# WHY NUMBERS CAN BELIEVABLY BE : A REPLY TO HARTRY FIELD

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## I

Contemporary English speaking philosophy of mathematics moves among a small number of fundamental, highly interrelated questions. But despite the interrelations there are some priorities. Perhaps most basic is the question

(1) Should pure mathematical statements be appraised in terms of truth and falsity at all, and, if so, in terms of what specific notions of truth and falsity ?

Formalism of the kind which Frege attacked held that pure mathematical statements have no specific content apt to contribute towards determination of truth-value. The later Wittgenstein held, by contrast, that the content they have is – at least in a large class of cases – specific enough, but ought not to be viewed as *judgmental*; the role of such "statements" is better assimilated to that of imperatives, or rules governing the use of the concepts which they feature. Opposed to these tendencies stands the whole gamut of opinions, from platonism through intuitionism and various other varieties of constructivism down to finitism and, sideways, to nominalism, which are at least united in their conviction that truth, in some substantial sense, supplies a standard appropriate to the appraisal of pure mathematical statements.

Where such views are not united is in response to

(2) Are pure mathematical statements true, when truth is so substantially conceived ?

One important kind of nominalism, recently championed by Field (1), holds that the answer should be negative : while we succeed in conferring upon pure mathematical statements such a content as is apt to render them substantially true or false, the fact is that they are all (2) false. Our pure mathematical convictions are based on massive error ; there are no entities which have the characteristics which their truth would require. Field's position is in some respects akin to John Mackie's view of moral judgement (3) : we talk in all respects as if there were mathematical entities, or moral qualities, and the truth of what we say requires that there should be ; but the reality is that the world is empty of entities and qualities of the appropriate kind.

Someone who answers both (1) and (2) affirmatively, however, now owes an answer to

(3) What makes pure mathematical statements true ?

More tendentiously, to what kind of states of affairs do pure mathematical statements correspond ? The traditional platonist answer is that the truth-conditions of pure mathematical statements are constituted by the properties of certain mind-independent abstract objects, the proper objects of mathematical reflection and study. The original mathematical Intuitionists, by contrast, held that pure mathematical statements are answerable not to mind-independent objects of any kind but to mathematical constructions, viewed as mental objects, to be investigated in the medium of a non-classical logic which properly reflects their constructed character. A third response, whose germ is in Dedekind and which has vividly been expounded in an influential article of Benacerraf (4), tries to save the *realism* implicit in the platonist response while jettisoning the

(1) See Hartry FIELD (1980), *Science Without Numbers*, Blackwell. Also his (1982) "Realism and Anti-Realism about Mathematics", *Philosophical Topics* 13, 45-69 ; (1984a), "Is Mathematical Knowledge Just Logical Knowledge ?", *Philosophical Review* 93, 509-52 ; (1984b), Critical Notice of Crispin WRIGHT : *Frege's Conception of Numbers as Objects, Canadian Journal of Philosophy* 14, 637-62 ; (1985), "On Conservativeness and Incompleteness", *Journal of Philosophy* 83, 239-60. Also, since the completion of the present paper, "Realism, Mathematics and Modality", *Philosophical Topics* 16 (1988), no. 1., 57-109. (Note however the aberrant remark at line 10 of Field (1980), viii).

(2) Or, if the principal operator is the universal quantifier, vacuously true. I shall ignore this qualification in what follows.

(3) See J. L. MACKIE (1977), *Ethics – Inventing Right and Wrong*, Penguin.

(4) Paul BENACERRAF (1965), "What numbers could not be", *Philosophical Review* 74, 47-73 ; compare Hilary PUTNAM (1967), "Mathematics without foundations", *Journal of Philosophy* 67, 5-22, and Field (1988).

commitment to abstract objects. On this view the truth-conditions of pure mathematical statements are constituted in the characteristics of certain structural concepts. Arithmetic for instance, rather than being the science of the natural numbers, conceived as certain determinate objects, is the general science of *progressions* — the body of theory which explores the features which any array of objects would exhibit which had the collective structure which the "natural numbers" are standardly taken to have.

On this approach pure mathematical statements are all implicitly general and hypothetical, and accepting them carries no specific ontological commitments in its train. But it is not clear whether this can be a stable view. For we have at least to grasp the structural concept ; to know, i.e., what it *would* be for there to be objects which collectively exemplified it. And the fact is that, for the larger part of classical pure mathematics, there is no question of any array of *concrete objects* exemplifying the relevant kinds of structure. This is because the domains of the intended interpretations of the classical theories of real and complex numbers, for instance, and of set theories such as that of Zermelo-Frankel, are uncountably infinite ([5]). Benacerraf's proposal is naturally viewed as an attempt to save something perceived as worthwhile in the platonist conception whilst allowing that there is justice in the nominalistic reproaches which it incurs. But the traditional nominalist view is that it is no mere contingency that there are no abstract objects, — talk of abstract objects is unintelligible ([6]). The stricture, if it is just, would apply equally to the counterfactual kind of involvement with abstract objects to which Benacerraf's position, if it is not to be revisionary of classical mathematics, is committed. At any rate a fourth distinct response to question (3) is that of the traditional nominalist : if the theses — axioms and theorems — of a pure mathematical theory are intelligible, it is because that theory has a model in a (possible) purely concrete domain ; and for such a thesis to be true is for it to be verified in all such (possible) concrete models ([7]).

(5) It merits remark that at least one nominalistically-inclined philosopher disputes that only abstract objects can constitute an uncountable domain. See FIELD (1980), chapter 4, on space-time points. But it is another question (a) whether Field's attitude to space-time points reflects a genuine distinction with which a nominalist should feel comfortable ; (b) whether the mathematical functions defined on real numbers can be represented in a domain of space-time points without invocation of further, presumably abstract objects — *distances*, for instance.

(6) *Locus classics*: N. GOODMAN and W. V. QUINE (1947), "Steps toward a Constructive Nominalism", *Journal of Symbolic Logic* 12, 105-22.

(7) I am ignoring the awkwardness which nominalism encounters in trying to explain these constraints on intelligibility and truth without quantifying over possibilia.

Naturally, answers to question (3) are subject to appraisal in the light of how well they can be made to dovetail with

(4) How may the true statements of pure mathematics be known to be true ?

Some platonists, for instance, have tended to opt for special intellectual faculties, sensitively attuned to the denizens of the abstract mathematical world ([8]). Others have tried to explain how statements concerning such objects can be known to be true without appeal to any special faculties save those involved in a priori knowledge generally: Frege's logicism is a classic example of a response of the second sort. But a quite different kind of platonism is possible, in the spirit of the empiricist conception of mathematics espoused by Mill. Mill thought that true arithmetical statements, in particular, do no more than encode very well supported inductive generalisations – generalisations whose degree of empirical support is so great, indeed, that we find it difficult, in the simplest cases, even to conceive of their falsity. Mill's empiricism involved interpreting the content of arithmetical statements in such a way that they would directly record general features of the ordinary material objects to which we apply arithmetic in everyday life. For Quine ([9]), and Putnam ([10]), in conrast, while the ultimate warrant for belief in the truth of mathematical theories is provided by their empirically successful participation within physical theory, the content of mathematical statements does indeed pertain, as it appears, to abstract objects of various sorts. For Quine and Putnam, the authority for mathematical beliefs, where they are rational, is, as for Mill, ultimately an empirical authority; but, in contrast with Mill, the beliefs are not beliefs about empirical objects. Field has put on record his conviction that the Quine-Putnam case for Platonism is the only one worth answering ([11]).

Benacerraf's attempt to combine pure mathematical realism with ontological neutralism would seem not to prejudice a choice between a priorist and a posteriorist accounts of mathematical knowledge; and the

(8) See e.g. P. MADDY (1980, "Perception and Mathematical Intuition", *Philosophical Review* 89, 163-96. Also perhaps K. GÖDEL (1963), "What is Cantor's Continuum Problem", reprinted in P. BENACERRAF and H. PUTNAM, eds., *Philosophy of Mathematics: Selected Readings*, 2nd edition, Cambridge, 1983, 470-85.

(9) See e.g. W. V. O. QUINE (1960), *Word and Object*, M.I.T. Press, § 55.

(10) H. PUTNAM (1971), *The Philosophy of Logic*, Allen & Unwin, chapters V-VIII.

(11) Field (1980), 4-5.

same choice is open, I think, for one who sympathises with the views of the original intuitionistic writers or with the kind of reconstructive nominalism into which, I suggested a moment ago, Benacerraf's position might slide. But something which is wanted from all hands is an account of the nature of *proof*: of how it is an instrment of knowledge, and of the high degree of certainty which, it is usually supposed, a good proof bestows upon its conclusion. The traditional platonist conception of proof is that it is a mere cognitive auxiliary whereby finite minds may, with sufficient ingenuity, grasp the characteristics of infinite domains. When a proof is available, it is no doubt necessary that the state of affairs which confers truth on the statement proved is one which can be recognised to obtain. But it is also held to be intelligible how such a state of affairs might obtain without the attendant possibility of proof. What confers truth on Goldbach's conjecture ([12]), for instance, is how things stand with each of the infinitely many even numbers. A proof of the conjecture may be possible. But there is no evident reason – on this view – why, if the conjecture is true of all even numbers, that it is so should be demonstrable in some finitely appreciable way. But the question

(5) Can truth transcend proof in pure mathematics ?,

has been one of the most actively debated in recent philosophy of mathematics. Dummett, in particular, has argued the intuitionistic case for a negative answer in a new and powerful way ([13]), drawing on general considerations from the philosophical theory of meaning. A positive answer goes naturally with the idea of proof as a medium of conceptual discovery ; but among those who have favoured a negative answer some, like Dummett himself, seem not to have wanted to challenge such a conception of proof, while others – notably the later Wittgenstein – have given a negative answer precisely by way of expression of such a challenge.

Those who wish to assign to pure mathematics its own special truth-conferring subject matter have to confront, finally, the question

(6) How is it possible to apply mathematics to statements which concern ordinary things, and how does the credibility which attaches to a pure mathematical statement as a result of proof carry over to its application ?

(12) That every even number is the sum of two prime numbers.

(13) *Locus classics*: M. Dummett (1975), "The Philosophical Basis of Intuitionistic Logic", reprinted in Benacerraf and Putnam, *op. cit.*, n. 8, 97-129.

The question of explaining the practical utility of pure mathematics also arises, of course, for those who give negative answers to questions (1) or (2). One attraction of the later Wittgenstein's proposal, that the theses of pure mathematics incorporate rules governing the application in contexts outside pure mathematics of the concepts which they involve is its promise of a rather straightforward response to the question. The most obvious difficulty with it, though, is that many of the concepts of classical mathematics, par excellence those involved in the arithmetic of transfinite numbers ([14]), have no such application. On the face of it, anyway, it cannot be of the essence of pure mathematics to supply such rules.

The problem of application may seem especially difficult for someone who, like Field, gives an affirmative answer to question (1) and a negative answer to question (2), so that large parts of classical mathematics are seen as *false* under their intended interpretation. Why should such massively erroneous theories be so massively useful ? Field's response is highly ingenious, and will occupy us below. But regarding classical pure mathematical theories as, in a large class of cases, false does not really add to the difficulties of question (6). For such theories, whether true or false, do not seem directly to concern the kinds of ordinary things to which they may be so fruitfully applied. Why is it of any use to theorise, whether truly or falsely, about the properties of a special range of (putative) objects, the natural numbers for instance, if one's purpose is to predict the arithmetical behaviour of pieces of fruit ? Mill was wrong to think that, as ordinarily understood, arithmetical statements merely embody very well-entrenched empirical generalisations about what happens when ordinary things are counted, aggregated, divided into separate groups, and so on. But how do the laws of number-theory, conceived as the abstract science of a special range of objects, or of progressions-in-general, have – as it appears – something to tell us about which such generalisations can be expected to be true ? A satisfactory response has to explain how the impression of a dislocation between the proper subject matter of pure mathematics and its applications is somehow misleading : the content of mathematical theories, even when platonistically construed, has somehow to be disclosed as germane to the practical purposes to which they are put. One familiar kind

(14) I do not know whether the applications, in physics, e.g., of the classical theory of the real numbers make full and essential use of the concepts involved in its intended interpretation – indeed, whether physical theory needs to recognise *real* (rather than merely rational) numbers at all.

of response has been the attempt to disclose mathematical objects as *idealisations* in relevant respects of the concrete things to which mathematics is applied ([15]). A brilliant example of a different approach was Frege's analysis of arithmetical concepts in the *Grundlagen*, which has the effect – if you accept it – of simultaneously disclosing both what pure number-theory is about and how its laws apply to non-mathematical objects ([16]).

