BOOK SYMPOSIUM

The Reason's Proper Study. Essays towards a Neo-Fregean Philosophy of Mathematics

By BOB HALE and CRISPIN WRIGHT
Oxford University Press, 2001. xiv + 456 pp. £35.00

SINGULAR TERMS AND ARITHMETICAL LOGICISM

IAN RUMFITT
University College, Oxford

I

In 1983, Crispin Wright published a short book entitled *Frege's Conception of Numbers as Objects*,[1] in which he attempted to renovate and then to defend the philosophy of arithmetic that Frege had espoused in his *Die Grundlagen der Arithmetik* of 1884.[2] That philosophy distinctively combines arithmetical logicism—the thesis that the basic laws of arithmetic are truths of logic—with the claim that cardinal numbers are objects.[3] Now in the actual derivations of (the versions of) the second-order Peano postulates which Frege gave in his later work, the *Grundgesetze der Arithmetik* of 1893 and 1903[4], the objects which he took cardinal numbers to be were value-ranges or *Wertverläufe*, and Russell's

1. Aberdeen University Press, 1983.
2. Gottlob Frege, *Die Grundlagen der Arithmetik: eine logisch mathematische Untersuchungen über den Begriff der Zahl* (Koebner, 1884), henceforth cited as *Gl.* The English translation by J.L. Austin, published under the title *The Foundations of Arithmetic: a logico-mathematical enquiry into the concept of number* (second revised edition, Blackwell, 1980), reproduces the pagination of the original German edition.
3. On the conception of logical truth that prevails today, these two doctrines are uncomfortable bedfellows. If cardinal numbers are objects, then the Peano postulates will entail the existence of infinitely many objects. Accordingly, if the Peano postulates are logical truths, there will be logical truths that entail the existence of infinitely many objects. That conflicts with the standard modern conception of logical truth, whereby a necessary condition for a proposition to be logically true is that it should be true no matter what domain of objects any first-order variables in it might range over. Since Hale and Wright do not undertake to defend logicism *au pied de la lettre*, they do not address the question of how the notion of logical truth might be articulated so as to permit the reconciliation of these doctrines. See, however, §VI below.
4. Gottlob Frege, *Grundgesetze der Arithmetik* I/II (Pohle, 1893/1903).

paradox shows Frege's theory of value-ranges to be inconsistent. Wright was struck, however, by an observation of Geach's:[5] when he sketched derivations of the Peano postulates in *Die Grundlagen* Frege did not invoke value-ranges. Instead, he proceeded directly from the principle (now called 'Hume's principle') that the number of *F*s is identical with the number of *G*s if and only if there is a one-to-one correspondence between the *F*s and the *G*s. Wright showed in detail how Frege's informal sketch could be converted into an actual proof of the Peano postulates directly from Hume's principle. More exactly, he considered a second-order system (now called 'Frege arithmetic') whose only non-logical primitive expression is a term-forming operator on one-place predicates which is intended to mean 'the number of', and whose axioms are precisely those of axiomatic second-order logic together with Hume's principle. And he showed how 'number', 'zero', and 'successor' may be defined in the language of Frege arithmetic in such a way that the second-order Peano postulates are derivable in that system from those definitions. He conjectured that Frege arithmetic, unlike the Fregean theory of value-ranges, is consistent.

Wright went on to suggest that the derivability of the Peano postulates within Frege arithmetic rehabilitates the kernel of Frege's early philosophy of arithmetic. For although Wright himself did not (and does not) wish to classify Hume's principle as a logical truth, he claimed (and continues to claim) that it is *analytic* inasmuch as it can be used "to *explain*, if not exactly to define, the general notion of identity of cardinal number" (p. 279).[6] He also claimed, and claims, that the principle can serve as a foundation for arithmetic through being "available without significant epistemological presupposition to one who has mastery of the other concepts [i.e. the concepts other than *number*] that it configures" (*ibid.*). Since the Peano postulates may be deduced from it, he went on to argue, all the theorems of axiomatic second-order Peano arithmetic may be known through logical deduction from an analytic truth—more particularly, from an abstraction principle "whose right-hand side deploys only logical notions". Accordingly, as he now puts it,

