis merely of the assumption that standard set theory is consistent. This is illuminating in showing that the assumption of the conservativeness of set theory is much 'closer to' the assumption that set theory is consistent than to the assumption that it is true.

As a preliminary, let's introduce some notation. Let ZF be standard Zermelo–Fraenkel set theory (including the axiom of choice); let restricted ZFU be ZF modified to allow for the existence of urelements, but not allowing for any non-set-theoretic vocabulary to appear in the comprehension axioms (for definiteness, we may stipulate that it contains as an axiom that there is a set of all non-sets); and if V is a class of expressions, let ZFU_V be restricted ZFU together with any instances of the comprehension schemas in which the vocabulary in V as well as the set-theoretic vocabulary is allowed to appear. What I earlier called 'full set theory' isn't really a single theory: rather, to 'apply full set theory' in the context of a theory T is to apply $ZFU_{V(T)}$, where V(T) is the vocabulary of T. Consequently, what we want to prove is that for any theory T, $ZFU_{V(T)}$ applies conservatively to T. That is, we want to prove

(C_0) If T is any consistent body of assertions, then $ZFU_{V(T)} + T^*$ is also consistent.

(The T here is the N + −A of Principle C'). This in fact will suffice for proving the conservativeness of $ZFU_{V(T)} + S$, for any mathematical theory S: for standard mathematical theories are embeddable in ZF.

So much for preliminaries. How then do we prove that (C_0) holds? The obvious set-theoretic line of proof is this:

Suppose T is consistent; then it has a model M of accessible cardinality, say with domain D. Pick any entity e not in D. (e is to be thought of as the empty set.) Let D_0 be $D \cup \{e\}$; Let D_1 consist of all non-empty subsets of D_0; let D_2 consist of all non-empty subsets of $D_0 \cup D_1$; and so on. Let D_ω be $D_0 \cup D_1 \cup D_2 \cup \ldots$; let $D_{\omega+1}$ consist of all non-empty subsets of D_ω; and so on. Continuing in this way until you reach an inaccessible cardinal, you get—if certain initial precautions[12] are taken on the choice of D and e—a model of $ZFU_{V(T)} + T^*$. (It is a model of $ZFU_{V(T)} + T^*$ rather than merely of $ZFU + T^*$ because at each stage you've added *every* set of things available at previous stages.) So $ZFU_{V(T)} + T^*$ is consistent. Q.E.D.

Now let us turn to the proof-theoretic line of argument for conservativeness; the point of doing this is to make clear how narrow the gap between the consistency of mathematics and its conservativeness is. Indeed, in the case of mathematical theories which don't allow for impure abstract entities (e.g. number theory by itself, or ZF), consistency implies conservativeness: this is an obvious consequence of the Robinson joint consistency theorem.[13] The same result holds also in the more interesting case of restricted ZFU: here one needs, in addition to the Robinson theorem, the well-known fact that if ZFU is consistent then one can't prove any result about how many nonsets there are.[14] But in the really interesting case of full ZFU, this whole line of argument via the joint consistency theorem is blocked by the fact that the empirical vocabulary that appears in the theory T also appears in the set-theoretic axioms.

The simplest thing to do in this case is to mimic proof-theoretically the set-theoretic argument given two paragraphs back: doing so, it becomes an argument that under certain conditions $ZFU_{V(T)} + T^*$ is interpretable within $ZFU_{V(T)}$, and in fact within ZF. (We don't need the inaccessible cardinal assumption anymore.) If the 'certain conditions' were merely that T is consistent, then we'd know that (C_0) holds as long as ZF is consistent, and this is what we wanted. Unfortunately however we need the stronger assumption that T is *provably* consistent within ZF; that is, the best we can show is that if ZF is consistent, the following holds:[15]

(C_1) If T is any body of assertions *whose consistency is provable in ZF*, then $ZFU_{V(T)} + T^*$ is consistent.

This is a restricted version of conservativeness: it says that full set

theory applies conservatively *to theories which are modellable in ZF*. In actual applications this is probably as much of the conservativeness claim as we ever need. For instance, later on in the book we will want to know that mathematics applies conservatively to a nominalistic version of Newtonian gravitation theory, N_0. But it is completely obvious that if N_0 is consistent then it is modellable in ZF (and the same would presumably be true for other nominalized physical theories); so the conservativeness result we actually need follows merely from the consistency of ZF.

Scott Weinstein (besides clearing up a number of confusions I had gotten into concerning the issues of the last paragraph) pointed out to me that if you strengthen the consistency assumption about ZF slightly, to ω-consistency (or even something a bit weaker than that known as 1-consistency), you can strengthen (C_1) in an attractive way: you can then prove

(C_2) If T is any *consistent and recursively enumerable* body of assertions, then $ZFU_{V(T)} + T^*$ is consistent.[16]

It is all the more obvious that *this* would be sufficient for practical applications.

Philosophers discussing set theory tend to discuss two of its properties: its consistency, and its (alleged) truth. The argument of this monograph is that the latter is completely irrelevant, and that the former is perhaps a bit too weak—it is too weak unless one is satisfied with (C_1) instead of the full (C_0). [Of course, for the kind of set theory philosophers tend to discuss—*pure* set theory, i.e. ZF—there is no difference at all between consistency and conservativeness (or rather, though they differ conceptually, they are provably equivalent). But pure set theory isn't what is of interest, since as remarked before it can never be applied to the physical world, so this is not much of a justification for ignoring conservativeness.] But though we perhaps need to assume a bit more than consistency, we don't need to assume all that much more; and in any case it seems pretty obvious that the stronger property of conservativeness does in fact obtain.

2

First Illustration of Why Mathematical Entities are Useful: Arithmetic

I have explained why it is *legitimate* for a nominalist to use mathematics in making inferences between nominalistically-stated sentences; but I haven't said anything about why or in what way it is *useful* for him to do so. It is important to have a rather vivid understanding of the way that mathematics is useful in such contexts if one is to grasp my strategy for nominalizing physical theories, and so I will devote both this chapter and the next to the matter.

Suppose N is a body of nominalistically-stated premises; in the case that will be of primary interest, N will consist of the axioms of a nominalistic formulation of some scientific theory. I think that the key to using a mathematical system S as an aid to drawing conclusions from a nominalistic system N lies in proving in N* + S the equivalence of a statement in N* alone with some other statement (which I'll call an *abstract counterpart* of the statement in N*) which quantifies over abstract entities. Then if we want to determine the validity of an inference in N* (or equivalently, of an inference in N), it is unnecessary to proceed directly; instead we can if it is convenient 'ascend' from one

or more statements in N* to abstract counterparts of them, then use S to prove from these abstract counterparts an abstract counterpart of some other statement in N*, and 'descend' back to that statement in N*. I will illustrate how this procedure works in certain concrete cases; but again I must emphasize that the only thing required for the procedure to be legitimate is not that S be true but merely that N* + S be a conservative extension of N*, a condition which will always be met if Principle C of the previous chapter is satisfied.

My first illustration of this general procedure will be a very simple one: here, the mathematical theory S to be applied is simply the arithmetic of natural numbers (or more precisely, arithmetic plus a small amount of set theory, since arithmetic without such things as functions from concrete entities to numbers can never be applied).

Let N be a theory that contains the identity symbol and the usual axioms of identity, but does not contain any terms or quantifiers for abstract objects. In particular, N will not contain singular terms like '87'. It will, however, be convenient to suppose that N contains, besides the usual quantifiers '$\forall$' and '$\exists$', also quantifiers like '$\exists_{87}$' (meaning 'there are exactly 87') and '$\exists_{\geq 87}$' (meaning 'there are at least 87'). The logic is still of course, recursively axiomatizable—e.g. we could merely add to standard logic the axioms

$$\exists_{\geq 0}xA(x) \leftrightarrow \exists xA(x)$$
$$\exists_{\geq k}xA(x) \leftrightarrow \exists x[A(x) \wedge \exists_{\geq j}y(y \neq x \wedge A(y))],$$

where k is the decimal numeral that immediately succeeds j, and

$$\exists_{j}xA(x) \leftrightarrow \exists_{\geq j}xA(x) \wedge -\exists_{\geq k}xA(x),$$

where k and j are as above. In supposing that N contains this extra structure, we are not enriching either the expressive or the deductive power of N, we are merely ensuring that we can say simply what can be said only in a very roundabout way on the usual but artificial limitation to the two standard quantifiers plus identity. In particular, I must emphasize that by giving N this extra structure, I am not giving it any arithmetic: it contains no singular terms or quantifiers for numbers or any other abstract entities: the numeral '87' occurs in it not as a name, but merely as part of an operator symbol. Our goal is to show how inferences in N can be facilitated by introducing a system S that *does* contain arithmetic.