(15) An impressive recent example is Philip KITCHER (1983), *The Nature of Mathematical Knowledge*, Oxford ; see especially chapter 6.

(16) The point is not easy to illustrate succinctly, but I can perhaps convey a very general idea of the response to the problem of the application of arithmetical laws which Frege makes possible. The key ingredients are two. First, there is the insight that statements of number are *second-level*, like statements of existence : to say that the cage has seven lions in it is not like saying that the cage has brown lions in it – a remark about the characteristics of the individual lions – but is to predicate having seven instances of the *concept*, lion-in-the-cage. Second, there is the perception that this can be reconciled with a straightforward construal of arithmetic, as concerning a special range of objects, by introducing a singular-term forming operator, "the number of ...", and taking the numbers, 0, 1, 2 ... etc., to be the referents of expressions resulting from applying this operator to expressions standing for concepts (e.g. "the number of lions in the cage"). Frege proceeds by defining the individual numbers by applying the numerical operator to a series of prototypically zero-, singly-, doubly-, ... etc., instantiated concepts. Thus

$0 = Nx : x \neq x$ - the number of things which are not self-identical
$1 = Nx : x = 0$
$2 = Nx : x = 0 \vee x = 1$
$3 = Nx : x = 0 \vee x = 1 \vee x = 2$

and so on, each number being defined by applying the numerical operator to the concept under which all and only its predecessors fall. It would take us too far afield to review Frege's construal of addition and how the standard recursive definition

$x + 0 = x$
x + the successor of y = the successor of (x + y)

may be derived from it. But the perspective which his constructions place on the problem of application can be well enough illustrated if we allow ourselves a direct definition of "+ 1" in terms of successor, and consider the equality

$2 + 1 = 3$

Frege defines "y is the successor of x" as

$(\exists F)\ (y = Nz : Fz\ \&\ (\exists w)\ (Fw\ \&\ x = Nv : (Fv\ \&\ v \neq w))$

It is easy to show that 3 is the successor of 2, and hence that $2 + 1 = 3$, by appeal to this definition and those of the individual numbers above. (It is necessary to appeal also Frege's definition of numerical identity – see WRIGHT, *op. cit.*, note 19, 104-7). So we have

(a) $(\exists F)\ (3 = Nz : Fz\ \&\ (\exists w)\ (Fw\ \&\ 2 = Nv : (Fv\ \&\ v \neq w)))$

as a theorem of pure number-theory.

How do we proceed to illuminate the application of (a) ? Well, what does "2 + 1 = 3", when it is applied, *say*? Roughly : no matter what concept, F, you consider, there are

The above is not intended as an exhaustive catalogue of issues in the philosophy of mathematics. But it does cover a good sweep of the ground, and brings out, I would claim, the close-knit, organic nature of the main problems. It is hard to foresee real progress with any of these issues if we do not simultaneously make ground with them all ; and it is unwise to take a stand on any of them before one has tested the commitments on the others which it would enjoin. Someone who takes this breadth of philosophical responsibility seriously is likely repeatedly to find that what seem to be attractive proposals for certain branches of mathematics become much more problematic when applied to others. An example, noted above, was Wittgenstein's conception of pure mathematical statements as normative with respect to non-pure mathematical discourse. Similar struggles confront someone who wishes to extend Benacerraf's "structuralism" to account for set-theory, for instance. Such difficulties tend to elicit two quite antithetical forms of temperament. One inclines in such straits to query the philosophy ([17]) ; the other is prepared, at least ultimately, to allow philosophy to question mathematics. Personally, I see little to commend the former view : mathematics is not a monolith of certainty but, as Wittgenstein stressed, a *motley*, and mathematicians are hardly less likely to seduce themselves, on occasion, by nonsense and confusion than are cosmologists, psychologists and social scientists.

A second unmistakable moral even of so brief a survey of the central questions, and as striking as their interrelations, is the extent to which properly considered answers must draw on work from other areas of philosophy, most especially the philosophy of language and the theory of knowledge. The sort of negative answer to question (1) favoured by Wittgenstein, for instance, calls for support from a general account of the

exactly three F's if and only if, if you consider all but one, you consider exactly two. That is – using the standard notation for numerically definite quantifiers –

(b) $(\forall F)\ ((\exists_3 x)\ Fx \leftrightarrow (\forall z)\ (Fz \leftrightarrow (\exists_2 y)\ (Fy\ \&\ y \neq z)))$

So the problem of explaining the application of (a) is that of showing how it entails (b). Given that Frege can show that, for every n,

$n = Nx : Fx \leftrightarrow (\exists_n x)\ Fx$

the matter reduces to showing how (b) is a consequence of

(a*) $(\exists F)\ ((\exists_3 x) Fx\ \&\ (\exists z)\ (Fz\ \&\ (\exists_2 y)\ (Fy\ \&\ y \neq z)))$

– a not very difficult derivation in second-order logic with identity.

(17) Thus Putnam, *op. cit.*, n. 4, first section. Compare John P. Burgess (1984), "Dummett's Case for Intuitionism", *History and Philosophy of Logic* 5, 177-94, section 1.

distinction between genuinely assertoric and merely "quasi-assertoric" ([18]) or "projective" ([19]) discourse, an account which must be fully investigated in its application to other areas in which philosophers have found attractions in such views – ethics, aesthetics and theoretical science, for instance. A positive answer to question (2) has to be backed by a positive mathematical epistemology which speaks to question (4), and our assessment of such an epistemology has to be constrained by its capacity to deliver a satisfactory response to question (6) and by its capacity to participate in a generally satisfactory theory of knowledge. As emphasised, the issues raised by question (5) about the relationship between truth and proof in mathematics have to be approached by consideration of the generalisation of such issues in the theory of meaning at large. And a platonist response to question (3) requires that it be appropriate to regard mathematical singular terms as instruments of genuine reference, and thus can be appraised only in the context of the general issues concerning reference which arise in the philosophy of language. The popular objection to platonism posed by the causal theory of reference is one product of the perception of such interdependencies.

These four examples – it would be easy to add to them – bring home that a commitment of any of the six questions highlighted is, more than a commitment on other central questions in the philosophy of mathematics, almost inevitably a bet on the proper resolution of basic questions in other areas of philosophy. Conversely, the philosophy even of relatively elementary branches of mathematics is a discipline in which theses and theories about the nature of language and language mastery, knowledge, reference and truth may be sharply focused and tested with clarity. It is, in this way, the fundamental character of the philosophy of mathematics which, in my view, makes it an especially rewarding and important area of philosophical enquiry.

In what follows, I want to focus on a particular illustration of philosophy of mathematics done in this spirit : an attempt to bring general ideas about the nature of singular termhood and reference to bear on the issue between mathematical platonism and its opponents. The argument in question took centre stage in my *Frege's Conception of Numbers as*

(18) See M. Dummett (1973), *Frege : Philosophy of Language*, Duckworth, chapter 10, especially 354-9.

(19) See S. W. Blackburn (1984), *Spreading the Word*, Oxford, chapter 5, § 6, and chapter 6, *passim*.

*Objects* [20], – (henceforward *Frege's Conception*) – but I attribute it to Frege –it is at least implicit in his way of proceeding in *Grundlagen* – and it is to be found in various of Dummett's writings [21]. The argument is deepened and strengthened in Hale's recent book [22]. It seems to me by far the best hope for a straightforward platonistic construal of large portions of classical mathematics, and I shall attempt to establish a case for such confidence by responding to some criticisms of it recently canvassed by Field [23], and by mounting something of a counter-attack. I should re-emphasise, though, that such an argument has no place before the first two questions in the six cited above are answered affirmatively. My own view is, to put it somewhat crudely, that abstract objects as such probably need be no philosophical bother. But – in response to question (1) – I am sceptical whether pure mathematical statements generally engage with a sufficiently substantial notion of truth to impose the full array of traditional mathematical objects upon us by the route I shall describe. My present view, in the briefest possible terms, is that pure mathematical truth is intelligible only in so far as it can be disclosed by proof; that what distinguishes a proof from, say, an experiment is the obtaining of certain internal relations – relations of conceptual necessity – between its basis, process and outcome; and that our ratification of internal relations is something of which a non-cognitivist account is appropriate [24]. But it is not just pure mathematics which poses the problem – ordinary language is replete with apparent references to mathematical and other abstract objects. And it is important, if true, that affirmative answers to questions (1) and (2) may have a fairly straightforward platonism at their disposal when it comes to answering question (3).

## II

Frege's belief that numbers are objects is not to be dismissed as a technicality. It is the belief that numbers are objects in what is (or ought

(20) C. Wright (1983), *Frege's Conception of Numbers as Objects*, Aberdeen University Press. Second edition, Blackwell, forthcoming.

(21) See e.g. Dummett's *op. cit.*, n. 18, 494-8. But Dummett is unhappy with the argument; see Wright, *op. cit.*, n. 20, 64 and following for references and discussion of Dummett's reservations.

(22) R. Hale (1987), *Abstract Objects*, Blackwell.

(23) Field (1984b).

(24) See C. Wright (1986), "Inventing Logical Necessity" in J. Butterfield, ed., *Language, Ming and Logic*, Cambridge, 187-209.

to be) the ordinary understanding of the term, and it is the product of a deceptively simple train of thought. Objects are what singular terms, in their most basic use, are apt to stand for. And they succeed in doing so when, so used, they feature in true statements. Certain sorts of expression, for instance the standard decimal numerals, and expressions formed by applying the numerical operator, "the number of ...", to a predicate, are used as singular terms in the pure and applied arithmetical statements of identity and predication in which they feature. Many such statements are true. So such terms do have reference, and their reference is to objects.

The basic idea is that a reference is, as it were, imposed on a singular term by its occurrence in true contexts of an appropriate kind. It will be agreed, I imagine, that identity statements in which the term in question is one of the related terms, and predications in which it is the subject term, are of the "appropriate kind" to subserve this thought. So the argument must succeed unless *either* the apparent singular terms of arithmetic do not really function as such *or* the apparently true "appropriate" contexts in which they feature are not really true.

Clearly, the notion of a singular term appealed to by the argument must not in the first instance be explained by appeal to the idea of reference to objects. The *semantic* function of a singular term is – if successfully discharged – so to refer. But the argument requires this to be a consequence of a classification formulated differently. In *Frege's Conception* I made a case, following on the discussions of Dummett and Hale ([25]), for thinking that singular terms can be characterised by syntactic criteria. Very roughly, singular terms are marked off from others by the inferential liaisons of the statements in which they occur. The issue is, in fact, a fairly intricate one, but I shall be giving it no further attention here. Rather, I shall assume that such an account is available and that, by its lights, a large class of numerical expressions, no less than proper names standing for persons, towns and rivers, and a large class of definite descriptions and demonstratives, qualify as singular terms.

The syntactic criteria for singular termhood come into play, of course, only for expressions whose use is already established. We shall want to ask, for instance, whether numerical expressions do indeed feature in genuine identity statements, (identified as such by proof-theoretic crite-

(25) DUMMETT, *op. cit.* n. 18, chapter 4 ; R. HALE (1979), "Strawson, Geach and Dummett on Singular Terms and Predicates", *Synthese* 42, 275-95. See in addition HALE, *op. cit.* n. 22, chapter 2.

ria), and whether they are so used as to sustain (first-order) existential generalisation. If a given class of expressions pass such tests, the question must then arise how the use of the contexts in question is established. The "deceptively simple" route to platonism requires that the use of such contexts has been established in such a way that we can indeed reasonably claim to recognise certain of them to be true. The essence of the arithmetical logicism proposed in *Grundlagen* was that the use of arithmetical singular terms can be established by a programme first of contextual and then of explicit definitions of arithmetical vocabulary by means of logical vocabulary, a programme which, if successfully executed, would establish beyond doubt the epistemological pedigree of the basic laws of arithmetic.

As is well-known, Frege's version of this programme turned out to be based on an incoherent notion of *extension of a concept* (or *course-of values*). One of the principal claims of *Frege's Conception* was that that is not the end of the matter. Logic may still provide the basis of an explanation of the concept of natural number from which the basic laws – the Peano axioms – follow. But the arithmetical case is complicated by the fact that, unless we follow Frege and presume ourselves to have certain "logical objects" at our disposal, there is no hope of so defining arithmetical vocabulary that we can exhaustively eliminate it in use. Something less rigorous than eliminative definition is therefore all that can be demanded of the explanations which a workable version of arithmetical logicism has to provide. Rather than risk distraction by a group of rather subtle questions which now loom ([26]), let us follow Frege's example and concentrate, for the moment, on a simple case where the use of the controversial contexts can indeed be fully established by means of a programme of contextual definition.