> always provided that concept-formation by abstraction is accepted, there will be an a priori route from a mastery of second-order logic to a full understanding and grasp of the truth of the fundamental laws of arithmetic. Such an epistemological result, it seems to me, would be an outcome still worth describing as logicism, albeit rather different from the conventional, more stringent understanding of the term. (p. 280)

5. "The main results we have so far considered [which include Frege's way of generating the infinite series of finite numbers] seem to me solidly established. We have still to discuss Frege's view that numbers are classes—extensions of concepts. He himself attached only secondary importance to this (Sec.107); rejection of it would ruin the symbolic structure of his *Grundgesetze*, but not shake the foundations of arithmetic laid down in the *Grundlagen*" (Peter Geach, 'Review of Frege's *Foundations of Arithmetic*, translated by J.L. Austin', *The Philosophical Review* 55 (1951), pp. 535–44; reprinted under the title 'Frege's *Grundlagen*' in Geach, *Logic Matters* (Blackwell, 1972), pp. 212–22; quotation at p. 219 of the reprint).
6. Unadorned page numbers will throughout refer to the volume under review.

So, even if Frege was wrong to classify the basic laws of arithmetic as logical truths, he was right to think that our knowledge of those laws could be explained as exercises of the intellectual faculty that he called our "logical source of knowledge" (*logische Erkenntnisquelle*),[7] so long as that faculty is conceived sufficiently broadly that its deliverances can include analytic truths as well as strictly logical ones.

Wright's attempt to rehabilitate Frege's philosophy of arithmetic has, deservedly, received as much attention as any recent piece of philosophy of mathematics.[8] Some of the consequent discussion has been helpful to Wright's 'neo-Fregean' cause. One might mention in this connection John Burgess's proof that Frege arithmetic is indeed consistent if second-order arithmetic is,[9] and Bob Hale's extended attempt[10] to defend the Fregean conception of abstract objects of which Wright's account of cardinal numbers is a special case. Much of the ensuing discussion, however, has been critical. The present volume collects a number of papers in which Wright and Hale, both severally and jointly, further elaborate their neo-Fregean theory of abstract objects in general and the *Grundlagen*-inspired theory of elementary arithmetic in particular; answer some of their critics (notably Michael Dummett and George Boolos); and consider possible extensions of the theory. (The last paper in the volume is an essay by Hale sketching a neo-Fregean treatment of real numbers.) All but one of the papers collected have appeared, or are about to appear, elsewhere, the exception being a long jointly authored essay in which Hale and Wright try to lay to rest the notorious 'Julius Caesar' problem that beset Frege's account of abstraction. But in view of the continuing interest in the neo-Fregean enterprise, many philosophers will find it convenient to have these papers in one volume.

The patience, dialectical skill, and resourcefulness which Hale and Wright display in attempting to defend their position against powerful objections are throughout impressive. They would, I think, concede that their defence is at best incomplete: at a number of crucial junctures, the viability of their theory is shown to depend upon more general issues in epistemology, in the philosophy of logic, or in the theory of definition, issues which they do not pretend to have resolved. I do not mean this observation as a criticism. On the contrary, one of the book's merits is the way it exposes connections between questions in the philosophy of arithmetic and these broader philosophical issues. Rather than try to award points in Hale and Wright's bouts with their critics, however, I shall devote this essay to an aspect of their position which