To see this, consider the following argument in N:

1 there are exactly twenty-one aardvarks (i.e., $\exists_{21}xA(x)$);
2 on each aardvark there are exactly three bugs;
3 each bug is on exactly one aardvark; so
4 there are exactly sixty-three bugs.

Is this valid? If one reasons in N, it will take a lot of work to find out—the inference needed for getting from the premises to the conclusion is long and tedious. (Though not nearly as bad as it would have been if we hadn't introduced the numerical quantifiers!) But if we have at our disposal a mathematical system S that includes the arithmetic of the natural numbers plus some set theory, things are considerably simplified. For then we can take, as an abstract counterpart of the first premise, the claim

1′ The cardinality of the set of aardvarks is 21;

1′ is an abstract counterpart of 1 because the equivalence of 1′ and 1 is provable in N + S.[17] Abstract counterparts of the other premises, and of the conclusion, are as follows:

2′ All sets in the range of the function whose domain is the set of aardvarks, and which assigns to each entity in its domain the set of bugs on that entity, have cardinality 3.
3′ The function mentioned in 2′ is 1–1 and its range forms a partition of the set of all bugs.
4′ The cardinality of the set of all bugs is 63.

But now in S we can prove:

(a) If all members of a partition of a set X have cardinality α, and the cardinality of the set of members of the partition is β, then the cardinality of X is $\alpha \cdot \beta$.

(b) The range and domain of a 1–1 function have the same cardinality; and
(c) $3 \cdot 21 = 63$.

But 1′, 2′, and 3′, in conjunction with (a)–(c), entail 4′; and since 1′–4′ are abstract counterparts of 1–4, i.e. their equivalence with 1–4 is provable in N + S,[17] we have proved 4 from 1–3 in N + S. So, by Principle C, 4 must follow from 1–3 in N alone. It is by some argument such as this that we know that 4 follows from 1–3 in N; certainly it isn't on the basis of having gone through a derivation in N that we know this.

The above illustration[18] of the application of mathematics is a very special one. Its special nature is illustrated by the fact that nothing was assumed about the theory N other than that it contained the logic of identity (supplemented with the numerical quantifiers; but these are in principle superfluous). This is not typical of the application of abstract entities in general, though it is typical of the application of the arithmetic of natural numbers. The fact that the natural numbers can find useful application outside the context of any powerful and specialized theories is what is behind the widely shared feeling that the arithmetic of natural numbers has a very special epistemological place. (Cf. for instance Kronecker's remark 'God created the natural numbers, all the rest is the work of man.')

But the fact that the arithmetic of natural numbers has this special status is not sufficient grounds to grant that it is *true*. For I have explained its special status instrumentally: its special status arises from its utility, and since we've shown that it is always in principle eliminable (i.e. you don't get any results with it that you couldn't get without it), its utility is no grounds for believing it true.

3

Second Illustration of Why Mathematical Entities are Useful: Geometry and Distance

Let us turn now to more complicated applications of abstract entities. Here, too, the situation fits the general description given in the second paragraph of Chapter 2: abstract entities are useful because we can use them to formulate abstract counterparts of concrete statements; then in proving a conclusion in N* from premises in N*, we can at any convenient point 'ascend' from concrete statements to their abstract counterparts, proceed at the abstract level for a while, and then finally 'descend' back to the concrete.

In the cases of application of mathematics that I will now turn to—which are the cases most important for physical theory—the key to carrying out the general strategy of finding 'abstract counterparts' is proving a *representation theorem*. Suppose that using some mathematical theory S which satisfies Principle C of Chapter 1, we can prove the existence of some mathematical structure $\mathscr{B}$ with certain specified properties. If we can then, using N* + S, prove the existence of one or more homomorphisms (structure-preserving mappings) from concrete objects (or k-tuples of concrete objects) into that mathematical

structure $\mathscr{B}$, then such a homomorphism will serve as a 'bridge' by which we can find abstract counterparts of concrete statements. Consequently, premises about the concrete can be 'translated into' abstract counterparts; then, by reasoning within S, we can prove abstract counterparts of further concrete statements, and then use the homomorphism to descend to the concrete statements of which they are abstract counterparts. The concrete conclusions so reached would always be obtainable without the ascent into the abstract (provided that the mathematical theory S satisfies Principle C); but the ascent into the abstract is often a tremendous saving of time and effort.

Let me illustrate this with an example: Hilbert's axiomatization of Euclidean geometry.[19] Any fully formulated physical theory will include a theory of physical space (or better, of space-time; but since our concern for the moment will be with Euclidean geometry, let's just consider space). Euclidean geometry, considered as a theory of physical space (which is how Euclid originally conceived it) is actually false, but that doesn't matter for my purposes: a false theory is still a theory, and we can use such a theory to illustrate the applicability of mathematical systems like the system of real numbers. Hilbert's formulation of the Euclidean theory is of special interest here because (besides being rigorously axiomatized) it does not employ the real numbers in the axioms; nevertheless, it explains why the system of real numbers can be usefully applied in geometric reasoning.

Without purporting to be very precise, we can say that Hilbert's theory is one in which the quantifiers range over regions of physical space, but do not range over numbers. The predicates of the theory include several, such as 'is a point', which need not concern us. In addition they include the following:

(a) a three-place predicate *between*, where 'y is between x and z' (symbolically, 'y Bet xz') is understood intuitively to mean that y is a point on the line-segment whose endpoints are x and z (the case where $y = x$ or $y = z$ is allowed, i.e. we're dealing with what I'll call *inclusive* betweenness);

(b) a four-place predicate of *segment-congruence*, which I'll write as 'xy Cong zw', understood intuitively to mean that the distance from point x to point y is the same as the distance from point z to point w;

and perhaps also

(c) a six-place predicate of angle-congruence, which I'll write as 'xyz A-Cong tuv', understood intuitively to mean that the angle formed by points x, y, and z with vertex at y is the same size as the angle formed by points to t, u, and v with vertex at u.

(The last of these predicates doesn't actually need to be taken as primitive, it can be defined in terms of the others.) Now, I have explained (b) and (c) intuitively in terms of distance and angle-size. But these explanations are not part of the theory: in fact the notions of distance and angle-size can't be defined in the theory (as is obvious from the fact that the theory doesn't quantify over real numbers). The fact that these quantitative notions are not definable in the theory might appear to raise a problem for Hilbert's formulation, for much of the reasoning in a typical book on Euclidean geometry proceeds in terms of the lengths of line-segments and/or the size of angles: in fact, many of the theorems are explicitly theorems about lengths (e.g. Pythagoras's theorem). Does this mean that Hilbert left something out? No, for he proved the kind of theorem I'm calling a *representation theorem*: he proved (in a broader mathematical theory) that given any model of the axiom system for space that he had laid down, there would be at least one function d mapping pairs of points onto the non-negative real numbers, satisfying the following 'homomorphism conditions':

(a) for any points x, y, z, and w, xy Cong zw if and only if $d(x,y) = d(z,w)$;

and (b) for any points x, y, and z, y is between x and z if and only if $d(x,y) + d(y,z) = d(x,z)$.