The example is that of *direction* ([27]). Suppose we have a first-order language containing, inter alia, a range of names, "a" "b" "c" ..., standing for members of a domain of straight lines (which are to be conceived, for these purposes, as concrete inscriptions), and a range of predicates and relations defined on straight lines, including the relation, "... is parallel to ...". We proceed to introduce a singular term forming operator on names of lines, "D( )", and a series of contextual definitions as follows :

(26) See WRIGHT, *op. cit.*, n. 20, chapter 4, and n. 8 on pp. 180-4, for an outline and discussion of some of the principal issues.

(27) G. FREGE, *Grundlagen der Arithmetik*, §§ 64-7. Translated by J. L. AUSTIN as *The Foundations of Arithmetic*, Blackwell, 1959.

1. Any sentence of the form "$D(a_1) = D(a_2)$" is true if and only if "$a_1$" and "$a_2$" are names of lines and the lines they denote are parallel.

A range of other kinds of open sentence, "φ[ ]", completable by direction-terms, are then inroduced by reference to established predicates and relations on lines in accordance with the schema

2. "φD(x)" is true if and only if "Fx" is true, where "... is parallel to ..." is a congruence for "F[ ]".

Finally we stipulate that

3. "(∃x)φx" is true if and only if "(∃x)Fx" is true, where "φ", and "F" are as stipulated under 2.

The effect of these stipulations is that we establish a simple language game of directions in which direction-terms satisfy any reasonable syntactic criteria for singular termhood. The claim of the sort of platonism with which we are concerned is then two-fold. First, there is no sense in which, despite their satisfaction of these criteria, direction terms might fail to be genuine – semantic – singular terms ; second, that the contextual equivalences of statements in which they feature with statements of a (purportedly) unproblematic kind about lines embody a satisfactory account of how statements involving such terms may be known to be true and, hence, how knowledge of the existence of directions, and of their properties, is possible.

The argument is open, accordingly, to two different kinds of challenge. One challenge will dispute, in effect, that syntactic singular termhood suffices for semantic singular termhood : an expression can pass as a singular term, by the syntactic criteria, without importing a specific ontological commitment into the truth-conditions of sentences in which it occurs. A familiar train of thought to this effect is that of the *ontological reductionism* criticised in *Frege's Conception.* The ontological reductionist contends that this is the fate of the direction-terms introduced by the above equivalences, and is shown to be so by the very equivalence of sentences in which they feature to sentences in which no reference to directions is made. The obvious reply ([28]) is that this way of looking at the equivalences presupposes that it is proper to take only their right-hand

(28) First made by W. P. Alston (1958), "Ontological Commitments", *Philosophical Studies* 9, 8-17.

sides at face value, which is just what the platonist disputes. But a reductionist reading of such equivalences could be enforced if it turned out that reference to directions, qua abstract objects, is impossible since reference is essentially a *causally* constrained relation. An objection in the same spirit would be that reference, properly construed, requires *identifying knowledge* of the referent, and that such knowledge, too, is causally constrained. A third line of objection, advanced by Dummett ([29]), is that it is proper to regard a (syntactic) singular term as genuinely referential only if the notion of reference plays an essential part in establishing its use – a part which it cannot play if that use is established by contextual definition.

The reductionist tendency will, of course, typically manifest itself not in response to a proposed *introduction* of a class of singular terms, but in a programme for rendering untroublesome a range of already established contexts in which such (purportedly troublesome) singular terms occur. Still, one way or another, reductionism has to earn the right to read such equivalences in the manner it prefers. The platonist strategy, by contrast, will be to argue that there is no good cause to endow the equivalences with such a significance – that, in particular, the objection of Dummett noted, and the objections issuing from causalist accounts of knowledge and reference, are misconceived ([30]) – and that no good distinction can be drawn between an expression's functioning as a singular term according to syntactic criteria and its being appropriate to construe its semantics referentially.

Platonism and reductionism are united in their acceptance of the equivalences. They are also united in the belief that it is by reference to the epistemology of the right-hand sides, as ordinarily understood, that an account should proceed of how it is possible to know the left-hand sides. But the platonist sees the situation as constituting an explanation of how thought of and reference to abstract objects is unproblematic – at least for one who has no reservations about the content and knowability of the right-hand sides ; whereas the reductionist regards the situation as de-

(29) See n. 21.

(30) The objections raised by the causal theories of knowledge and of reference and Dummett's objection, are discussed in my *op. cit.*, n. 20, chapter 2, sections xi, xii, and x respectively. For a very sophisticated treatment of the causality objections which advances the debate to a point where, in my view, their defeat is firmly in prospect, see HALE, *op. cit.*, n. 22, chapters 4, 6 and 7.

monstrating, rather, how the comprehending use of the left-hand sides need involve neither thought of nor reference to abstract objects.

Set against both these views is (what I shall call) the *rejectionist* response. The rejectionist rejects the equivalences. He denies, that is, that any such equivalences can be uncovered by correct analysis of contexts already established in the language ; and he denies that it is legitimate even to stipulate that such equivalences obtain by way of attempted explanation of the use of statements which purportedly involve reference to abstract objects. The second denial, in particular, may seem hard to comprehend : how can it be illicit to stipulate that the use of one class of contexts is to coincide with that of another, well-understood class ? The answer is that the statements on the left-hand sides are not being regarded as utterly unstructured : rejectionism agrees with platonism that expressions of the form "$D(a_n)$", for instance, will meet the syntactic criteria for singular termhood if stipulation of the equivalences is allowed ; and is further agreed that no distinction is to be drawn between functioning by syntactic criteria as a singular term and importing commitment to an object. Rejectionism is thus the only outlet for an anti-platonist who judges that platonism wins its dispute with reductionism. Field is a rejectionist ([31]).

## III

The immediate question is : what good *motive* is there for the rejectionist response ? It has one, rather superficial attraction. Both the platonist and the reductionist are committed to regarding the overt grammar of statements on one side or other of the equivalences as misleading. For the reductionist, what is misleading is the appearance of reference to abstract objects, fostered by the grammar of the left-hand sides ; for the platonist, what is misleading is the appearance of neutrality with respect to the existence of e.g. directions, fostered by the grammar of the right-hand sides. The rejectionist, in contrast, is free to construe statements of each of the two kinds at face-value ; of neither need the surface grammar be regarded as deceptive. But little weight should be attached to this. Such a face-value construal is sometimes definitely wrong. "Hilary is a brother" is equivalent to "Hilary is male and there is someone of whom Hilary is a sibling", for instance. The apparently simple predication conceals a

(31) Field (1984b), 651.

quantifier. It is true that the equivalence in this case is uncontroversial, and that there is no new kind of entity which is apparently being referred to or quantified over. But that the equivalences which concern us are controversial is hardly an independent reason to reject them. And an equally good example of a concealed quantification would be "Hilary is a tenant" where the implicit quantification *is* over objects of a different sort to the subject of the sentence.

Field's rejectionism is a product of his nominalism : flat disbelief in the objects which the truth of sentences on the left-hand sides would call for. But why accept that such entities *would* be called for by the truth of those sentences – why not rest content with the reductionist response ? Here it is crucial to recognize the very restrictive character of that response. For the syntactic criteria of singular termhood are going to incorporate a reference to the permissibility of existential generalisation ; at least, they are going to incorporate a reference to the permissibility of inference to what is in fact existential generalisation on occurrences of the relevant terms, even if it is not described as such in the formulation of the criteria, which are framed, rather, in terms specific to a particular language ([32]). Moreover, the resulting existential generalisations are going to be *true*, as judged by the criteria incorporated in the equivalences, whenever the original statements are true. So how is it possible to regard the use of the terms in question as existentially non-committal, to regard them as singular terms in a merely syntactic sense ? Existential neutrality requires that what *appear* to be existentially quantified statements – those on the left-hand sides of the equivalences that come under clause 3 above for the language game of directions, for instance – are not really so : they contain an expression which looks like the existential quantifier or the English expression "something", for instance, but in reality fails to have the appropriate content. That is what the reductionist has to say. And since no further account of the meaning of such pseudo-quantifiers is provided – we know only that sentences containing them have the same truth-conditions as their right-hand side equivalents – the reductionist has no

(32) It would arguably involve a crude circularity to appeal directly to the notion of existential generalisation – see WRIGHT, *op. cit.*, n. 20, 58-9. (For discussion of the consequent worry that the attempt to characterise singular termhood syntactically will infect the notion with an undesirable relativity, see my *loc. cit.* 62-4 and HALE (1984), "Frege's Platonism" in C. WRIGHT, ed., *Frege : Tradition and Influence*, Blackwell, 40-56. Cf. HALE, *op. cit.*, n. 22, chapter 2, section III).

defence against the claim that he is committed to holding that the left-hand sides serve merely as notational variants of the right-hand sides with no determinate internal syntax – that it is illegitimate to regard what appears to be the standard first-order apparatus of quantification and identity, featuring in the statements on the left-hand sides, as being really that. Thus the reductionist reponse to such equivalences slides inevitably into what I called in *Frege's Conception* the "austere reading". For there is no more sense in the idea of an expression which behaves just like a singular term but imports no reference than there is in the idea of an expression which behaves just like the existential quantifier but says nothing about existence ([33]).

Field (rightly) expresses scepticism about the prospects of reductionism actually delivering the sought-for equivalences in the case of the mathematical theories which are his primary concern ([34]). But the foregoing reflections show that any anti-platonist who is prepared to go beyond the austere reading of the left-hand sides must reject reductionism in any case, whatever its technical prospects. Frege thought that stipulation (1) above introduces us to the concept of direction ; but it can do so only if we assume that "=" and the occurrences of the line-denoting terms embedded within the direction operators on the left-hand sides have their customary meaning. Once, in the grip of the austere reading, we drop any presuppositions about even the syntax of the left-hand side's, no such assumption is in order. So if reductionism has no option in the end but to insist on an austere reading of such equivalences, then it is in no position to allow that they serve to introduce new concepts. Conversely : anyone who regards the austere reading as inadequate as an account of the contents of the left-hand sides, can only be platonist or rejectionist.

Field's conception of the meaning of mathematical language makes no place for the austere reading ([35]). The reductive equivalences embody, in his view, a genuine *theory* every bit as much as the fundamental laws of

(33) Development of this thought is, I believe, the proper reply to Dummett's objection that the notion of reference – or at least a "realist" conception of reference – is inapplicable to contextually defined abstract singular terms ; see my *op. cit.*, n. 20, chapter 2, sextion x. Compare Hale, *op. cit.*, n. 22, chapter 7, section I.

(34) Field (1984b), 642.

(35) For instance, as Bob Hale drew to my attention, it is essential, e.g. to the kind of instrumental explanation of the utility of number theory illustrated in Field (1980), chapter 2 (the "Aardvarks and Bugs' example) that the syntax of the set-theory invoked be taken at face value. (Field also presents this example in his (1982), 53-5).

physics embody a genuine theory about the entities with which they are concerned. Such fundamental laws are subject to empirical revision, but they also embody our concept of what, e.g. an electron is – for there is nothing else to determine that concept. It is along these lines that Field proposes to regard the reductive equivalences for the language game of directions as fixing the concept of direction : they embody a theory about direction, and the concept has no standing except as embodied in this theory. But the theory can be – and Field believes is – false. It is false because there are no such objects.

The question is therefore : *why* does Field believe that there are no such objects ? Quine and Goodman in their famous nominalist "manifesto" wrote of nominalism as a :

> philosophical intuition that cannot be justified by appeal to anything more ultimate ([36])

This establishes a somewhat distinguished tradition of nominalistic inexplicitness and Field seems happy, for the most part, to belong to it. But it will not do. When so much of our discourse – scientific, mathematical, and informal – is peppered with apparent references to abstract objects, and not regarded as especially problematic on that account, there is a definite onus on a philosopher who thinks there is a problem to give reasons for his view. Some bad reasons – misguided versions of empiricism, misinterpretation of the requirements of less misguided versions, and muddles about the role of ostensive definition, for instance – were exposed in *Frege's Conception* ([37]). But – and this is a second crucial consideration if a satisfactory perspective on these matters is to be attained – there is good reason to think that the more sophisticated sort of considerations to do with, for instance, causality and identifying thought, or the role played by reference in explaining the use of certain apparent singular terms, which were referred to above cannot support rejectionism ; rather, they tend, if sustained, to give reason not to regard the controversial "singular terms" as being genuinely such at all.