7. For this expression, see especially the late fragment 'Erkenntnisquellen der Mathematik und der mathematischen Naturwissenschaften' in Frege, ed. H. Hermes et al., *Nachgelassene Schriften*, henceforth *NS* (Felix Meiner, 1969), pp. 286–294. (Translated as 'Sources of knowledge of mathematics and the mathematical natural sciences' in Frege, tr. P. Long and R. White, *Posthumous Writings*, henceforth *PW* (Blackwell, 1979), pp. 267–274.) But the idea of such a source of knowledge animates Frege's thought from the outset.
8. The literature is usefully surveyed in Fraser MacBride, 'Speaking with shadows: a study of neo-logicism', forthcoming in the *British Journal for the Philosophy of Science*.
9. In his review of Wright's book in *The Philosophical Review* 93 (1984), pp. 638–40.
10. In his book *Abstract Objects* (Blackwell, 1987).

has not, I think, been scrutinised as closely as it deserves—namely, their case for treating numbers as objects.

II

The thesis that cardinal numbers are objects is Frege's, and Hale and Wright's defence of it elaborates Frege's own argument for it in *Die Grundlagen*. Discussing *Zahlangaben* ('attributions of number') such as 'The number 0 belongs to the concept *F*', Frege remarks (in §57 of that work) that

> 0 is only an element in the predicate (taking the concept *F* to be the real subject). For this reason, I have avoided calling a number such as 0 or 1 or 2 a *property* of a concept. Precisely because it forms only an element in what is asserted [i.e. in what is asserted about the concept *F*], the individual number shows itself for what it is, namely a self-subsistent object. I have already drawn attention above to the fact that we speak of 'the number 1', where the definite article serves to class it as an object. In arithmetic, this self-subsistence comes out at every turn, as for example in the identity $1 + 1 = 2$. (*Gl*, pp. 68–9)

Insofar as we can distil from this passage an argument for the thesis that numbers are objects, one of the premises would appear to be the claim that, in their arithmetically central uses, number-words and numerals behave as singular terms. Frege acknowledges the patent fact that in everyday speech number-words appear more frequently as adjectives than as singular terms. However, he contrasts these ordinary uses of numerical expressions with the scientific deployment of arithmetical vocabulary:

> Now our concern here is to arrive at a concept of number usable for the purposes of science; we should not, therefore, be deterred by the fact that in the language of everyday life number appears also in attributive constructions. That can always be got around. For example, the proposition 'Jupiter has four moons' can be converted into 'The number of the Jupiter's moons is four'. . . . Here, 'is' has the sense of 'is identical with' or 'is the same as'. (*Gl* §57, p. 69)

Frege's suggestion here—that attributive, or predicative, uses of number-words are confined to 'everyday' talk and are not found in scientific discourse—is surely wrong. 'There are three prime factors of thirty' is a (true) proposition of the science of arithmetic, but in it the first number-word (viz. 'three') is an adjective, while the second (viz. 'thirty') is a noun. The crucial issue, then, is not whether to give priority to the scientific over the quotidian. Rather, it arises within the overarching Fregean project of constructing a formal language to facilitate the rigorous axiomatisation of the science of arithmetic. The issue is, first, whether we should take one of these two uses of number-words as basic and explain the other use in terms of it; and, if we do, which use ought to be taken as basic.

Hale and Wright follow Frege in focusing on such propositions as ‘Two is a prime number’, ‘3 + 5 = 8’, and they elaborate his case for treating at least these occurrences of number-words and numerals as terms standing for self-subsisting objects. As they reconstruct it, the argument runs as follows:

1. There are criteria for deciding whether a given occurrence of an expression is a singular term which can be applied in advance of determining to which category an expression’s reference belongs, or even in advance of determining whether it has a reference.[11]
2. If occurrences of an expression meet these criteria, and are found used in true extensional contexts, then they will refer to a self-subsistent object.[12]
3. The relevant occurrences of numerical expressions do meet the criteria for being singular terms, and are found used in true extensional contexts.