So if we take d to represent distance, segment-congruence becomes 'equivalent' to just the claim about distance we'd expect, and similarly for betweenness. (Hilbert also proved the existence of a function m mapping triples of points into numbers, satisfying analogous conditions: m was a representation for angle-sizes.) Given these results it was easy to show that the standard Euclidean theorems about lengths and angle-sizes would be true if restated as theorems about any functions d and m meeting the given conditions. So *in the geometry itself* we can't talk about numbers, and hence we can't talk about distances or angle-sizes; but we have a metatheoretic proof which associates claims about distances or angle-sizes with what we can say in the theory. Numerical claims then, are abstract counterparts of purely geometric claims, and the equivalence of the abstract counterpart with what it is an abstract counterpart of is established in the broader mathematical theory.

Incidentally, in addition to the representation theorems Hilbert established *uniqueness theorems*, one for distance, one for angle-size: e.g. the uniqueness theorem for distance says that if d_1 and d_2 are two functions mapping pairs of points into non-negative reals, both of which satisfy the two conditions just laid down, then d_1 and d_2 differ only by a positive multiplicative constant; and conversely, that if d_1 and d_2 differ only by a positive multiplicative constant, then d_1 satisfies (a) and (b) if and only if d_2 does. Thus the fact that geometric laws, when formulated in terms of distance, are invariant under multiplication of all distances by a positive constant, but are not invariant under any other transformation of scale, receives a satisfying *explanation*: it is explained by the *intrinsic facts* about physical space, i.e. by the facts about physical space which are laid down without reference to numbers in Hilbert's axioms. This is a point that will be important later, but for now let's go back to the representation theorem.

Hilbert's representation theorem, I've said, shows that statements that talk about space alone, without reference to numbers, are equivalent to certain 'abstract counterparts' which do talk about numbers. Because of this, we can use the theorem as a device for drawing conclusions about space (conclusions *statable without* real numbers) much more easily than we could draw them by a direct proof from Hilbert's axioms. For instance, it is not difficult to say intrinsically (see Figure 1):

(a) that a_1, a_2, a_3 and b_1, b_2, b_3 form right triangles with right angles at a_2 and b_2;

(b) that there is a segment $\overline{cd}$ such that $\overline{a_1a_2}$ is twice the length of $\overline{cd}$, $\overline{a_2a_3}$ is five times the length of $\overline{cd}$, $\overline{b_1b_2}$ is three times the length of $\overline{cd}$, and $\overline{b_2b_3}$ is four times the length of $\overline{cd}$. (E.g. we say that $\overline{a_1a_2}$ is twice the length of $\overline{cd}$ by saying that there is a point x between a_1 and a_2 such that a_1x Cong cd and xa_2 Cong cd.)

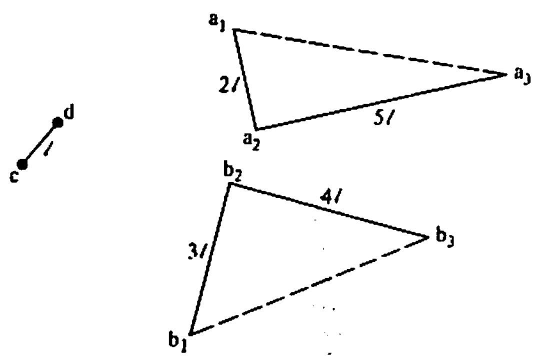

FIGURE 1

One might then wonder whether $\overline{a_1a_3}$ is longer than $\overline{b_1b_3}$. If one tries to answer this without using the representation theorem, it will be very difficult. But if one uses the representation theorem, one can invoke Pythagoras's theorem to quickly establish that $\overline{a_1a_3}$ is $\sqrt{29}$ times the length of $\overline{cd}$ and that $\overline{b_1b_3}$ is five times the length of $\overline{cd}$ and therefore that $\overline{a_1a_3}$ is indeed longer than $\overline{b_1b_3}$.

So invoking real numbers (plus a bit of set theory) allows us to make inferences among claims not mentioning real numbers much more

quickly than we could make those inferences without invoking the reals. And the inferences we make in this way will be correct every time. *Prima facie*, this might seem to be good evidence that the theory of real numbers (plus some set theory) is *true*: after all, if it weren't true, invoking it in arguments in this way ought to sometimes lead from otherwise true premises to a false conclusion. But we've seen in Chapter 1 that this *prima facie* plausible argument is deeply mistaken: the fact that the theory of real numbers (plus set theory) has this truth-preserving property is a fact that can be explained without assuming that it is *true*, but merely by assuming that it is *conservative*, which is a different matter entirely; in fact, as remarked in the Appendix, we really need only to assume a restricted form of conservativeness, which follows from the *consistency* of set theory alone.

4

Nominalism and the Structure of Physical Space

The reader might reasonably wonder about the assertion at the very end of the previous chapter: after all, Principle C says that when mathematical theories are added to nominalistic theories, you can never deduce any nominalistic consequences you couldn't deduce otherwise; but I haven't yet claimed that Hilbert's formulation of the Euclidean theory of space is genuinely nominalistic, I have claimed only that it doesn't quantify over *real numbers*. Now, this worry can be easily alleviated: for whether or not Hilbert's theory ought to be counted nominalistic on philosophical grounds, there can be no doubt that (if set theory is consistent) our mathematical theories apply to it in a conservative fashion. I will explain this, but first I want to raise the more controversial question of whether Hilbert's formulation of the Euclidean theory of physical space *can* be counted as genuinely nominalistic on philosophical grounds. This question raises several important issues.

I

Some of these issues can be brought out by considering the following objection. 'Hilbert's axiomatization of geometry just builds into physical space all the complexity and structure that the platonist builds into the real number system. For instance, Hilbert's axiomatization requires physical space to be uncountable, and in fact requires lines in physical space to be isomorphic to the real numbers. And there doesn't seem to be a very significant difference between postulating such a rich physical space and postulating the real numbers.'

In reply to this, let me first remind the reader that as I am conceiving nominalism, the nominalistic objection to using real numbers was not on the grounds of their uncountability or of the structural assumptions (e.g. Cauchy completeness) typically made about them. Rather, the objection was to their abstractness: even postulating *one* real number would have been a violation of nominalism as I'm conceiving it. Conversely, postulating uncountably many *physical* entities (e.g. uncountably many parts of a physical object, or of a light ray, or, as here, of physical space itself) is not an objection to nominalism; nor does it become any more objectionable when one postulates that these physical entities obey structural assumptions analogous to the ones that platonists postulate for the real numbers.

Perhaps it is a bit odd to use the phrase 'physical entity' to apply to space-time points.[20] But however this may be, space-time points are not abstract entities in any normal sense. After all, from a typical platonist perspective, our knowledge of mathematical structures of abstract entities (e.g. the mathematical structure of real numbers) is *a priori*; but the structure of physical space is an empirical matter. That is, most platonists who believe current physical theory believe that it is *a priori* true that there are real numbers obeying the usual laws, and that it is a high-level empirical hypothesis (not *easily* disconfirmed, but subject to revision by the development of an alternative physical theory) that there are lines in space which (locally anyway) are isomorphic to the real numbers. No platonist would identify the real numbers with the points on any physical line: for one thing, it would be arbitrary *which* such line one picked to identify the real numbers with, and arbitrary which point on the line to identify with 0 and which with 1; but more fundamentally, to make any such identification would be to identify the real numbers with something we can know about only empirically. (Occasionally it is suggested by those seeking a satisfactory formulation of quantum mechanics that we ought to view space and time as quantized. To my knowledge, no such proposal has ever been worked out very far; but if one were, and if it turned out to make the best sense of the evidence and best solve the interpretational difficulties of quantum theory, we would have strong empirical reasons to believe that between any two space–time points there were only finitely many others. Surely however we ought not to count such a development as an empirical discovery that there are only finitely many real numbers between 0 and 1.)