To elaborate. Field is content to regard the direction-equivalences as establishing the use of a class of genuine singular terms – terms which purport to refer to objects of an understood kind. They fail, in his view, so to refer. But could they so much as *purport* to refer – import existential

(36) GOODMAN and QUINE, *loc. cit.*, n. 6, 105.
(37) WRIGHT, *op. cit.*, n. 20, chapter 1, sections vii and viii.

commitment – if the proper analysis of the notion of reference necessarily excluded reference to abstract (acausal) entities ? It is natural to think that if, as on Field's view is so, we derive an understanding of a theory of directions from the equivalences, we must have some conception of what it would be for the theory to be true, and thereby for the singular terms it contains to succeed in referring to the things to which they purport to refer. Can we understand that – if such reference is necessarily impossible ?

I am not, of course, denying that it is possible to have some understanding of a conceptual or a priori impossibility. Anyone who understands an undecided mathematical conjecture may be in that situation. But an account is always owing of what such understanding consists in, and in the present case it is exceedingly difficult to see how such an account might proceed. The problem, simply, is how to represent the *content* of the alleged illusion to which someone succumbs who follows the equivalences and believes that direction terms, for instance, function as semantic singular terms. Such a person may or may not also believe that such terms do indeed successfully refer, but that is not the present point. What they must believe, it seems, is that there is a sortal concept associated with such terms, and that they grasp and can identify this concept. Is this belief also to be dismissed as illusory ? If not, then there is no problem about reporting e.g. that Jones (falsely) believes that "D(a)" has the semantic role of reference to a direction ; his belief is necessarily false – according to the kind of view we are considering – but there is a genuine sortal concept, direction, which is at the service of the description of its content. The trouble is that it is utterly unclear what grasping this genuinely sortal concept culd consist in when it is *necessarily* divorced from any capacity of identifying reference to its instances (if any). If, on the other hand, it is claimed that there is no such concept – that the sortality of *direction* cannot survive, as it were, if direction-terms cannot play the semantic role of singular terms – what notion do we have by means of which to describe the content of the illusory belief ?

There is, accordingly, a strong suggestion that any view which holds that the idea of reference to abstract objects is a kind of solecism, for reasons central to the concept of reference, abrogates the means to construe the belief that any particular class of purported abstract singular terms do indeed have that semantic role. But the essence of rejectionism is that the equivalences fail precisely *because* statements on the left-hand side make existential demands which are not made by the statements on the right-hand side. So rejectionism holds that the apparent abstract singular terms on the left-hand sides are indeed functioning as such. It follows that rejectionism cannot be soundly motivated by the view that reference is necessarily causally constrained, or any view about the conceptual nature of reference which excludes abstract objects from its range. A proper outlet for such views may be reductionism ; or dismissal of discourse putatively involving reference to abstract objects as unintelligible ; or old-style nominalistic reconstrual of such discourse. But the thesis that such discourse, platonistically construed, is *false* is not a proper outlet.

Field does at one point make it clear that his scepticism about abstract objects is driven ([38]), at least in part ([39]), by the kind of orthodox worries about causality which, if the foregoing argument is good, ought to drive him in a different direction. But it remains unclear what better motive might be available to him. I believe it will emerge that he can have none, through the reasons for saying so will be fully in place only at the end of the paper.

## IV

Field's programme is designed, as he makes clear, to address head-on the argument of Quine, Putnam, and others, that a belief in the existence of mathematical objects is warranted because reference to and quantification over such objects is an indispensable part both of successful physical theory and of the mathematics which such theory utilises. Field describes this argument as the

> "one and only one serious argument for the existence of mathematical entities" ([40])

(38) Field (1982), 59 and n. 14 on p. 68.

(39) He also attaches importance to Benacerraf's objection in BENACERRAF, *op. cit.*, n. 4. I have argued that Benacerraf succeeded in demonstrating only that numerical singular terms, like all singular terms, are prey to arguments for inscrutability of reference (WRIGHT, *op. cit.*, n. 20, chapter 3, section xv). That is still my opinion, after some skirmishing in the literature. (See e.g. M. MCGINN, "Wright's Reply to Benacerraf", *Analysis* 44, 69-72 G. SPINKS, "McGinn on Benacerraf, *ibid.*, 197-8 ; A. HAZEN, "McGinn's Reply to Wright's Reply to Benacerraf", *Analysis* 45, 59-61). For a remarkable attempt to meet Benacerraf's argument head-on – i.e. to resolve the inscrutability – see HALE, *op. cit.*, n. 22, chapter 8.

(40) Field (1980), 5.

and later speaks of it as

> "the only available argument for Platonism" ([41])

That it certainly isn't. In particular Field is passing over, in these remarks, any suggestion that mathematical statements involving quantification over and reference to abstract objects, might be known *a priori.* The central argument of *Frege's Conception,* though limited in scope to the natural numbers, was exactly to that effect.

Field's programme divides into two. First, he needs to show that physical theories can be satisfactorily stated without reference to or quantification over abstract entities, that is, satisfactorily stated *nominalistically.* Second, that endorsing the mathematics used in physical theory incorporates no separate commitment to abstract objects. The traditional response among anti-platonistically inclined philosophers to the second project has been the attempt to interpret such mathematics at face-value but in a concrete domain, or to provide some kind of systematic reductive paraphrases of the putatively exceptionable quantifications and singular terms. What is most distinctive about Field's approach is its departure from this tradition. His claim is rather that the mathematics which physical theory needs to use is *conservative* with respect to nominalistically-stated theories :

> "any inference from nominalistic premises to a nominalistic conclusion that can be made with the help of mathematics could be made (usually more longwindedly) without it ([42]).

If this is true, it follows that the success of science in no way rebounds to the credit of the mathematical theories which are its instruments ; it could all be done, at least in principle, without using any mathematical theories whose truth would require the existence of abstract objects. Field's intended effect is thus to prise apart reason to accept mathematical theories, as utilised in science, and reason to believe them to be true.

In what follows I shall concentrate exclusively on the second part of Field's programme, and try to develop an important line of thought originally due to Hale ([43]). First, though, reflect that it is immediate from Field's account of conservativeness that consistent mathematics which was

(41) Field, *ibid.*
(42) Field (1980), x.
(43) HALE, *op. cit.*, n. 22, chapter 5.

not conservative could play an essential part in determining the testable consequences of a physical theory to which it was harnessed, and hence that the success of that theory could rub off on the mathematics, as it were. So Field needs to offer positive, nominalistically acceptable reason for thinking that the relevant parts of classical mathematics are conservative – otherwise he provides someone who wishes to reject platonism with no response to the Quine/Putnam argument. Field is well aware of this, of course, and has plenty to say about it, but nothing he says on the matter resolves, it seems, a rather serious basic question. Believing a mathematical theory to be conservative involves, presumably, believing it to be consistent. What, for Field, is the content of this belief in consistency – or relatedly, the content of the concept of *consequence* explicitly utilised in the account of conservativeness ? There has been some discussion of the matter in the literature following Field's book ([44]), pivoting on the dilemma that standard accounts of both notions, whether proof-theoretic or semantic, equally involve quantification over nominalistically uncongenial entities – *sequences* of sentences in the case of the proof-theoretic notions, and *models* in the case of the semantic ones. Field's response ([45]) is that both notions – consistency and consequence – are to be interpreted, nominalistically, in a primitively modal way. That is : a statement is a conequence of others just in case it is not possible for it to be false while they are all true, and is inconsistent with others just in case its negation is a consequence of them.

It would be a bad tactic, I think, to object to Field's appeal to modal notions which are primitive in the requisite sense, i.e. which are not explained by recourse to quantification over abstract entities of any sort. One may or may not feel that modal notions cry out for explication and cannot decently be taken as primitive, but the issue is obviously very difficult. What is clear is that nominalism is something of a dead weight on the prospects of such explication. (It is hardly likely that Field would prefer to take modal notions as primitive if he saw any promising strategy for explicating them nominalistically). But note, in any case, the light in which Field's primitive modalism places the belief in the conservativeness of e.g. number theory or analysis. One who believes in the conservative-

(44) See e.g. S. Shapiro (1983), "Conservativeness and Incompleteness", *Journal of Philosophy* 81, 521-31, and Field (1985).

(45) Field (1985), 241. Compare his (1982), 65-7, and (1984a), 514-7.

ness of those theories, and so in their consistency, is committed to holding that their axioms are *possibly collectively true*. Under what circumstances ?

A possible nominalistic answer could be that certain *material* (or otherwise nominalistically acceptable) objects might be such that the sentences of the theories in question were true when interpreted so as to involve only quantification over and reference to those objects. But Field would not want to answer along these lines. For one thing he is quite explicit – and gives himself, it seems to me, good reason for holding – that the primitive modal operators should function *within* the language rather than metalinguistically ([46]). To claim that the axioms of number theory are possibly true is thus, for Field, to make a claim about what, as standardly meant, they say. For another, if the belief in consistency were interpreted (metalinguistically) as proposed, it would amount to nothing other than the belief in the feasibility of the *traditional* nominalist programme – the programme of providing concrete (or otherwise nominalistically acceptable) reinterpretations of classical mathematical theories – from which Field explicitly wants to distance himself. The whole point of the proposed play with conservativeness was to avoid the labour, and the hostages held out to (a priori) fortune, which the traditional nominalist programme entails. The legitimacy of having recourse to mathematical theories whose truth would apparently demand the existence of abstract objects was to be founded not on a laborious demonstration that the existence of such objects was indeed not a presupposition of such theories' truth but on a demonstration that their role in mathematical theory was in principle only one of convenience, that their utility could be explained without bringing in the idea of their truth at all.

It thus emerges that Field's recourse to primitively modal conceptions of consequence and consistency, coupled with his belief in the conservativeness and hence consistency of number theory, analysis, and other central theories of classical mathematics, and his belief in the falsity of those theories, commits him to the thesis that their truth, when they are interpreted as classically intended, is a matter of *contingency*. There are not, but might have been natural numbers. There are not, but might have been real numbers exactly as classical analysis describes. Is this coherent ? It is certainly a far cry from the traditional nominalist thesis that talk of abstract objects is simply unintelligible. But although I know of nowhere

(46) Field (1984a), 515.

where Field explicitly accepts this somewhat startling conception, it would not be surprising if he did so. For it is broadly consonant with his conception of e.g. the direction-equivalences as constituting a *theory*, as establishing a concept which can fail to apply. Nevertheless, I believe that, unless he can somehow avoid it ([47]), this upshot is the Achilles heel of Field's position. The obvious question is : what space has he left himself for justifying the claim that there are indeed e.g. no natural numbers, that number theory *is* contingently false ? I shall return to the matter in section VII.

## V

I turn now to Field's response to the central argument of *Frege's Conception*, displayed in his critical study. As noted, Field is happy to accept that the notion of a singular term can be characterised syntactically, along the lines attempted in *Frege's Conception*, and that in the light of that characterisation numerical expressions do indeed function as singular terms in arithmetical statements, whose truth demands, accordingly, the existence of numbers as objects. His question is : what reason do we have to think that any such statements are true ?

Field makes heavy weather of discerning the response of *frege's Conception* to this question. He quotes ([48]) the following passage :

> Frege requires that there is no possibility that we might discard the preconceptions inbuilt into the syntax of our arithmetical language, and, the scales having dropped from our eyes, as it were, find that in reality there are no natural numbers, that in our old way of speaking we had not succeeded in referring to anything. Rather, it has to be the case that when it has been established, by the sort of syntactic criteria sketched, that a given class of terms are functioning as singular terms, and when it has been verified that certain appropriate sentences containing them are, by ordinary criteria, true, then it follows that those terms do genuinely refer. And being singular terms, their reference will be to objects. There is to be no further, intelligible question whether such terms *really* have a reference, whether there really are such objects ([49]).

(47) See note 66 below.
(48) Field (1984b), 644.
(49) WRIGHT, *op. cit.*, n. 22, 14.