So:

4. The relevant occurrences of numerical expressions refer to self-subsistent objects.

Where Hale and Wright attempt to improve upon Frege is in explicitly formulating the criteria which an expression must meet in order to qualify as a singular term. They advance “criteria relating to inferential role of the kind originally proposed by Dummett” in Chapter 4 of *Frege: Philosophy of Language*;[13] although they “believe that those [Dummett] actually proposes require some refinement” (p. 8). On this understanding of the matter, premise (1) above comes to express what Wright calls ‘the syntactic priority thesis’: the concept of a singular term is a syntactic notion. The pertinent sense of ‘syntax’ is rather generous: in assessing whether given occurrences of an expression qualify as

11. “It must be possible to identify expressions as functioning as singular terms *independently* of the assumption that those expressions refer—or purport to make reference—to objects. It must be possible to discern, from independently accessible features of their use, which expressions function—successfully or not—as singular terms” (p. 8).
12. “[If] certain expressions function as singular terms in various true extensional contexts, there can be no further question but that those expressions have reference, and, since they are singular terms, refer to objects. The underlying thought is that—from a semantic point of view—a singular term just *is* an expression whose function is to effect reference to an object, and that an extensional statement containing such terms cannot be true unless those terms successfully discharge their referential function” (p. 8).
    I shall not discuss this premise further in this paper, but it is worth noting that it has come under powerful attack in the writings of Harold Hodes. Hodes agrees with Hale and Wright that numerals pass the relevant syntactic and inferential tests to qualify as singular terms, but he denies that all expressions that so qualify have the semantic function of effecting reference to an object. Some do, of course; but the numerals, he thinks, have the quite different semantic function of “encoding” exact cardinality quantifiers. Hodes adumbrates this view in ‘Logicism and the ontological commitments of arithmetic’, *The Journal of Philosophy*, 81 (1984), pp. 123–49; and develops it in formal detail in ‘Where do the natural numbers come from?’, *Synthese*, 84 (1990), pp. 347–407. Since Wright’s 1983 book serves as Hodes’s chief foil in the latter article, it was somewhat disappointing to discover that that the present volume contains no defence of premise (2) against his criticisms.
13. Second edition, Duckworth, 1981.

instances of a singular term, we may have regard not only to whether certain strings of words constitute well formed sentences, but also to whether certain simple inference schemata are, intuitively, valid. However, if given occurrences of an expression pass these tests, then they *ipso facto* purport (at least) to refer to an object. And if such occurrences are to be found in true sentences of the appropriate kind, then they succeed in so referring.

I doubt whether this way of elaborating the criteria for being a singular term catches Frege's intentions. As we shall see, the criteria that Hale and Wright propose make no express mention of identity. And yet not only does Frege give an arithmetical identity as his one example in *Grundlagen* §58 of a proposition which brings out the objectual status of number, he also writes that

> we have already established that number words are to be understood as standing for self-subsistent objects. Therewith is given to us a class of propositions which must have a sense—namely, those which express our recognition [of a number as the same again]. If we are to use the symbol *a* to signify an object [i.e. if we are to use the symbol *a* as a singular term], we must have a criterion for deciding in all cases whether *b* is the same as *a*, even if it is not always in our power to apply this criterion. (*Gl* §62, p. 73)

The 'therewith' just quoted suggests forcibly that Frege took it to be necessary, if an expression is to qualify as a singular term, that sense should attach to the result of concatenating it first with the sign of identity and then with another such term. And, as the passage just quoted makes it clear, he took it to be necessary, if sense is to attach to such a concatenation, that some criterion of identity should be associated with the candidate singular term. As an interpretation of Frege, then, the syntactic priority thesis stands or falls with the claim that Frege's notion of a criterion of identity can be given a purely syntactic explication. Since Hale and Wright do not essay such an explication, a crucial element is missing from their defence of the thesis.