Even ignoring these points, there is a further reason that postulating physical space isn't like postulating real numbers: and that is that the ideology that goes with the postulate of points of space is less rich than that which goes with the postulate of the real numbers. With the postulate of real numbers goes the operations of addition and multiplication: no such operations are directly defined on space–time points in Hilbert's theory; indeed none are even implicitly definable since any introduction of an addition or multiplication function on space–time points would have to rely on an arbitrary choice of one point to serve as 0 and another to serve as 1. Something like addition can be reconstructed within Hilbert's theory, but it is addition of intervals rather than of points (and it doesn't give an addition *function* but rather a non-functional relation, 'interval x is the same length as the sum of intervals y and z'). With multiplication, we can't even reconstruct the relation of one interval being the product of two others: any introduction of such a product relation on intervals would have to depend on an arbitrary choice of one interval to serve as 'the unit interval', and no such notion is employed in the Hilbert theory. The best one can do with the Hilbert primitives is to reconstruct

comparisons of products of intervals, and it takes quite a bit of work to reconstruct such comparisons in a suitably generalizable way.[21] These observations make it clear that the objection that we are using the space-time points as if they were real numbers is quite erroneous.

These points are further reinforced by the fact that the usual theory of real numbers includes not only the first-order theory that invokes only the functions of addition and multiplication: it includes also the apparatus of quantification over functions defined on the real numbers, and also enough higher-order sets to enable us to define the continuity, differentiability, etc. of such functions. No such apparatus is invoked in the theory that takes space-points as the objects of quantification: though we will eventually see that the invariant content of many statements of continuity, differentiability, etc. of functions is expressible in the system to be developed, it is to be expressed without referring to or quantifying over functions or anything like functions.

One might think that if the system of space–time points was as distinct from the system of real numbers as I've been saying, then it would be a remarkable coincidence that points on a physical line should happen to have precisely the structure of such an important mathematical system as the real numbers, and that important mathematical operations (e.g. differentiation) on functions of real numbers should have analogs which play an important role in the physical theory. Surely, it could be argued, this can't be a coincidence: doesn't this show then that the physical theory is really platonism in disguise?

The trouble with this objection is that it completely ignores history: the theory of real numbers, and the theory of differentiation etc. of functions of real numbers, was developed precisely in order to deal with physical space and physical time and various theories in which space and/or time play an important role, such as Newtonian mechanics. Indeed, the reason that the real number system and the associated theory of differentiation etc. is so important mathematically is precisely that so many of the problems to which we want to apply mathematics involve space and/or time. It is hardly surprising that mathematical theories developed in order to apply to space and time should postulate mathematical structures with some strong structural similarities to the physical structures of space and time. It is a clear case of putting the cart before the horse to conclude from this that what I've called the physical structure of space and time is really mathematical structure in disguise.

So in summary: there is indeed a good deal in common between on the one hand the structure of physical space that both I and the platonists postulate and on the other hand the structure of mathematical objects postulated by platonists; and there is an obvious reason why there should be this commonality of structure, given that the mathematics was developed to deal with physical space (and time). Still, there are many ways in which the physical structure is less rich than the mathematical structure (e.g. no addition relation defined on points; no multiplication relation defined on points or even on intervals; no functions, sets of functions, etc.). And the physical structure is all an empirical postulate, subject to revision by experience in a way that mathematics is not.

There are, to be sure, certain views of space-time according to which the quantification over space-time points or space-time regions really would be a violation of nominalism. I'm speaking of *relationalist* views of space-time, as opposed to the *substantivalist* view. According to the substantivalist view, which I accept, space-time points (and/or space-time regions) are entities that exist in their own right. In contrast to this are two forms of relationalist view. According to the first (*reductive relationalism*), points and regions of space-time are some sort of set-theoretic construction out of physical objects and their parts; according to the second (*eliminative relationalism*), it is illegitimate to quantify over points and regions of space-time at all.[22] It is clear that reductive relationalism is unavailable to the nominalist: for according to that form of relationalism, points and regions of space-time are mathematical entities, and hence entities that the nominalist has to reject. So a nominalist must either be a substantivalist or be an eliminative relationalist, and only in the first case can he find Hilbert's theory acceptable.

It is my view however that independently of nominalism, a substantivalist view is preferable to either form of relationalist view, for a number of reasons most of which cannot be discussed here. I will merely say that I don't think that any relationalist programme, of either a reductive or an eliminative sort, has ever been satisfactorily carried out, even given a full-blown platonistic apparatus of sets. The problem for relationalism is *especially* acute in the context of physical theories that take the notion of a *field* seriously, e.g. classical electromagnetic theory. From the platonistic point of view, a field is usually described as an assignment of some property, or some number or vector or tensor, to each point of space-time; obviously this assumes that there are space-time points, so a relationalist is going to have to either avoid postulating fields (a hard road to take in modern physics, I believe) or else come up with some very different way of describing them. The only alternative way of describing fields that I know is the one I use later in the monograph in connection with the gravitational potential field in Newtonian mechanics: it does without the properties or the numbers or vectors or tensors, but it does not do without the space-time points.[23] In general, it seems to me that recent developments in both philosophy and physics have made substantivalism a much more attractive position than it once was; it certainly has been adopted by the majority of the 'new wave' of space-time theorists. (For two good discussions, see John Earman, 'Who's afraid of Absolute Space?' and Michael Friedman, *Foundations of Space-Time Theories*[24]). In any case, substantivalist views of space-time are certainly possible, and on such a substantivalist view it is perfectly nominalistic to quantify over space-time points and/or space-time regions.

This doesn't justify quantifying over points or regions *of space* actually, if a point or region of space is construed as an entity that endures through time. And indeed, there are real difficulties about quantifying over points or regions of space on any such construal, for on such a construal it would seem to make objective sense to ask whether two non-simultaneous events are at the same point of space, and hence to ask whether a given object is at absolute rest. The notion of absolute rest is one that positivists have quite rightly objected to, in my view: this is a point I will return to briefly in the next chapter. Fortunately, however there is a way to construe quantification over points and regions of space so that it involves no commitment to absolute rest, in any physical theory in which a notion of simultaneity is available: simply regard a claim about space as an abbreviation for the assertion that the claim holds for each of the spatial slices of space-time (i.e. the slices generated by the simultaneity relation). So the claim that physical space is Euclidean is translated into the claim that each of the spatial slices of space-time is Euclidean. It is trivial to rewrite Hilbert's axiomatization of the geometry of space so that that is explicitly what it says; if we do so, then the objects in the domain of the quantifier are really space-time points rather than points of space, and there can be no danger of viewing the theory as being committed to the idea that absolute rest is a physically significant notion. (I won't bother to explain how to rewrite Hilbert's theory in this way however, since the theory that resulted would be of less use than a stronger nominalistic theory about space-time structure to be set out in Chapter 6.)

II

I have allowed our nominalist to quantify over points *or* regions of space-time. Is there any reason why he shouldn't quantify over *both* points *and* regions? Some philosophers would be willing to accept the existence of certain kinds of regions—say, regular open regions—but not of points. This is not a view I *object to*: it may well be possible to find nominalistic systems similar in many respects to the Hilbert system (and to the systems to follow later on in the book), but that quantify over arbitrarily small regular open regions instead of over points; and if it *is* possible, then the nominalist has no reason to object to dispensing with points in favor of regular open regions. But I also do not see that the nominalist has any particular *reason* to forego points for arbitrarily small regular open regions—the desire for such purity

is a quasi-finitist desire, not a nominalist desire. Since the desire to forego points is not one I share, and since it appears to be mathematically difficult, I will make no attempt to satisfy that desire in this book.

How about the converse question: given a nominalism in which we quantify over space-time points, is there any added difficulty in quantifying over regions? If our nominalist accepts Goodman's calculus of individuals,[25] then the introduction of points carries with it the introduction of regions: for a region is just a *sum* (in Goodman's sense) of the points it contains.[26] And even if one does not accept the calculus of individuals in general—even if one thinks that there are entities that can't meaningfully be 'summed'—there seems to be little motivation for allowing points and yet disallowing regions: in fact, it seems attractive to regard points of space-time as a special case of regions, namely as regions of minimal size. So it seems to me that regions are nominalistically acceptable. (I should note however that only fairly 'regular' regions are directly used in the monograph, so a nominalist who would balk at the use of highly 'irregular' regions need not balk at the uses to which regions will actually be put.)[27]

If these claims about what should count as nominalistic are accepted, then there is at least an important sense in which Hilbert's formulation of the Euclidean theory of space is or can with a little rewriting be made nominalistic. Hilbert's theory is usually formulated as a second-order theory, in which the first-order variables range over points, lines, and planes; in other words, the first-order variables range over regions of various kinds. Consequently, the second-order variables range over *sets* of points, lines, and planes, and that doesn't look very nominalistic. However, only one second-order axiom is really needed, the Dedekind continuity axiom; and in this axiom one quantifies only over non-empty sets of *points*. This is important, for in the absence of any further use of sets, there is no substantive difference between a *set* of points on the one hand and a Goodmanian *sum* of points or a region on the other. So we can regard the second-order quantifiers in Hilbert's theory as ranging over regions. (And we can then if we like restrict the range of the first-order quantifiers to points, either by using second-order quantifiers whenever we want to speak of lines and planes, or by paraphrasing claims about lines and planes in terms of claims about points and the relation of betweenness.) If we write Hilbert's theory in this way, then the quantifiers (both first-order and second-order) range *only* over regions of space; and I've argued that regions of space are nominalistically acceptable entities. So *if we write Hilbert's formulation of the Euclidean theory of space in this way, it has a purely nominalistic ontology*.