Field responds

> The kicker [sic] here is the phrase "by ordinary criteria" ...

and proceeds to suggest, in effect, that the argument reduces to the claims that to function as a singular term by the syntactic criteria sketched in *Frege's Conception* is to function as a genuine (semantic) singular term — which he accepts — and

> (S) what is true according to ordinary criteria really is true, and any doubts that this is so are vacuous ([50])

Field proceeds to raise the obvious kind of query about (S) : did the "ordinary criteria" for truth in ancient Greece make "Zeus is throwing thunderbolts" true whenever there was lightning ? But the impression that such a question is pertinent is owing — as Field knows — to a bad formulation of the Fregean argument. The passage he quotes could no doubt have been clearer ; but to say that a statement has, by ordinary criteria, been verified to be true is not the same as saying that it has been verified to be true by ordinary criteria. The difference is just that the former is, whereas the latter need not be, *factive* — it implies that the statement has indeed been verified *tout court.* What I intended to endorse by the inclusion, in the quoted passage, of the phrase "by ordinary criteria" was not the principle (S) ; it was rather that, whenever a statement is associated with certain *canonical* grounds, i.e. grounds such that to suppose that the statement is true is a commitment to the availability of such grounds for believing it to be true ([51]), it is senseless to ask more of a candidate confirmation of the statement than what is provided by the obtaining of such grounds. The satisfaction of canonical grounds for a statement may not preclude a sceptical doubt about its truth, since such grounds are often in principle defeasible, depending on the kind of statement concerned, by additional information. And the sceptic may want to dispute, besides, whether a particular kind of state of affairs whose obtaining is guaranteed by the truth of disputed statement is indeed a *ground* for that statement at all. But the present point is only that lightning is not, presumably, a canonical ground for Zeus's ballistic extravagances ; whereas, for example, one line's being parallel to another is a canonical

(50) Field (1984b), 646.
(51) Cf. my (1982) "Strict Finitism", *Synthese* 51, 203-82, 211.

ground for the identity of their directions and is, indeed, in the platonist view, conceptually sufficient for that identity.

This is the crux of the matter. Field is prepared to allow, as noted, that the direction-equivalences have a concept-fixing role. They constitute a theory in which a concept of direction is embodied, as contemporary physics constitutes a theory in which the concept of an electron is embodied. More than that, he is prepared to allow that the conditionalisation of those equivalences on the supposition that directions do indeed exist *does* result in conceptual truths ; it is a conceptual truth, for example, that if directions exist, then the directions of two lines are identical just in case those lines are parallel. But he insists that the concept so explained can – and actually does – apply to nothing. The theory can be – and is – false. He rejects the claim that the parallelism of two lines suffices a priori for the identity of their directions ; what we can know a priori is only that it so suffices if they *have* directions. And he will take a parallel stance on the corresponding equivalences which, for instance, a logicist of the sort adumbrated in *Frege's Conception* will base his account of number theory on, and of all similar attempts to construe talk of abstract objects, mathematical or otherwise. The relevant kind of equivalences do indeed have an explanatory status ; but it is an explanatory status which allows the possibility of their falsity.

In *Frege's Conception* I formulated a dilemma for anyone who would doubt the existence of any species of abstract object whose covering sortal concept is explained along the lines of the Fregean paradigm illustrated by direction ([52]). The dilemma was simple : if, in accordance with the explanation, it is accepted that the obtaining of the relevant equivalence relation among items of the relevant, previously familiar kind suffices for identity under the new concept, then the reflexivity of that relation guarantees that the new sortal concept is instantiated. If "a is parallel to b" is accepted as sufficient, in accordance with the explanation of direction, for the truth of "The direction of a is identical with the direction of b", then the parallelism of every line to itself guarantees the self-identity of the associated direction and hence its existence. If, on the other hand, the obtaining of the relevant equivalence relation is not accepted as sufficient for identity under the new concept, then – since that was an integral part of the explanation of that concept – no doubt about the

(52) Wright, *op. cit.*, n. 20, 148-52.

existence of that sort of thing can be intelligibly entertained ; for there is no concept in terms of which to formulate the doubt. But now it might seem as though Field's position allows him to dodge the horns of this dilemma : to insist both that parallelism does not suffice for identity of direction and that he does have a concept of direction – given by the equivalences – in terms of which to formulate the doubt about their existence. I think that this is an illusion, and that the dilemma is good. Reflect that in rejecting the platonist claim that the equivalences are true purely in virtue of their explanatory status, Field has thrown out certain proposed sufficient conditions for the truth of the claim that directions exist. But he has accepted that we understand the "theory" of directions, and know what it would be for directions to exist – and indeed that it is a conceptual truth that if directions exist, their identities and other characteristics may be determined in accordance with the equivalences. So we understand what it is for directions to exist although we have no sufficient condition or weaker ground on which we can in practice rely in order to determine whether or not they do. It would, no doubt, be pointless to charge Field that he owes an alternative account of what would suffice for the existence of directions ; he will reply that he is under no obligation to provide such an account, that what would suffice for the existence of directions is only and precisely that. One is inclined to press that, failing some such alternative account, it is quite unclear what it can be to *understand* the hypothesis that directions exist, which features in the conditionalised equivalences which Field accepts. But Field will stonewall again, replying that that is simply the hypothesis that direction theory is true – a hypothesis which anyone who derives as much explanatory content from the direction equivalences as is legitimate will understand well enough. One feels the position is deeply unsatisfactory, but how is it to be assailed ?

Two different questions have emerged for our attention. First, there is the question whether Field suceeds in disclosing any incoherence in the platonist reading of the relevant kind of equivalences, or any other reason for regarding that reading is impermissible. Even if Field's position has the resources to defeat its critics, and his programme of nominalistic interpretation of physical theory can indeed be carried through, he cannot avoid the attribution of massive error to great sweeps of contemporary mathematical theory. Platonism carries no such implication. So unless Field can show that it is actually illicit to treat the relevant kind of equivalences as, in the platonist manner, explanatory truths, his programme must inevitably seem less attractive than the platonist programme. (Of course, a platonist programme for mathematics of ambitions of corresponding extent to Field's may go wrong further down the line ([53])).

The second question is whether the view of the direction equivalences as embodying a false theory, and, more generally, of mathematical theories as, platonistically construed, merely contingently false, can withstand scrutiny. I suggested earlier that there is a serious, possibly fatal weakness hereabouts, but the claim has yet to be made good. I shall attempt to do so in the final section of the paper, devoting the next to the first of these questions.

## VI

It is in section 4 of his critical study of *Frege's Conception* that Field presents his objections to the Fregean species of platonism in general and the platonist treatment of the equivalences in particular. These seem to be two. The first is that it is not clear how, in contrast with its epistemologically more exotic relatives, Fregean platonism really can avoid the postulation of special intuitive or quasi-perceptual faculties, sensitive to abstract objects and their properties and relations. Second, Field is deeply suspicious of what he describes as

> The idea that the existence of objects can flow from an explanation of concepts ... The idea that the existence of numbers flows from the explanation of the concept of number is reminiscent of the ontological argument for the existence of God, according to which it follows from the very concept of God that God exists ([54]).

His presentation of the first of these objections, and indeed much of the earlier part of his critical study, is gratuitously complicated by the presence of a character he calls the "ontological inflationist". The ontological inflationist is a kind of obverse of the reductionist who holds that the

(53) Remember that the sort of platonism that simply reverses the direction of significance of reductionist equivalences can ultimately fare no better than reductionism. The crucial questions concern the capacity of Fregean platonism to avoid dependence on such equivalences in developing an account of mathematics. The sort of logicism canvassed in my *op. cit.*, n. 20 is one strategy, but there is no discussion in that book of its prospects beyond arithmetic. Some exciting possibilities are raised, however, by George Boolos (1987a), "Saving Frege from Contradiction", *Proceedings of the Aristotelian Society* 87, 137-51.

(54) Field (1984b), 659-60.

direction-terms occurring on the left-hand sides of the direction equivalences are not genuine singular terms : the inflationist, for his part, holds that the *line-terms* occurring on the right-hand side are not genuine singular terms. Field seems to introduce this position only in order to make a meal of distinguishing it from Fregean platonism, although he realises perfectly well that it is not available to someone who endorses the syntactic account of singular termhood on which Fregean platonism depends. Nevertheless the "inflationist" and the platonist do have this in common : they agree that the existence of directions follows from the truth of statements on the right-hand sides. But if – and this is the objection – the claim that line *a* is parallel to line *b*, no less than the claim that the direction of *a* is identical to the direction of *b*, presupposes the existence of directions, why does the problematic epistemology of direction not come across, as it were, to infect ordinary statements about lines and their properties and relations as well ? No doubt the epistemology of the right-hand sides is in fact relatively unproblematic ; but it is so – the objection continues – only because such statements are standardly taken *not* to import any claims about abstract objects ; as soon as they are conceived as the platonist wishes, it is no longer clear that the routine verification-procedures associated with them suffice, and the appearance of unproblematic epistemology disappears.

The platonist claim is that statements on the right-hand sides entail that directions exist. The objection is that if they do, they inherit the epistemological difficulties associated with the existence of abstract objects. And the immediate reply should be that this is a *non-sequitur* : it cannot *always* be true that the consequences of a statement must be verified independently before that statement may be regarded as known – if it were, advancement of knowledge by inference would be impossible. Sometimes it is true ; we might not be able to verify that Kim is a brother without first verifying that Kim is male, for instance. But no reason has been produced for regarding the present kind of case as coming into that category, and it is reasonable to expect that none will be which does not beg the question against the platonist. For the platonist view, to stress, is that the concept of direction is *established* by reference to these equivalences ; it is therefore out of the question that previously accepted verification-procedures for the right-hand sides could suddenly cease to be adequate as a result of their being saddled with a new kind of implication. Our very understanding of these implications, the platonist will insist, is dependent upon our fixing the concept of direction in such a way that statements about directions are verified by the *very same procedures* which we antecedently took to verify the corresponding statements about lines. To suppose that the ordinary procedures suffice for verification of the right-hand statements if but only if they are not construed as entailing the existence of directions is to assume – not argue – that there is no sortal concept which can be constituted by the stipulation that the equivalences, under the ordinary understanding of their right-hand sides, are to hold.

Field proceeds to anticipate, via some skirmishes concerning whether or not the left-hand statements may be regarded as following *logically* from the right-hand statements, more or less this reply, and moves to his second objection, the mystery of the existence of objects "flowing" from the explanation of concepts. He does not say, but does nothing to discourage his reader from thinking, that the Ontological Argument for the existence of God may stand or fall with platonism of the kind under consideration. "Does Wright have anything to say", he in effect enquires, "which makes Platonism look any more respectable than this suggestion would imply" ([55]) ?

Well, yes I do. It needs emphasis, to begin with, that – with a qualification I shall make below – it is no part of Fregean platonism to regard the existence of the relevant species of abstract objects as entailed just by the way in which their covering sortal is explained. It is not, for instance, the way the above concept of direction is explained – via stipulation of the equivalences – which entails that there are such things as directions, but those stipulations *together with* the truth of appropriate statements apt to feature on the right-hand sides. What does follow from the explanation is that any one of a relevant class of straight lines has a direction ; but the existence of directions is contingent on the existence of members of that class. So the gap between such cases and the uncomfortable precedent of the Ontological Argument seems adequately broad.

It narrows, of course, when we move back to the case which the analogy with directions was mainly supposed to illuminate, namely the natural numbers. Now concepts replace streight lines and one/one correspondence replaces parallelism. And some of the right-hand statements in the relevant equivalences are theorems of second-order predicate logic with identity. Any concept, including all which may be defined purely logically,

(55) Field (1984b), 660.

is put into one/one correspondence with itself by the relation of identity, for instance. So it is straightforward to prove, via

$(N^{=})$ $\quad Nx : Fx = Nx : Gx \quad \leftrightarrow \quad (\exists R)R$ 1/1 correlates F with G,

that there is, for instance, a number of things which are self-identical, and a number of things which are not. Here the substance of what Field says is, for anyone disposed to accept the version of logicism for which I was arguing in *Frege's Conception*, correct. The existence of numbers follows from the nature of the concept of number – more accurately, from a statement of a canonical explanation of that concept – together with certain truths of logic, just as the existence of directions follows from a canonical statement of the concept of direction together with certain truths about straight lines. But what follows from certain statements together with truths of logic follows from those statements *simpliciter.* So what Field says is true – (or at least it may be true, if some of the difficulties, concerning impredicativity and other matters, which were canvassed in *Frege's Conception* can be satisfactorily met and the logicist's explanations turn out in good order). The existence of numbers, and indeed their satisfaction of the Peano axioms, flows out of the concept of number. But everything here is above board ; why is this not a *congenial* discovery, rather than a cause for complaint about mystery ?