It is doubtful, moreover, whether this omission could be rectified. On a natural and sympathetic reading, Frege's argument about criteria of identity runs as follows. If the (English) expressions '*a*' and '*b*' are to qualify as singular terms with senses, then the sentence '*a* is identical with *b*' must qualify as a complete proposition with a sense. If this sentence is to qualify as a complete proposition with a sense, then there must be a possible circumstance in which a thinker could either know it to be true (i.e. in which he would "recognise *b* as *a*")[14] or know it to be false. But it calls for explanation, Frege supposes, how a thinker with our intellectual faculties could possibly attain such knowledge; it needs to be explained how his judgement (whether of identity or of distinctness) could qualify as knowledge. A Fregean criterion of identity, I suggest, is a proposition which codifies the relevant explanation. The sort of codification I have in mind may be illustrated by the case of number. Let us suppose that a waiter judges that the number of forks on the table is identical with (or is

14. Note that the word which Austin renders 'recognise', viz. *wiedererkennen*, is etymologically connected with a species of knowledge.

distinct from) the number of knives on the table. How could this judgement qualify (in a favourable case) as knowledge? The natural explanation (which is encapsulated in Frege's criterion of identity for numbers) is that it can so qualify so long as the waiter has checked, and thereby knows, that there are (or are not) just as many forks as knives. More generally, a thinker's capacity for knowing whether there are just as many *F*s as *G*s (a capacity which he may possess in advance of mastering the concept of a cardinal number) explains how he can come to know whether the number of *F*s is the same as the number of *G*s. On this reading, Frege is concerned to explain the very possibility of knowledgeable judgements of identity or distinctness, so we can account for his insouciant remark that it need not be "always in our power to apply the criterion".

I shall not try to determine whether the argument just presented is sound. While the reasoning is plausible when applied to such candidate singular terms as 'the number of forks', perhaps nothing can be said to explain the possibility of knowledgeable judgements of identity or distinctness in more basic cases. Perhaps, then, only some singular terms are associated with Fregean criteria of identity. But if Frege's *notion* of a criterion of identity is to be glossed in anything like the way just suggested, the syntactic priority thesis is in trouble as an interpretation of Frege. For whether a condition can serve as the right-hand side of a criterion of identity will depend upon whether it can contribute to explaining the possibility of a certain kind of knowledge. Even on the most generous conceivable delimitation of 'syntax', this can hardly be a syntactic matter. But in that case the syntactic priority thesis—the claim that it is "possible to explain the notion of singular term in, broadly speaking, syntactic terms"[15]—cannot be attributed to Frege.

The interpretative hypothesis that I am advancing—that Frege took association with a criterion of identity to be a necessary condition for an expression to be a singular term—is confirmed by his later writings. In diary entries for March 1924, Frege recorded his suspicion "that our way of using language is misleading, that number-words are not singular terms standing for objects at all, . . . so that 'Four is a square number' simply does not express the subsumption of an object under a concept, and hence just cannot be construed like the proposition 'Sirius is a fixed star' ".[16] On Hale and Wright's interpretation,

15. Wright, *Frege's Conception of Numbers as Objects*, p. 53.
Oddly, Hale seems to agree with Frege (as I read him) that association with a criterion of identity is necessary for an expression to qualify as a singular term. (See esp. p. 69 where he records his "complete agreement with Dummett on the importance of this further condition [for being a singular term]".) And he appears to take this to show that "the relatively formal tests under discussion are insufficient for genuine singular termhood" (*ibid.*). But then one wants to hear where Hale stands vis-à-vis Wright's syntactic priority thesis. Does he think that criteria of identity involve reference to, and quantification over, objects? If not, how does he conceive of a criterion of identity? If so, what is left of the syntactic priority thesis as Wright formulated it in *Frege's Conception*?

16. 'Tagebucheintragungen über den Begriff der Zahl', *NS* 282–3 at p. 282 (translated as 'Diary entries on the concept of number', *PW* 263–4 at p. 263). Contrast Wright: "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" (*Frege's Conception*, p. 14).