It does, admittedly, have a logic that one might find objectionable: it involves what might be called *the complete logic of the part/whole relation*, or *the complete logic of Goodmanian sums*, and this is not a recursively axiomatizable logic. To clarify this, note that the theory as I've suggested it be written is still a second-order theory, that is, it still involves second-order logic: it is merely that because of the nature of the objects in the range of the first-order quantifiers (viz. because they do not overlap), and because also we haven't invoked variables for functions or for predicates of more than one place, no nominalistically dubious entities need be invoked to serve in the range of the second-order quantifiers. This ontological difference is perhaps sufficiently striking so that we ought not to call the logic 'second-order logic' anymore, but something else, such as 'the complete logic of Goodmanian sums'; nonetheless, the consequence relation is still like that of second-order logic, which is not recursively axiomatizable. Consequently, insofar as one objects to the strength of the second-order consequence relation, one will object to this version of Hilbert's formulation of the Euclidean theory of space.

I share the feeling that the invocation of anything like a second-order consequence relation is distasteful, and will discuss the possibility of eliminating it in the final chapter of the book. For now, let me simply note that for platonistic theories too, the most natural and intuitive formulation of a theory is often a second-order formulation. For instance, intuitive set theory—by which I mean not the intuitive Cantorian set theory that was shown inconsistent, but the intuitive set theory that underlies the Zermelo–Fraenkel and similar axiomatizations—is a

second-order theory: e.g. it will include as an axiom or a theorem the second-order separation principle

$$\forall P\ \forall x\ \exists y\ \forall z(z \in y \leftrightarrow z \in x \wedge P(z)).$$

To get a first-order axiomatization we have to weaken the theory, replacing the second-order axiom or axioms by schemas of first-order axioms, namely the schema of replacement and/or separation. This first-order weakening of intuitive set theory has a lot of 'non-standard' models (e.g. models in which sets that are really infinite satisfy the formula that is usually regarded as defining finiteness): such models are 'non-standard' precisely because they are *not* models of second-order set theory.[28] Similarly, the second-order Hilbert axiomatization of geometry can be weakened to a first-order system, in either of two ways: a severe weakening which drops the use of regions entirely has been studied by Tarski[29] and a less severe weakening to a first-order axiomatization will be mentioned in the final chapter. But these first-order weakenings of the Hilbert system all have non-standard models. These non-standard models together with the non-standard models of first-order set theory make the question of the relation between the first-order nominalistic theory and the first-order platonistic theory harder to settle; a representation theorem like Hilbert's is much easier to state and prove if it is taken as relating the intuitive (second-order) nominalistic geometry to the intuitive (second-order) set theory than if it is taken as relating their first-order weakenings. For this reason I will put off the issue of first-order axiomatization until the final chapter.

Since I am putting that off, it is necessary to make sure that nothing in my remarks in the previous chapter, about the philosophical significance of Hilbert's representation theorem, turned on the false assumption that Hilbert's axiomatization was first order. The only remark which might seem suspect from this point of view came at the very end of the chapter. After pointing out that mathematical entities (real numbers together with functions from space-time points into the reals) can usefully be employed in connection with Hilbert's axiomatization, and that when they are employed we are never led to a false conclusion about space from true premises, I raised the question of whether this fact is evidence that the theories which postulate mathematical entities are *true*. My answer was no: we could, I claimed, explain the truth-preservingness of mathematics in this context entirely by its conservativeness, which is a much weaker (or more accurately, a quite different) property; in fact, I remarked that we really only need to assume a restricted form of conservativeness, which follows from the consistency of mathematics alone. This, however, raises a question: is the consistency of mathematics (i.e. the consistency of set theory, since mathematics reduces to set theory) sufficient to entail that mathematics can be employed in reasoning about *second-order* theories in a truth-preserving way? The answer is that the semantic consistency of *second-order* set theory *is* sufficient for this conclusion: in fact, the main arguments of the Appendix to Chapter 1 go over with little alteration when all the theories are taken to be second order.[30] The upshot is that in the context of reasoning about Euclidean geometry at least, the nominalist can invoke the theory of real numbers (with the attendant functions) as much as he likes, for he is guaranteed that he can never be led into error by so doing.

5

My Strategy for Nominalizing Physics, and its Advantages

So far, I have not tried to argue that we can come up with nominalistic theories to replace platonistic ones: I have merely argued that *if* we had a nominalistic theory, then it would be legitimate to introduce mathematics as an auxiliary device that aids us in drawing inferences; and I have tried to indicate why that auxiliary device would be useful, and to show that its usefulness as an auxiliary device is no grounds whatever for supposing that it consists of a body of truths. The real question then is whether an attractive nominalistic formulation of physics is possible. I say an *attractive* nominalistic formulation, because if no attractiveness requirement is imposed, nominalization is trivial: simply take as axioms of your physical theory all the nominalistically-statable consequences of the platonistic formulation of the theory. (Or, if you want a recursive set of axioms, take the Craigian transcription of the set of nominalistically-statable consequences.) Obviously, *such* ways of obtaining nominalistic theories are of no interest. The way that I will suggest of obtaining nominalistic theories is very different from this.

In order initially to motivate the idea that an attractive nominalistic formulation of physics is possible, let us return to Hilbert's axiomatization of geometry. There are two approaches to axiomatizing geometry, sometimes called *the metric approach* and *the synthetic approach*. In the metric approach we take as primitive a particular function-symbol d, which we regard as denoting a particular mapping of pairs of points of space into the real numbers. Then if we regard the mathematical laws of real numbers, functions, and so forth as independently given, we can use d to lay down a relatively simple set of axioms for the geometry. The synthetic approach is the one that Hilbert followed, the one which does without real numbers, functions, etc. This approach is also the one that Euclid had (less rigorously) followed long before—Euclid *had* to follow the synthetic approach, because the theory of real numbers hadn't been sufficiently developed in his day for the metric approach to be possible. (The real numbers were in fact first introduced into mathematics as a means of simplifying geometric reasoning). But to anyone already familiar with the theory of real numbers, the metric approach is a good deal easier, and for that reason it is used in many recent books in geometry. If one were familiar only with the metric approach to Euclidean geometry, one would probably conclude that one needs to quantify over real numbers in developing a theory of the geometry of space. The Hilbert axiomatization, however, shows that this is not so.

My guess is that the same is true for other physical theories. Insofar as they've been rigorously formulated at all, they've been formulated platonistically, for it is *easier* to formulate a theory that way when one has a sufficiently developed mathematics. My guess, however, is that a thorough foundational analysis of such theories will show that reference to real numbers, etc. is no more necessary in them than it is in geometry. And this isn't a *mere* guess: I substantiate it in Chapters 6–8 with respect to one physical theory, viz. Newton's theory of gravitation; and it would be routine to extend the nominalistic treatment of gravitational theory to other theories with a similar format, such as special relativistic electromagnetic theory.