Well, what about the comparison with the Ontological Argument ? An immediate point of disanalogy is that that argument makes no attempt to fix the concept of God by associating the truth-conditions of statements concerning Him with those about any subject matter of an overtly non-theological sort about which we antecedently believe that a priori knowledge is possible. But the main point is that the Ontological Argument is flawed not in its very enterprise – the project of trying to establish an existential conclusion on a basis of conceptual reflection – but in detail. Briefly, since I have no wish to digress on the matter, it founders, inter alia, on the following dilemma. If the putative explanation of the concept of God, "God is that than which nothing greater can be conceived", is represented as a universally quantified statement :

(i) For all x, x is identical with God if and only if no greater being than x can be conceived,

no contradiction follows from adding the supposition that there is no x identical with God. If, on the other hand, the explanation is couched like this :

(ii) God = (ιx) no greater being than x can be conceived,

then it is consistent with assigning this statement a concept-fixing role to allow that it may be false – when no individual satisfies the description on the right-hand side. So, once again, the explanation fails to imply existence.

The latter, of course, is exactly what Field wants to say about the equivalences which, in the logicist programme, constitute the core of the explanation of the concept of number. But I am not at the moment – though I shortly shall be – questioning whether it is coherent to take Field's view of them. The germane point is that Field has to produce some independent argument why it is impermissible to view them in the platonist fashion to which he takes exception. And none seems to be in the offing. If the suggestion is, for instance, that once we reserve the right to stipulate that the numerical equivalences are true, we are powerless to refuse any proponent of the Ontological Argument the right to stipulate that (ii) is true, then the answer is that, whereas the first stipulation does no more than assign truth conditions to statements of numerical identity, some of which it is then possible to show to be realised by accredited methods, the second would be an attempt to stipulate not truth-conditions but truth itself.

I do not, by these remarks, mean to suggest that nothing else is amiss with (many versions of) the Ontological Argument than its inability to the avoid the dilemma mentioned, or that better versions of the argument are not available which do avoid that prticular dilemma. What is clear is that the strategy of the argument has no point of affinity with the Fregean platonist's ontological strategy. An ontological argument which did have such an affinity would proceed by contextual definitions of the truth-conditions of statements concerning God of such a kind that they could be established by the accredited methods associated with the *definientia*. No such argument is possible, one would imagine, for the straightforward reason that the cosmological implications of the *definienda* preclude their having any correct *definientia* with routinely accredited methods of verification.

There is, however, a third line of objection to the equivalences which Field does not explicitly offer. Indeed, it seems to me prima facie more powerful than those he does offer. The reductionist holds that the appearance of singular reference to directions on the left-hand sides of the relevant equivalences is misleading, that the ontological commitments of the left-hand side statements are only and exactly what are suggested by the surface grammar of the right-hand sides. The Fregean platonist holds just the reverse : that the ontological commitments of the right-hand sides are just what are displayed in the surface grammar of the left-hand sides – an ontology of both directions and lines. But that seems to imply that even if we speak exclusively in the vocabulary of the right-hand sides, we nevertheless refer, willy-nilly, to directions as well. Indeed, this would be so even if we had no inkling of the concept of direction and never inroduced direction terminology. For, as I wrote in *Frege's Conception*,

> ... the absence of any *need* to construe natural number as a sortal concept would be completely irrelevant for "ontotaxonomic" purposes. Availability is enough ... The question, what kind of things are there, should not be approached by reference only to the sortal concepts which we need to employ for whatever purposes we happen to have, but by reference to all such concepts which admit of satisfactory explanation ... if a sortal concept of natural number is available, and normal criteria determine that it has instances – that is, contexts of relevant types are true which contain terms purporting to denote natural numbers – then there *are* such things ([56]).

The point is : it *is* a species of Platonism with which we are here concerned. We do not *create* directions, or numbers, or sets by creating sortal concepts of direction, number and set. When a sortal concept is coherently explicable, and statements purporting to involve reference to instances of it are verifiable in the light of canonical grounds associated with the concept, then it does indeed have instances ;and it has them whether we choose to acknowledge the fact or not, or even arrive at any understanding of the concept in the first place. But is there not something absurd about the resulting situation ? For the platonist now has to acknowledge, it seems, that even if we had never arrived at the concept of number, still less done any number-theory, but had utilised for our arithmetical purposes only the vocabulary of second-order predicate logic with identity, we should nevertheless have been referring unwittingly to numbers. And the idea of such a community-wide unwitting reference seems to be in tension with the very notion of reference.

Unintentional reference is, of course, a genuine occurrence, as when a man at a fancy dress party unintentionally refers to his neighbour by use of "that fool dressed up as Mr Punch". But here the unintentional

(56) WRIGHT, *op. cit.*, n. 20, 129.

reference is the by-product of an intended reference to the very same object. A different case is where someone misunderstands a singular term but is nevertheless said to have referred, via use of it, to its proper designation – when, as we say, they do not know whom or what they are speaking about. Such a case might even be one where the speaker had no concept of what they were speaking about – I mean, we might describe it like that. But even that is no analogy for what the platonist needs. If a community speaks only in terms of the vocabulary of the right-hand sides of the direction equivalences, for instance, and is quite innocent of the concept of direction, there need be no expression in use which they do not fully understand. Unintentional reference is a phenomenon of factual misapprehension or imperfect understanding. Neuther rubric covers the present kind of case. The Fregean platonist seems to be committed to the bewildering claim that one can fully understand a class of contexts by which references of a certain kind are effected, yet be unaware that any such references are effected and have no concept of the kind of thing to which reference is being made.

A Platonist might brazenly try to insist that the right-hand statements are indeed only imperfectly understood by subjects who have not grasped their equivalence to the left-hand statements. But it would be an unhappy stance ; for it would remain that such a "misunderstanding" of statements about lines and their properties and relations would be quite consistent with an apparently perfect grasp of the vocabulary of these statements and of their syntax. The proper response to the objection is rather, it seems to me, to point out that it confuses reference with ontological commitment. So far, we have been content to speak of the semantic role of singular terms as essentially one of reference, but now it is time to be more careful. Not every use of what is – by the Fregean's syntactic criteria – a singular term is a referring use ; though every such use is, of course, existentially committing. The contrast is that which Donnellan aimed to draw ([57]) between *referential* and *attributive* uses of definite descriptions. The platonist must hold, undeniably, that an endorsement of appropriate statements about lines, etc., *commits* a subject to the existence of the relevant directions. But that is not the same as saying that they unwittingly refer to the directions to which they are committed. To use a sentence in

(57) Keith S. DONNELLAN (1966), "Reference and Definite Descrptions", *Philosophical Review* 75, 281-304.

such a way that a reference is effected to a particular object, it seems to me, is to use a sentence of which a full understanding, in context, presupposes identifying thought of that object. Since identifying thought involves bringing the thought-of object under some concept or other – and under the relevant covering sortal in the case of abstract objects ([58]) – it follows that the objection is quire right : it would be absurd to regard speakers whose arithmetical discourse was restricted to what could be formulated in second-order predicate logic with identity as unwittingly referring to numbers ([59]). It is also true that, on any plausible account of the matter, the making of a statement which involves reference to a particular object cannot express the same thought as one which does not ; reference is no eliminable aspect of a thought. And it is clear that the platonist will want to regard at least some uses of the left-hand side sentences as involving reference in the strict sense of the term to abstract objects. But what follows is only that platonism must not identify the thoughts respectively expressed by uses of the sentences on the right-hand sides and left-hand sides of the equivalences. And there would, presumably, be no inclination to do that in any case since the conceptual resources involved in grasping the two kinds of thoughts are evidently different. What platonism needs to hold is, first, that statements which have the same truth-conditions may express different thoughts ; second, that if a statement involves reference to an object of a particular sort, we cannot lay it down as a necessary condition for another statement to have the same truth-conditions that it too involves reference to that object – the most we can say is that it must entail that the object in question exists. The first is uncontroversial ; mathematics is especially replete with examples of equivalences where quite different conceptual resources are called on by an understanding of the equivalent statements. Think, for instance, of statements concerning right-angled triangles and their equivalents afforded by Pythagoras Theorem. As for the second, a proper appraisal would have to engage the issues which Donnellan was addressing when he drew the distinction between referential and attributive uses of definite descriptive phrases.

Some of the germane matters are briefly discussed in the Appendix. But a simple example may provide a useful perspective. Someone who knows

(58) In contrast, identifying thought may be possible of demonstrated concrete objects of which one has no idea what sort of thing they are.

(59) This involves repudiating some of the formulations in my *op. cit.*, n. 20. See e.g. p. 32.

that first-cousins are children of siblings and that n + 1th cousins are children of nth cousins may need time to realise that second cousins share two great-grandparents. So such a person, on being told, by way of introduction, that "This girl and you are second-cousins" may not recognise the equivalence of that to "The closest ancestors which you and this girl have in common are great-grandparents of you both". This equivalence, it is arguable, is unimpaired, even if the elderly couple in question are known to the speaker and his use of "The closest ancestors which you two have in common" is referential. But we do not want it to be an implication of that equivalence that such a reference is implicit in the original use of "This girl and you are second-cousins". For one thing, the introducer may have no identifying knowledge of the elderly couple, and so not be in a position to refer to them. For another, it is not clear that grasping the relation, "is a second-cousin of", involves having the concept of an ancestor or indeed a concept of the ancestral of any relation at all.

To revert to Field's discussion. Field draws a distinction between *proving* Platonism and merely defending it against certain epistemological objections, and raises the question whether *Frege's Conception* might succeed it at the second even if not at the first ([60]). His answer, based on the Ontological Argument objection – the "mystery" of objects "flowing" from concepts, etc. – is negative. But the way Field draws the distinction emphasises the most important feature of his view of the platonist project.Field's complaint is not that *Frege's Conception* fails to make a case for platonism by standards acknowledged in that book. Rather, we are not agreed about what "proving" platonism should involve. For Field it is a matter of showing that a relevant *theory* – encompassing, inter alia, any germane equivalences – is true. *Frege's Conception* did not attempt to do that – nothing could do it, as I shall shortly argue. For the Fregean platonist about number, or direction, in contrast, a proof would consist in showing, first, that a genuinely sortal concept of number/direction can be formed by stipulating that the appropriate equivalences are to hold true, or shown to be already in place by demonstrating that such equivalences are analytically true ; and, second, by further straightforward moves consequent on verification of appropriate statements on the right-hand sides. *Frege's Conception* contains no proof of this view, because it did not conclusively establish that the relevant notions are genuinely sortal.

(60) Field (1984b), 661-2.

Establishing that would require seeing off, once and for all, the various objections – based on causalist conceptions of reference, etc. – to the syntactic account of singular termhood, and also meeting certain special objections which are consequent on the second-order character of the concept of number which involves that the explanations of arithmetical vocabulary can only initially take the form of eliminative paraphrases. What I tried to do in *Frege's Conception* was map out the form which a proper defence of Platonism would take, weed out some popular but feeble objections and outline what seemed to me more serious ones, and make at least the beginnings of a case for thinking that they can be answered. If those objections can be answered, then proving platonism about e.g. number is a triviality ([61]). But Field's call for proof is not a call for a demonstration that those objections can be met. As we have seen, he makes nothing of them and seems content to accept number as a sortal concept. What he is calling for is a demonstration that it is legitimate, a fortiori intelligible, to treat the equivalences in the fashion followed by *Frege's Conception* – that it is indeed a feature of the concept of number that the right-hand sides of the appropriate equivalences encode a priori necessary and sufficient conditions for the truth of the left-hand sides. If that is accepted, the distinction between proving platonism and defending it against the usual epistemological objections near enough collapses ; all that the proof requires, in addition, is the truth of appropriate right-hand statements.

As I stressed in *Frege's Conception,* you cannot force someone to accept a concept. What Field needs to deny is that it is *possible* to establish concepts with the characteristics – a priori truth of the relevant equivalences – which the platonist wants. Such a denial can only be based on disclosure of something unintelligible on the route. Field does not accomplish that – his criticisms are all based either on assumption of his own "theoretical" view of the equivalences or on spurious comparisons (Greek mythology, the Ontological Argument, and so on).

There are, alas, no generally accepted ground rules for the adjudication of disputes about what is intelligible. Like David Lewis, I do not know how to refute a profession of incomprehension. But if someone is prepared to accept that, ceteris paribus, such equivalences do indeed establish a concept, so that the question is only whether they should be viewed in the Fregean way or in Field's, then I think it can be shown that the latter is

(61) Cf. n. 30.

ultimately quite unsatisfactory. That will be the burden of the concluding section.

## VII

As we saw in section IV, Field commits himself to holding that number-theory, platonistically interpreted, is at worst contingently false. And since he accepts that a priori conceptual truths are expressed by the results of conditionalising the relevant equivalences on the supposition that numbers exist, that supposition – that numbers exist – has to be the only element of contingency. What number theory says of numbers is necessarily true providing numbers exist. But, contingently, they do not.

Still, under different possible circumstances, the arithmetical platonist would have been right – or so Field must hold. So, as stressed earlier, the obvious question is : what reason does Field have to espouse nominalism in the first place – what evidence is there that these putatively different possible circumstances are not the circumstances that actually obtain ?

It is fair to say, I think, that no-one seriously doubts that number-theory is indeed conservative in Field's sense (though, it is another question whether the reasons for this confidence – the belief that number-theory faithfully reflects an informally but well-understood conceptual structure and hence incorporates a body of necessary truths – are available to Field). But if number-theory is conservative, no nominalistically stateable evidence can bear on whether it is acceptable, since it can play no essential part, by hypothesis, in the deduction of predictions which such evidence confirms or refutes. The "theory" is, accordingly, as Hale emphasises, insulated from all possible nominalistically stateable evidence ; and Field will and should admit as evidence nothing which is not nominalistically stateable. Hence, if Field does have reason to doubt the existence of numbers, it is nothing that has been thrown up by the day-to-day commerce of physical theory. What other kind of reason could there be to doubt the existence of numbers, when it is conceived as a contingency ?

So far as I can see, the only other form such evidence might assume would be as the output of some kind of direct test, if such was possible, informed by some kind of analogue of the perception we have of concrete objects. The idea is doubly strange : it is strange to think that abstract entities might be objects for some sort of quasi-perceptual faculty in any case, and it is additionally strange to think that we might recognise that we had such faculties, despite never sensing any abstract entities, and proceed to accumulate good reason to think – on the basis of a comprehensive but frustrated search ? – that no such things existed. But Field has no time for such putative faculties (62) ; so that cannot be the reason for his nominalism.

It seems, then, that Field has no choice but to admit that he has *no* evidence for his nominalism, and that which way the alleged contingency goes – whether numbers exist or not – is indeed beyond the testimony of any evidence which human beings can gather. It follows that he ought not to be a nominalist but an agnostic. Such a reorientation of his position would involve cancelling a lot of things he actually says, but it might not seem so very important a change : the critique of the Quine/Putnam argument, in particular, would be left entirely intact.

But this will not do. The question is, what *content* attaches to the alleged contingency – what does it mean to say that numbers, for instance, exist and how is it contingent whether they do ? Since the question has already emerged as beyond all humanly possible evidence, philosophers of verificationist sympathies will be deeply suspicious whether a genuine possibility can be involved. But Field will not, presumably, share those sympathies. Nor will he admit the legitimacy of the demand for a further account of what the existence of numbers might consist in. Yet the sense that there is something profoundly unsatisfactory about the position does not depend upon verificationism, at least as ordinarily understood, or on the assumption that it is appropriate to demand some further elucidation of what the existence of numbers would involve (63). Many who would not regard themselves as verificationists would still accept the principle that there is an interdependence between our understanding a certain type of state of affairs – knowing what the obtaining of such a state of affairs would consist in – and our understanding of the kind of cognitive powers which it would take to be sensitive to the obtaining of a state of affairs of that type. The principle is, admittedly, very vague : the relevant cognitive powers may be highly idealised and very imperfectly characterised. It is also non-verificationist insofar as it allows that a certain *type* of state of affairs could pass the test although a being of the appropriate powers would not necessarily be able to detect the obtaining of a *particular* such

(62) See Field (1980), 107, n. 4.
(63) That no such elucidation is in prospect is Hale's main complaint against Field (*op. cit.*, n. 22, chapter 5).

state of affairs. All that is required is that such a being could, in favourable circumstances, detect some states of affairs of that kind. But even this principle, anodyne – indeed virtually contentless – though it seems, is offended by Field's conception of the existence of numbers. For the limitations, noted above, on possible evidence have nothing to do with human nature but are essential. If number theory is conservative with respect to nominalistically stateable consequences, then it is necessarily so ; and no being can accrue nominalistically stateable evidence for or against the existence of numbers. Moreover it is no mere limitation of humanity that we lack abstract-object detective faculties. The idea of a quasi-perceptual cognitive relation with abstract objects has gained a certain respectability from its association with the name of Gödel – an association which is, I suspect, mistaken and is anyway based on only the slenderest textual evidence – and from Maddy's heroic efforts on its behalf. But it seems to me hopeless, and I think Field would agree. Perception is indeed, as Maddy stresses, something which involves conceptualisation and activity of mind. But it also essentially involves a mode of interaction with the object perceived – call it sensing – in which a creature could engage which had the appropriate sensory equipment but no ability of conceptualisation. Where there is no sensing there is no perception either. But no such relation can be made out in the case of abstract objects if, as standardly conceived, they are acausal.

It would be no solution to attempt to discard that view. If abstract objects are, after all, causal, other imponderable questions ensue. What other effects do they have besides figuring in the "quasi-perceptions" of mathematicians and others ? What would it be like to sense an abstract object without knowing what it was ? What is the physics of abstract objects – how do they produce their effects, do they obey the laws of conservation of energy, etc. ? At the root of the whole idea is a flat disregard of what ought to be a common-sense methodological precept : we are justified in ascribing cognitive faculties of a certain kind to a subject – or novel cognitive faculties to ourselves – only if there is independent evidence of a power to detect *what is so* and at least the prospect of an account of how this detection is accomplished. The precept requires that we must *first* know of the existence of abstract objects, and have at least some idea of how we *could* be sensitive to such things, before the postulation of a special faculty – whether toned down by the prefix "quasi" or not – could be justified. Certainly the phenomenon of agreement about axioms, referred to in Gödel's remark ([64]), calls for no such response ; it fails to do so for the same reasons that our finding many of the same things funny does not call for explication in terms of (literally) a *sense* of humour.

Neither direct (non-nominalistically stated) nor indirect (nominalistically-stated) evidence for or against the existence of abstract objects is conceivable when the matter is viewed as by Field. Field's alleged contingency transcends all possible evidence and we have no conception of what it would be for a cognitive subject, however idealised, to have a justified opinion about it. But that is not the worst of it. There is a further, very simple point. Field has no prospect of an account of what the alleged contingency is contingent *on.* This world does not, in Field's view, but might have contained numbers. But there is no explanation of *why* it contains no numbers ; and if it had contained numbers, there would have been no explanation of that either. There are no conditions favourable for the emergence of numbers, and no conditions which prevent their emergence. We have no model of such a contingency and no other illustration of it ; everything we ordinarily think of as existing contingently exists by grace of the obtaining of causally favourable circumstances. But the idea of a contingent state of affairs that is contingent on nothing, admits of no evidence even in principle, and is beyond anything we could justifiably regard as cognition, is simply not a credible piece of metaphysics, and is a sure sign of error, I believe, in the philosophical claims which impel Field towards it.

Field has painted himself into a corner. He wants to allow (1) that the Fregean platonist's equivalences are concept-fixing, but only in the sense in which any theory may establish concepts which may yet fail to apply to the world. He wants – like everyone else – to hold (2) that number-theory is conservative with respect to inferences from nominalistically-stated premises to nominalistically-stated conclusions. And he wants to hold (3) that it is sufficient to characterise the notions of consistency and consequence in primitively modal terms, without quantification over abstract objects of any sort. In addition, of course, he wants to hold (4) that nominalism is correct. What I have argued is that the consequences of (1)-(4) are incredible. (4) and (3) entail that number theory is not but

(64) GÖDEL, *op. cit.*, no. 8, 483-4 : "But, despite their remoteness from sense experience, we do have something like a perception also of the objects of set-theory, as is seen from the fact that the axioms force themselves upon us as being true".

could be true. (1) entails – since, as demonstrated in *Frege's Conception*, the basic axioms of arithmetic follow from the logicist definition of numerical identity – that the only element of contingency in number theory concerns the existence of numbers ; and (2), conjoined with the reflections about "quasi-perception" above, then places this contingency on the bizarrely transcendent plain I have described. Field cannot save the essentials of his position by rejecting either (2) or (4), and would – failing some better nominalistic explication of modal notions – be severely embarrassed to have to reject (3). He can hardly reject (1) by way of an endorsement of Fregean platonism, though he could do so by way of reneguing on the claim that the equivalences do establish a concept of number, or direction, etc. But making that response good would require argument that we cannot understand the left-hand sides if their syntax is taken at face-value. So we should be back with the old idea that what appears to be talk about abstract objects is strictly unintelligible if taken as such, and demands physicalistic or reductive construal if that is at all possible. Once again, what is distinctive about Field's position would be lost.

I have considered the "theoretical" view of the relevant kind of equivalences only in the context of certain other opinions actually held by Field. Might the view be more comfortable for someone else ? The essence of the view is that someone can know all there is to know about the concept of number, e.g., and still have no basis for the belief that numbers exist. But it is Field's commitment to the contingency of the existence of numbers, coupled with his belief in conservativeness (and presumed acceptance of my observations about "quasi-perception"), which generates the difficulties. Belief in conservativeness and rejection of "quasi-perception", seem to me not to be optional. But before we are entitled to believe that the Fregean view of the equivalences is incumbent on anyone who wishes to regard them as playing a concept-fixing role, it is necessary to consider whether the "theoretical" view could not be combined with conceiving of the existence of numbers as an *impossibility*. However a moment's reflection discloses that there is no credible option of this kind. The conception that the existence of numbers is impossible would require – when combined with the view that the equivalences establish a genuine and consistently formulated concept – some supplementary account of what makes for the impossibility in question and what kind of impossibility it is. Such an account would have to allow that – by the "theoretical" view – we know well enough what numbers would *per impossibile* be, but independently demonstrate that there could be no such things. But how could such a demonstration proceed except by drawing on aspects of our concept of number which, by hypothesis, is coherent and encoded in a consistent system ? How indeed could it *be* impossible that there should be such things unless somewhere along the line the concept does implicitly impose impossible demands upon its instances – demands which it should be possible to represent explicitly in such a way as to disrupt the consistency of a full theoretical embodiment of the concept ? Such an embodiment would have to go well beyond Peano arithmetic – which we are assuming is conservative – or the kind of logicist foundational second-order system sketched in *Frege's Conception*, which, as Boolos has recently noted (65), is consistent if classical analysis is. So the view that the "theory" of number necessarily applies to nothing would have to call for some excess content in that "theory" beyond anything contained in the logicist foundation, which would therefore have to suffer from a curious kind of incompleteness. I think we have no notion of what this excess content could be.

In summary : I have contended that Field fails adequately to come to terms with the strongest form of argument for platonism, and that the coherence of his positive views is – in their present formulation at any rate (66) – open to serious question. At the very least, rejectionism and the

(65) G. Boolos (1987b), "The Consistency of Frege's *Foundations of Arithmetic*" – in Judith Jarvis Thomson, ed., *On Being and Saying : Essays for Richard Cartwright*, MIT Press. (Cf. John Burgess' review of *Frege's Conception* in *Philosophical Review* 93, 1984, 638-40.)

(66) It is possible that Field would do better by refusing the connection, on which the argument has depended, between conservativeness and consistency ; i.e. as he construes the latter notion, between conservativeness and possible truth. Michael Dummett expressed the view (in commenting on an earlier draft of this paper) that Field has some room for manoeuvre here. Why should a necessarily false platonistic theory, S, not be conservative with respect to inferences from nominalistically stated premises to nominalistically stated conclusions ? This thought need not depend on selection of a second-order S and appeal to the incompleteness of second-order logic. S might be first-order axiomatisable without inconsistency yet necessarily false under its intended interpretation. (A nominalist who held that there are necessarily no abstract objects would presumably take this view of first-order number-theory).

A while ago I put the same thought to Bob Hale, in commenting on a draft of chapter 5 of his *op. cit.*, n. 22. Belief in the conservativeness of a mathematical theory with respect to nominalistically stated theories, in so far as it is a commitment to belief is its having some consistency property, seems to involve only this :

S is *nominalistically consistent with respect to* N iff any set of nominalistically stated associated "theoretical" view emerge in a highly unattractive light. I conclude that someone who wants to regard the equivalences as playing a legitimate concept-fixing role ought, so long as they are satisfied of the substantial truth of appropriate statements on the right-hand sides, to be a platonist about the relevant kinds of entity. The outstanding issues concern whether, as I believe, such equivalences and other explanatory statements can indeed successfully play such a concept-fixing role ([67]).

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consequences of S + N are (primitively modally) consistent provided N is (primitively modally) consistent.

Hale discusses this proposal in his book and – prescinding from the infelicitous quantification over sets – notes that nominalistic-consistency-with-respect to depends on simple proof theoretic consistency (always provided the conditional of the underlying sentential logic is material). So the problem, for the nominalist, of construing the latter notion remains. I am not sure this point is absolutely conclusive since, *unless* nominalism can successfully construe that notion, it might occur to the nominalist to deny the dependency. Of course, an explanation would then be owing of what reason someone could have to believe that a particular S was nominalistically consistent with respect to some N if such reason had to be independent of belief in proof-theoretic consistency.