I believe that such 'synthetic' approaches to physical theory are advantageous not merely because they are nominalistic, but also because they are in some ways more illuminating than metric approaches: they explain what is going on without appeal to extraneous, causally irrelevant entities. The attempt to eliminate theoretical entities of physics (e.g. electrons) from explanations of observable phenomena is not likely to be possible without bizarre devices like Craigian transcriptions; it is also not likely to be illuminating even if it is possible, because electrons are causally relevant to the phenomena they are invoked to explain. But even on the platonistic assumption that there are numbers, no one thinks that those numbers are causally relevant to the physical phenomena: numbers are supposed to be entities existing somewhere outside of space-time, causally isolated from everything we can observe. If, as at first blush appears to be the case, we need to invoke some real numbers like 6.67×10^{-11} (the gravitational constant in $m^3/kg^{-1}/s^{-2}$) in our explanation of why the moon follows the path that it does, it isn't because we think that that real number plays a role as a *cause* of the moon's moving that way; it plays a very different role in the explanation than electrons play in the explanation of the workings of electric devices. The role it plays is as an entity *extrinsic to the process to be explained*, an entity related to the process to be explained only by a function (a rather arbitrarily chosen function at that). Surely then it would be illuminating if we could show that a purely intrinsic explanation of the process was possible, an explanation that did not invoke functions to extrinsic and causally irrelevant entities.

In saying that this is an advantage, I don't mean to suggest that extrinsic explanation should always be avoided: the point is rather that from a proper synthetic theory, one will be able to prove the equivalence of the intrinsic and extrinsic explanations. (That is, one will be able to prove that the two explanations are equivalent given the assumption that the entities involved in the extrinsic explanation exist. If one believes that they don't exist, then one will hold that the extrinsic explanation is merely a useful fiction, but one which can be used in good conscience by anyone who knows of the intrinsic explanation, because of the conservativeness of mathematics.) An illustration of this is provided by synthetic geometry: given the axioms of synthetic geometry, one can prove (given standard mathematics) the equivalence of on the one hand explanations of features of physical space stated in terms of betweenness and congruence and on the other hand extrinsic explanations involving quantitative distance and angle measures; hence one is free to use the extrinsic explanations in practice.

I am saying then that not only is it much likelier that we can eliminate numbers from science than electrons (since numbers, unlike electrons, do not enter causally in explanations), but also that it is more illuminating to do so. It is more illuminating because the elimination of numbers, unlike the elimination of electrons, helps us to further a plausible methodological principle: the principle that *underlying every good extrinsic explanation there is an intrinsic explanation*. If this principle is correct, then real numbers (unlike electrons) have *got* to be eliminable from physical explanations, and the only question is how precisely this is to be done.

Note that the principle I've italicized is not a nominalistic principle: it could perfectly well be accepted by a platonist, though of course, not by any platonist who believed that one could argue for platonism by saying that mathematical entities are needed for physics. Conversely, a nominalist need not accept the principle. There are indeed ways of trying to establish the possibility of nominalism that, even if successful, would not establish the italicized principle. One such approach is that of Charles Chihara in his book *Ontology and the Vicious Circle Principle* (see note 4 above). Chihara's approach is one of those alluded to in the introduction, on which one tries to *reinterpret* mathematics: in this case, one reinterprets it as being about linguistic entities instead of abstract entities. I find my approach preferable to his for three reasons. In the first place, as Chihara of course recognizes, the linguistic view requires that only predicative mathematical reasoning be used in application, and it isn't at all obvious that we don't need impredicative

reasoning in doing science. (My view licenses (but doesn't demand) the use of impredicative reasoning, as we shall see in Chapter 9.) In the second place, the linguistic entities that Chihara appeals to include sentence types no token of which has even been uttered, and it is not at all obvious to me whether these should count as nominalistically legitimate. But third and most fundamental, Chihara's view does nothing to illuminate the use of extrinsic, causally irrelevant entities in the application of mathematics. That is, Chihara's methods do not show us how to provide intrinsic explanations underlying extrinsic explanations; they merely show that linguistic surrogates of mathematical entities can be used in place of mathematical entities in our extrinsic explanations (a fact which I take to be uninteresting, since as I've argued, there is no need in the mathematical case to regard *extrinsic* explanations as literally true).

I conclude this chapter by noting that one of the things that gives plausibility to the idea that extrinsic explanations are unsatisfactory if taken as *ultimate* explanation is that the functions invoked in many extrinsic explanations are so arbitrary. For example, in the case of geometry, the choice of one distance function over any other one which differs from it by positive multiplicative constant is completely arbitrary; it reflects in effect an arbitrary choice of units for distance. (When we move from geometry to physics generally, there is in the metric approach not only an arbitrary choice of a unit of distance, but also an arbitrary choice of units for other quantities, an arbitrary choice of a rest frame, and various other arbitrary choices as well). Now an analogous arbitrariness *could* exist on an intrinsic approach too: it would exist if we singled out a particular pair of points of space-time (say, the endpoints of a certain platinum rod in the Bureau of Standards at such and such a time), and constantly referred to this pair of points in making distance comparisons when we developed the theory. Hilbert, however, did not resort to such an unaesthetic move in his intrinsic development of geometry; nor shall I resort to it in my intrinsic formulation of gravitational theory. What Hilbert did do (in his uniqueness theorem) was to *explain, in terms of intrinsic facts about* *space which are statable without such arbitrary choices, why the choice of functions to be invoked in the extrinsic theory will be arbitrary to precisely the extent that it is.* This feature of the Hilbert approach to geometry is highly attractive, and it is a feature I will take pains to emulate when I extend the synthetic treatment of geometry to a synthetic treatment of gravitational theory.

6

A Nominalistic Treatment of Newtonian Space-Time

I turn now to the problem of giving a nominalistic formulation of physics, a formulation which meets the additional constraints imposed in Chapter 5: it is to be 'attractive', unlike Craigian axiomatizations; it is to be a 'purely intrinsic' formulation; and it is to be a formulation that does not appeal to arbitrarily chosen objects to serve as units of length, or to arbitrarily chosen systems of coordinates, or to any such thing. These further constraints are not very precise, but I hope that they are *reasonably* clear; for I will implicitly and sometimes explicitly invoke these constraints (especially the last one) in motivating the construction to follow.

The first step in giving a nominalistic formulation of physics is to give a nominalistic treatment of space-time. I've already discussed a nominalistic treatment of space, but space-time is a little different, both in Newtonian mechanics and in special relativity. It is different not just in being 4-dimensional instead of 3-dimensional, but in not having a full Euclidean structure. (Also in having some *extra* structure not present in Euclidean 4-space.)

settles for is a nominalistic one like N_0 or a platonistic one like P_0; hence it can't be used as an argument for the inadequacy of N_0 unless platonistic first-order theories are also admitted to be inadequate. Consequently, if one is committed to first-order theories, then the only obvious way to decide if one is good enough is to decide whether it is powerful enough to get the results that are seriously needed in practice, i.e. excluding recherché results like those obtained by Gödelization. As I've said, I think it highly likely that N_0 or some slightly stronger first-order subtheory of N passes this test.

The argument at the beginning of the previous paragraph, then, *may* indicate an inadequacy in N_0; but if so, it is an inadequacy in P_0 as well, and hence it is not an argument for platonism. If you want to cure this 'inadequacy', the only recourse is to go to a second-order theory—either N, or platonistic gravitational theory in the context of second-order set theory. But since as we've seen N has all the nominalistic consequences that second-order platonistic set theory has, it is hard to see in the context of second-order logic what the advantages of platonism can be. Either way, then, it looks as if nominalism triumphs.

Notes

Preliminary Remarks.

1. The 'part of mathematics that doesn't contain references to abstract entities' is really just applied logic: it is the systematic deduction of consequences from axiom systems (axiom systems similar in many respects to those used in platonistic mathematics, but containing references only to physical entities). Very little of ordinary mathematics consists merely of the systematic deduction of consequences from such axiom systems: my claim however is that ordinary mathematics can be replaced in application by a new mathematics which does consist only of this.

2. I believe the approach is generalizable to curved space-time, but haven't thought through all the details.

3. As it happens, a certain reduction of structural assumptions will fall out 'by accident', on one of the two nominalistic formulations of gravitational theory I will give (the one I will call N_0 in Chapter 9). Moreover, both nominalistic formulations, but especially N_0, seem especially well suited for a study of the effects of further weakenings of the structural assumptions.

4. The most thorough presentation of the Quinean argument is actually not by Quine but by Hilary Putnam: cf. *The Philosophy of Logic* (New York: Harper, 1971), especially Chapters V–VIII.

Some of the arguments I do not take seriously (e.g. the argument that we need to postulate mathematical entities in order to account for mathematical intuitions) are well treated in Chapter 2 of Chihara, *Ontology and the Vicious Circle Principle* (Ithaca: Cornell University Press, 1973).

5. Actually, I do not think that a platonistic proof of the adequacy of our theories serves *merely* as a *reductio:* I think that a nominalist too should be convinced by a platonistic proof about the deductive powers of a given nominalistic theory. But a defense of this claim would be a long story. (Some much too brief remarks on this matter are contained in note 10 in the next chapter.) In any case, the nominalist need not ultimately rely on such platonistic proofs of the adequacy of his systems: in principle at least, he and his fellow nominalists could simply spin out deductions from nominalistic axiom systems like the ones suggested later in the monograph. In this sense, the reliance on platonistic proofs could be regarded as a temporary expedient.

CHAPTER 1

Why the Utility of Mathematical Entities is Unlike the Utility of Theoretical Entities

6. Count '$=$' as logical.

7. That is, replace every quantification of form '$\forall x_i$ (...)' by '$\forall x_i$ (if not $M(x_i)$ then ...)', and every quantification of form '$\exists x_i$ (...)' by '$\exists x_i$ (not $M(x_i)$ and ...)'.

8. The formal content of saying that N is 'nominalistically statable' is simply that it not overlap in non-logical vocabulary with the mathematical theory to be introduced. (Recall that '$=$' counts as logical.) This is all we need to build into 'nominalistically statable' in order for Principle C to be *true*. For Principle C to be *of interest*, we must suppose in addition that the intended ontology of N does not include any entities in the intended extension of the predicate 'M' of S; for if this condition were violated, then $N^* + S$ would not correspond to the 'intended' way of combining N and S.

9. Proof: Suppose $N^* + S + \{\exists x - M(x)\}$ implies A^*. Then $N^* + S$ implies $A^* \vee \forall x(-M(x) \rightarrow x \neq x)$; that is, it implies B^* where B is $A \vee \forall x(x \neq x)$. Applying Principle C$'$, we get that N^* implies B^*, and consequently that $N^* + \{\exists x - M(x)\}$ implies A^*. From this it clearly follows that N implies A.

Principle C$'$ does not quite follow from Principle C, for a theory S could imply that there are non-mathematical objects but not imply anything else about the non-mathematical realm (in particular, not imply that there are at least two mathematical objects—the latter would violate Principle C as well as Principle C$'$).

10. In what sense does he know this? At the very least, he knows it in the sense that a platonist mathematician who proves a result in recursive function theory by means of Church's thesis knows that he could construct a proof that didn't invoke Church's thesis. The platonist mathematician hasn't proved using the basic forms of argument that he accepts that such a proof is possible, for he hasn't proved Church's thesis. (Nor can he even state Church's thesis except by vague terms like 'intuitively computable'.) Still, there is a perfectly good sense in which our platonist mathematician does know that a proof without Church's thesis is possible—after all, he could probably come up with Turing machine programs at each point where Church's thesis was invoked, if given sufficient incentive to do so. In precisely the same sense, the nominalist knows that for any platonist proof of a nominalistically-stated conclusion from nominalistically-stated premises there is a nominalistic proof of the same thing.

Just what this sense of 'know' is (or, just what *kind* of knowledge is involved) is a difficult matter: it doesn't seem to me quite right to call it 'inductive' knowledge. But however this may be, it is a kind of knowledge (or justification) whose strength can be increased by inductive considerations: in the recursive function case, by knowledge that in the past one had been able to transform proofs involving the imprecise notion of 'intuitively computable' to proofs not involving it when one has tried (or by knowledge that others have been able to effect such transformations, and that one's own judgements of intuitive computability tend to coincide with theirs). In the conservativeness case, the kind of inductive considerations that are relevant are the knowledge that in the past no one has found counter examples to conservativeness, and also the knowledge that in many actual cases where platonistic devices are used in proofs of nominalistic conclusions from nominalistic premises (such as the cases discussed in Chapters 2 and 3), these devices are eliminable in what seems to be a more or less systematic way.

These remarks suggest that the nominalistic position concerning the use of platonistic proofs is about comparable to the platonist's position concerning proofs that use Church's thesis. Actually I think that the nominalist's position is in one respect even better, for he can rely on something that the platonistic recursion theorist has no analog of: viz., the mathematical arguments for conservativeness given in the Appendix. Of course, these arguments don't raise the claim that mathematics is conservative to complete certainty, for two reasons. One reason is that something at least as strong as the consistency of set theory is assumed in them, and no one (platonist or nominalist) can be *completely* sure of that. The other reason is that these proofs (at least the first, and both if one is sufficiently strict about what counts as nominalist) are platonistic, and so some story has to be told about how the nominalist is justified in appealing to them outside the context of a *reductio*. I think some such story can be told, but it would be a long one. (An essential idea of the story would be that we use conservativeness to argue for conservativeness: we've seen that the nominalist has various initial quasi-inductive arguments which support the conclusion that it is safe to use mathematics in certain contexts; if he then *using mathematics in one of those contexts* can prove that it is safe to use mathematics in those contexts, this can raise the support of the initial conclusion quite substantially.)

A platonist might be inclined to dismiss the sort of quasi-inductive knowledge discussed in this note. But to do so would be to pay a high price: *most* of mathematics is known only in this quasi-inductive sort of way. For most of it is proved by rather informal proofs; and though we all do in an important sense *know* that we could reconstruct such proofs formally if forced to do so, still the principle that formal proofs are always possible when we have an intuitively acceptable proof is, like Church's thesis, a principle that we haven't proved and have no prospect of proving.

11. We will see, however, that the utility of number theory is less subject to such empirical vicissitudes than are theories about say the real numbers.

12. D should either be taken to consist entirely of non-sets, in which case c should be taken to be the empty set (or another non-set); or D should be taken to consist entirely of sets of the same rank and c should be another set of that rank. Given any model of a theory, there is no difficulty in getting another model whose domain meets these conditions.

13. Suppose S + T* is inconsistent; the Robinson theorem says that there is a sentence B in the language common to S and T* such that S⊢ B and T*⊢ −B. Clearly if S and T are both consistent, then B can't be either a logical truth or a contradiction. The language common to S and T* consists, in the case of a 'pure' mathematical theory, of 'M' (the predicate 'mathematical' discussed prior to the formulation of Principle C) and '=', and nothing else. The only statements in this language other than logical truths or contradictions are statements saying how many mathematical objects there are and/or how many non-mathematical objects there are. But since all statements in T* are explicitly restricted to non-mathematical objects, T* can't imply anything about how many mathematical objects there are, and since the mathematical theory is assumed to be a pure one it can't imply anything about how many *non*-mathematical objects there are. So there can be no such B; that is, the supposition that S and T are consistent but S + T* is inconsistent has been reduced to absurdity.

14. A sketch of the proof of the last fact is given in Thomas Jech, *The Axiom of Choice*, p. 51, problem 1. Using this fact, the proof that conservativeness implies consistency is just as in note 13.

15. Proof: if ZF is consistent, and ZF⊢ 'T is consistent' (where 'T is consistent' abbreviates the formalization in ZF of the claim that T is syntactically consistent) then ZF + 'T is consistent' is certainly consistent. Since the Gödel completeness theorem (together with various more elementary facts) is provable in ZF, then so is ZF + 'there is a model of T in which all elements of the domain have the same rank and such that there is a set of that rank that is not in the domain'. (Cf. note 12 for the motivation of this.) If T has n primitive predicates, then a model of T consists of a domain together with n items each corresponding to one of the terms. Introducing new names b, c_1, . . ., c_n for these things, and a name d for the set of the right rank that isn't in the domain of the model, we see that ZF + '$\langle b, c_1, \ldots, c_n\rangle$ is a model of T' + 'all members of b have the same rank' + 'd has the same rank as all members of b' is also consistent. Call this theory ZF_T.