However that may be, it is clear that Field could not – if he pursued Dummett's thought – apply his preferred primitively modal account to the notion of *consequence* as it occurs in the characterisation of nominalistic-consistency-with-respect-to-(i.e. impossibility of simultaneous truth of premises and falsity of conclusion) – unless there is indeed the distinction between primitive *logical* possibility and some more general notion of *conceptual* possibility which he proposes (see e.g. Field (1984a), 518-9). And the problem of explicating the specific (alleged) impossibility of the existence of mathematical objects, outlined in the immediately preceding paragraph in the text, would remain in any case.

(67) My thanks to Philip KITCHER for inviting me to contribute to this volume, and to Michael DUMMETT and Bob HALE for helpful comments, at very short notice, on an earlier draft of this paper.

## APPENDIX

### Reference and Existential Commitment

In an *attributive* use a definite description may, according to Donnellan, be analysed as a complex quantification, exactly as Russell proposed. In a *referential* use, the role of the description is to serve in the expression of an object-directed thought : to focus upon a particular individual, presumed to satisfy the description. (See *Frege's Conception*, 99-100 for a brief account). Since one and the same sentence can be used in both these ways, the immediate question is : what is the proper account of the contrast ? Manifestly, since it applies in the case of simple predications, it is not constituted by differences of scope. But even where a context has the complexity to sustain different scope readings, the distinctions cut across eachother. If I say "If the 1984 Presidential Election had gone differently, the President would not have risked arms shipments to Iran", all four cases are possible for "the President" : narrow scope, referential use (reference to Mondale) ; narrow scope, attributive use (I do not know who ran against Reagan) ; wide scope, referential use (reference to Reagan) ; wide scope, attributive use (I do not know who the current (*) U.S. President is, but believe that he had a massive electoral majority and that that tends to encourage irresponsibility in political leaders, and have some general inkling about the nature of the Irangate scandal).

Donnellan himself argued that, when a definite description is used referentially, it is not a necessary condition for the truth of the thought expressed that the object referred to should actually satisfy the descriptive condition expressed by the referring phrase. Thus I may, in certain circumstances, express a true thought by an utterance of the sentence, "The man drinking a Martini is the new Assistant Quaestor", even though the gentleman is drinking Malvern Water. Such a distinction in truth-conditions seems pleasantly palpable, but I do not think it is the right account of the matter.

The plausibility of Donellan's view that, even where there is a misfit between the content of the description and the object referred to, the definite description may nevertheless effect a reference to that object, depends, it seems to me, upon concentration on examples where there is a route from the sense of the descriptive phrase and features of the context to justified conclusions about the speaker's intentions. In other cases, where there is no such route – where someone simply misdescribes the object of which they intend to speak and confusion results – we would not regard the speaker as having succeeded in

(*) At the time of writing.

expressing the intended thought ; and since the predication may very well have been just what was intended, the explanation has to be that they have not succeeded in referring to the intended referent. But it would be undesirable to have the question of whether or not a particular use of a definite description is referential turn on the response of an audience. That would have the effect that the referential status of the description would depend on the audience's having, or acquiring identifying knowledge of the referent ; whereas what matters is the knowledge that the *speaker* has. The distinctions seem to me to cut across eachother : a speaker may be using a description referentially – in the expression of an object-directed thought – although the audience, through no fault of their own, are unable to identify who or what is being referred to. Conversely, an audience may reasonably infer, in context, and given the literal sense of what a speaker says, that such-and-such is the intended referent when there is no intended referent and the speaker is using the descriptive phrase attributively. Donnellan was after a distinction which seems to me to be correct and fundamental. But is is not to be drawn by saying that when the subject term in a statement of the form "The φ is ψ", is used attributively, its truth-condition is that something be uniquely φ and also ψ ; but that when it is used referentially, its truth-condition is that whatever a competent audience would, in the context in which the statement is made, take to be the speaker's intended referent be ψ ([1]).

(1) This is not, I ought to make clear, quite the way Donnellan proposed to draw the distinction. Donnellan himself explains the truth-condition of the referential use as being merely that the object the speaker *intends* to refer to be ψ. Against the accusation of "Humpty-Dumptyism", Donnellan – wisely and correctly – replied that intending is constrained by expectation – a subject may not intend a certain result by a certain action unless they genuinely expect that action to be attended by that result. ("Putting Humpty Dumpty together again" *Philosophical Review*, LXXVII (1968), 203-15, esp. 212ff.). He takes this to require that in order for you to intend to refer to a particular object by using a particular phrase, you must genuinely expect that the audience would understand you to be speaking of that object. But that does not really seem to meet the Humpty Dumpty objection –such an expectation might be genuine enough and nevertheless be quite irrational or even lunatic. (Humpty Dumpty might genuinely have expected Alice telepathically to divine what he had in mind by "glory"). The appropriate next move would seem to be to stipulate that the relevant expectation must be, more than genuine, reasonable in context. But this too seems unsatisfying. The distinction between the object-directed thought expressed by using a definite description referentially in some sentence and the corresponding general thought expressed by using it attributively is a distinction in the *contents* of those thoughts, and should therefore be reflected in a proper account of the meanings which two such utterances would respectively have. And meaning, it is natural to think, has less to do with what it is reasonable to expect by way of uptake from a particular audience than – in any case where the two diverge – with the way a competent audience actually does or would respond. The proposal discussed in the Appendix represents a natural way of trying to take this thought seriously while staying close to the spirit of Donnellan's own account.

The foregoing proposal would have allowed a referential use of "The φ is ψ" to express a truth in circumstances when an attributive use of it would not. It would also allow the converse: someone might, by a referential use of "The φ is ψ" speak a falsehood in circumstances where, had their use been attributive, they would have spoken a truth. Against both it and Donnellan's own proposal (see note 1 to this appendix) I would like to set the orthodoxy that there is a distinction between what a speaker intends to say, how a competent audience will interpret what they say, and what they have strictly and literally said. A speaker may intend to express a certain object-directed thought by use of a particular sentence, and may indeed be understood by a competent audience as intending to convey that thought; but it is a further question whether the words used are strictly and literally apt for the expression of that thought. But if we take this view – if, in particular, we require, against Donnellan, that in order for a use of "The φ is ψ" to succeed in expressing a particular object-directed thought, the object thought about must satisfy the definite description, – what is the right account of the referential/attributive distinction? Can whether a particular use of such a sentence is referential or attributive still affect the truth-conditions of what is thereby said?

It depends what is demanded of identity of truth-conditions. A referential use could express a truth in circumstances where the corresponding attributive use would not only if the referent failed (uniquely) to satisfy the description; and a speaker could fail to express a truth by referential use in circumstances where, had he spoken attributively, he would have spoken the truth only if his referent isn't a φ. So *in all actual circumstances* what is expressed by a (successful) referential use is true if and only if what is expressed by the corresponding attributive use is true. But there is still something worth describing as a truth-conditional distinction. Consider not whether what is actually respectively said by each of the two kinds of use in a particular context could diverge in truth-value in that context, but whether those two thoughts, *identified in that way*, would have the same truth-value in all possible circumstances. (This is not, of course, the same question as whether *what would have been said* in different possible circumtances by tokens of the two kinds of use could have diverged in truth-value). The answer, for a large class of cases, is, no. For what I succeed in saying by a referential use of "The φ is ψ" would not have been true if the referent had been neither φ nor ψ, even if something else had been ψ and uniquely φ. Notice too that this contrast does not depend on the questionable idea that the very statement I make by the successful referential use could be made, albeit falsely, in the counterfactual circumstances. The distinction is still good if we suppose instead, as I believe we should, that the thought actually expressed by the successful referential use is a possible thought if but only if its object is indeed uniquely φ.

Nevertheless we should not be satisfied even with this way of drawing Donnellan's distinction. The reason why not is that the property, φ, may be essential and so inalienable, so that conditionals of the form "If the actual φ had not been φ, ..." invite consideration of no possible state of affairs. Where whatever is the φ is necessarily so, and nothing else in different possible circumstances would have been the φ, the sort of distinction illustrated cannot be drawn. Yet the distinction between referential and attributive applies to uses of such definite descriptions also. For instance – assuming, with Kripke, the necessity of biological origin – everything Donnellan said about "Smith's murderer is insane" could as well have been illustrated with respect to "Your child is a lucky fellow".

The situation is that where φ is alienable, the object-directed thought (expressed by the referential use) and the general thought (expressed by the attributive use) have distinct survival conditions, as it were. This is not, however, the basis of an account of the distinction, which has also to apply to definite descriptive uses of inalienable characteristics. This would not be the place to attempt an improved account, even if I had one to offer. But a suggestive thought is that someone who has identifying knowledge of what is in fact uniquely φ is thereby actually *debarred* from making an attributive use of "The *f* is ψ" unless they somehow fail to believe that that knowledge concerns the φ. What this suggests is that a proper account of the distinction should proceed by reference not to truth-conditions but the background information of the speaker : what they possess in the way of identifying knowledge – and a crucial part of the task, of course, is to characterise that notion – and how that knowledge is related to beliefs of theirs concerning the condition expressed by the descriptive phrase.

The foregoing brief remarks, inconclusive though they are, are favourable to the platonist's needs. The platonist needed identity of the references respectively effected by a pair of statements not to be necessary in order for those statements to share their truth-conditions. For meeting the objection raised in the paper requires the possibility that someone who affirms "The direction of *a* is identical to the direction of β" say something which is true if and only if what is affirmed by an assertion of "*a* is parallel to *b*", is true, even though references are effected by the former utterance which an assertion of the latter would not involve. Since the assertion of the latter does, on the platonist view, entail the existence of the relevant objects and their identity, the platonist can avoid the unwelcome and probably incoherent commitment to "unwitting" reference only if in making a statement which implies the existence of certain objects but does not involve reference to them I may say something which, necessarily, is true if and only if a corresponding statement is true whose author does, in making it, refer to those objects. Now, we have noted that, for a large class of cases – those where the characteristic expressed by the definite description is alienable –, there is, in one good use of the phrase, a difference in truth-conditions between two such statements – the statements respectively expressed by referential and attributive uses of "The φ is ψ". But this does not matter for the purpose at hand. For it is the other concept of identity of truth-conditions which is germane to the platonistic view of the equivalences. Suppose I count out the hazelnuts from a mixed bag of hazelnuts and brazils and can see on finishing that, without counting the latter, there are many fewer of them than the hazelnuts. I assert "The number of hazelnuts" – thereby referring to 57 – "is larger than the number of brazils" – the latter descriptive phrase being used attributively. The platonist, who wants it to be possible to introduce locutions involving reference to numbers by means of the equivalences, is committed to regarding the thought expressed by my utterance as true just in case someone who, in the same context, says "There are more hazelnuts than brazils in that bag" speaks truly, even if they have no idea how many nuts of either kind there are and their remark contains no implicit reference to either number. But in holding that it is necessary that either assertion is true if and only if the other is, there is no commitment to holding that the thought I express would be true in exactly the same *counterfactual* circumstances as the thought expressed by the other speaker. The point is just that noted earlier. My thought is essentially directed upon the number 57 under the description "the number of hazelnuts". If there had been, say, four times as many nuts of each kind in the bag, that thought would not have been true ; but the other speaker's thought would have been true.

The reply to the objection in the text thus runs in two stages. First, there is no special difficulty about the idea that a subject who asserts a right-hand side statement may be committed to the existence of objects of a kind of which they have no concept. It frequently happens that understanding consequences or equivalents of a statement may call on conceptual resources not involved in the understanding of that statement itself. Second, such an assertion may have the same truth-conditions – in the sense germane to the platonist's needs – as an assertion in the same context of the corresponding left-hand side statement by which reference is effected to the objects in question. At any rate, this can be so if, as I have been suggesting, referential and attributive uses of one and the same definite descriptive sentence in the same context likewise share their truth-conditions. (It is irrelevant to this comparison that the existential commitments of an attributive use of a definite description are explicit, whereas the existential commitment e.g. to directions incurred by someone who asserts statements on the right-hand side of the direction equivalences is only implicit). If these thoughts are on the right lines, then it is possible to accept the platonist's equivalences, and to agree that – what is essential to platonism – the left-hand sentences may be used, when so explained, to refer to abstract objects of the relevant kind, without commitment to the absurd idea that someone who has mastered only the right-hand sides and is innocent of the relevant covering sortal, nevertheless unwittingly refers to such objects.