By the principle of transfinite recursion, there is a formula $\mathscr{S}(x)$ (in the language of ZF_T) such that

$$ZF_T \text{ (in fact, ZF)} \vdash \mathscr{S}(x) \leftrightarrow x \in b \vee x = d \vee (x \neq \varnothing \wedge \forall y(y \in x \rightarrow \mathscr{S}(y))).$$

If we translate statements of $ZFU_{V(T)} + T^*$ into ZF_T by using $\mathscr{D}(x)$ to restrict all variables, and translating 'Set(x)' as '$x \notin b$', '$\in$' as '$\in$', '$\varnothing$' as 'd', and '$A(x_1, \ldots, x_k)$' where A is the ith predicate of T as '$\langle x_1, \ldots, x_k \rangle \in c_i$', then each of the translations of the axioms of $ZFU_{V(T)} + T^*$ is a theorem of ZF_T. Since ZF_T is consistent (on the assumption that ZF is), so is $ZFU_{V(T)} + T^*$.

16. To see this, observe first that the preceding note proved a slightly stronger result than was claimed: it proved that if ZF + 'T is consistent' is consistent, then $ZFU_{V(T)} + T^*$ is consistent. So we now need only show that if ZF is ω-consistent and T is consistent and recursively enumerable, then ZF + 'T is consistent' is consistent.

The reason for this is simple: if T is consistent, then nothing is a proof from T of '0 = 1'; and if T is also recursively enumerable, ZF is strong enough to prove "Ⓚ is not the Gödel number of a proof from T of '0 = 1'", for each numeral k. By the ω-consistency of ZF it follows that one cannot prove in ZF anything of the form '∃x(x is the Gödel number of a proof from T of '0 = 1')'; so one can't prove 'T is not consistent' from ZF, and so ZF + 'T is consistent' is consistent.

CHAPTER 2

First Illustration of Why Mathematical Entities are Useful: Arithmetic

17. To simplify things I haven't shifted from N to N* in this case, because in this example such a shift isn't needed. If we did shift from N to N*, we would rewrite 1 as

1* There are exactly twenty-one aardvarks that are not mathematical objects.

and take as an abstract counterpart of 1* the claim

(1*)′ The cardinality of the set of aardvarks that are not mathematical objects is 21.

18. Hilary Putnam gives a similar illustration, in 'The thesis that mathematics is logic' in *Philosophical Papers*, Vol. 1 (Cambridge: Cambridge University Press, 1975): cf. pp. 26–33, and in particular pp. 31–3, where Putnam points out that the application of number theory requires only the consistency of mathematics. I was in fact originally led to the view that I take in this monograph largely by thinking about these striking remarks of Putnam's. Note, however, that the conclusion that Putnam draws from his remarks is rather different from the one I draw: his conclusion is that we should interpret pure mathematics as asserting the possible existence of physical structures satisfying the mathematical axioms, whereas my conclusion is that we don't need to interpret pure mathematics at all.

In another paper in the same volume, 'What is mathematical truth?', Putnam takes back the view put forth in the earlier paper, claiming in effect that the account given of the application of number theory couldn't possibly be extended to an account of how the theory of functions of real variables is applied to physical magnitudes. (Cf. pp. 74–5. Putnam has presented this point at greater length in *The Philosophy of Logic* (see note 4).) Perhaps in part his pessimism is due to the assumption that any extension of the account of how number theory is applied would have to be put into the framework of a reinterpretation of mathematics; in any case, the later chapters of this monograph (starting with Chapter 3) show how to perform the extension in question, if we forget about reinterpreting pure mathematics and worry only about reinterpreting its applications.

CHAPTER 3

Second Illustration of Why Mathematical Entities are Useful: Geometry and Distance

19. David Hilbert, *Foundations of Geometry* (LaSalle, Ill: Open Court, 1971).

CHAPTER 4

Nominalism and the Structure of Physical Space

20. For the reader who wonders why I say 'space-time point' instead of 'point of space': your curiosity will be alleviated in the last paragraph of Section I of this chapter.

21. 'In a suitably generalizable way' means 'in a way generalizable to products of spatio-temporal intervals with scalar intervals'. The suitably generalized way of making product comparisons is given in Chapter 8.

22. Or anyway, it is illegitimate to quantify over *unoccupied* points and regions: quantification over occupied points or regions (i.e. points or regions wholly occupied by parts of physical objects) could be regarded as equivalent to quantifying over the objects which occupy them, and hence as unproblematic to the relationalist.

23. Note incidentally that according to theories that take the notion of a field seriously, space-time points or regions are full-fledged causal agents. In electromagnetic theory for instance, the behavior of matter is causally explained by the electromagnetic field values at unoccupied regions of space-time; and since, platonistically speaking, a field is simply an assignment of properties to points or regions of space-time, this means that the behavior of matter is causally explained by the electromagnetic properties of unoccupied regions. So according to such theories space-time points are causal agents in the same sense that physical objects are: an alteration of their properties leads to different causal consequences.

24. Earman, *Australasian Journal of Philosophy*, 48, 287–319 (1970); Friedman (Princeton University Press, forthcoming).

25. Nelson Goodman, *Problems and Projects* (Indianapolis: Bobbs-Merill, 1972), Part IV.

26. As the reference to Goodman indicates, I use 'region' in such a way that there is no empty region, i.e. no region containing no space-time points. Also regions don't need to be connected, or measurable, or anything like that: very 'unnatural' collections of points count as regions.

27. This is not to deny that there might be difficulties in figuring out how to axiomatize the 'regular' regions without assuming the existence of the 'irregular' ones. How difficult this task would be presumably depends on the concept of regularity involved.

28. See Richard Montague, 'Set theory and higher order logic', in Crossley and Dummett (Eds), *Formal Systems and Recursive Functions* (Amsterdam: North-Holland, 1965), pp. 131–48, for the sort of second-order axiomatization I have in mind, and a defense of the idea that not only in set theory but elsewhere as well, the way to explicate the idea of a standard model of a first-order theory is as 'model of an associated second-order theory'. As Montague points out, the models of Zermelo–Fraenkel set theory that are 'standard' on this explication are precisely those models that are isomorphic to models in which the domain is the set of all sets of rank less than α for some strongly inaccessible α (greater than ω), and in which '$\in$' is assigned the membership relation restricted to this domain. I agree with Montague that this is the most natural notion of a standard model for set theory.

29. 'What is elementary geometry?', in Hintikka (ed.), *The Philosophy of Mathematics* (London: Oxford University Press, 1969), pp. 164–75.

30. In more detail: recall that conservativeness as I defined it initially is a *semantic* notion, one involving *consequence* rather than *provability*. In the Appendix to Chapter 1, I reformulated it in terms of consistency; this is ambiguous between the semantic and the syntactic, but in referring to some of the arguments as proof-theoretic, and in the way I wrote the proof in note 15, I showed that it was the syntactic notion I was dealing with. The justification for the shift from semantic to syntactic notions is of course the Gödel completeness theorem for first-order logic. In the case of second-order logic there can be no such completeness theorem: here, we must stick to semantic notions throughout. But the key results of the Appendix remain unchanged. In particular, if 'consistent' in (C_0) is understood as 'semantically consistent', the set-theoretic proof of (C_0) is as before: the method described for turning a model of T into a model of $ZFU_{V(T)} + T^*$ can remain unchanged as long as both ZFU and T are second-order theories. (Recall the remarks in note 28 on what the models of second-order set theory are like.) Analogously, the proof in note 15 that (C_1) follows from the consistency of ZF needs no alteration when T and ZF are made second order, except that since we're replacing syntactic consistency by semantic consistency, the step involving the Gödel completeness theorem is unnecessary. (Two less central results of the Appendix are more problematic: the proofs via the Robinson theorem (which is not valid in second-order logic) and Weinstein's proof that the ω-consistency of ZF suffices for (C_2). But these results are not required for the remarks in the text to be true.)

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