Frege could entertain this suspicion only if he had departed radically from the conception of singular termhood implicit in *Die Grundlagen*. For on their interpretation of that early work, an expression's qualifying as a singular term consists in the fact that certain elementary inferences involving it are valid. Their interpretation, then, simply leaves no room for the possibility that there should be whole categories of expression (such as number-words) which "mislead" us in exhibiting the surface inferential characteristics of singular terms without actually being such. (We may allow that an individual expression which purports to be a singular referring expression may yet fail to be a "singular term standing for an object" on account of reference failure.) Hale and Wright may argue that these late writings embody a conception of singular termhood which departs radically from the notion Frege had in mind when he wrote *Die Grundlagen*. In those writings, however, Frege does not present himself as repudiating his earlier standards for classifying expressions into logical levels; instead, he presents himself as considering whether he had correctly applied those standards in classifying number-words as singular terms. On the hypothesis that a requirement for termhood is association with a criterion of identity, we can understand why he does so present himself. For having identified cardinal numbers with value-ranges, and then having seen the inconsistency in his principle of identity for value-ranges, the Frege of 1924 might reasonably have doubted whether he did have a criterion of identity for cardinal numbers.

More generally, anyone who accepts the Fregean argument that I reconstructed three paragraphs back will be suspicious of the whole idea that expressions of a given kind can be classified as singular terms in advance of any consideration of the nature of the objects to which they might refer. We cannot definitely classify some expressions as singular terms, the argument goes, unless and until we have explained how thinkers like us could know the truth or falsity of at least some propositions of identity involving them. But that explanation will, in general, appeal to features of the *objects* that are the putative references of those expressions, and to our ways of acquiring knowledge about those objects. There is, for example, no prospect of explaining how we can come to know whether person *A* is identical with person *B* without appealing to the various ways that we have of coming to know things about persons, ways of knowing which are themselves intelligible only in the light of a conception of the sort of thing a person is. On this view of the matter,[17] any classification of expressions as singular terms on the basis of the "relatively formal tests" that Hale and Wright labour to formulate can never be more than provisional. Confirmation of their status as such will require some plausible explanation of how we can come to know the truth or falsity of propositions of identity involving them, and this will in turn require some account of the nature of the objects to which they refer. So far, then, from lending support to the syntactic priority thesis—according to which the notion of an object is to be *explained* by saying that an object is the correlate *in rebus* of a

17. Which has been elaborated in some detail by David Wiggins. See now his *Sameness and Substance Renewed* (Cambridge: Cambridge University Press, 2001).

singular term—this view encourages an alternative conception whereby the notions of a singular term and of an object are coeval. The final classification of an expression as a singular term will be possible only after information about its syntactic-cum-inferential behaviour has been considered in the light of our ways of gaining knowledge about its reference, and *vice versa*. In describing the operation of this two-way flow between syntax and ontology, the syntax is no doubt the right place to start, and syntactic considerations are in this sense 'prior' to any considerations about the nature of objects. But those considerations have to be brought into play before any provisional, syntactic, classification can be confirmed.

The irony here is that nobody has worked harder than Hale and Wright to try to show how this further requirement could be met in the case of numerical terms. In their discussion of the Julius Caesar problem, they argue, in effect, that Hume's principle can itself serve as a criterion of identity for numbers.[18] If it can so serve then, had he adopted Wright's suggestion and taken numbers to be an irreducible sort of abstract objects whose identity conditions are given by Hume's principle, Frege would have had an identity criterion for cardinal numbers even after his theory of value-ranges had been destroyed. Even on the richer conception of singular termhood just sketched, then, it is arguable that he still had the resources to sustain his original classification of numerals as singular terms after the disaster of Russell's paradox. So my disagreement with Hale and Wright over the proper explanation of Frege's notion of an *Eigenname* does not seriously compromise their claim to be defending a renovated version of the philosophy of *Die Grundlagen*. There are reasons other than the desire to map Frege's own position faithfully, however, for striving to be clear where the notion of a singular term resists a purely syntactic explanation. On Frege's view as I have reconstructed it, a failure—or at least, a chronic failure—to explain our ability to know a suitably wide range of propositions of identity involving numerals and number-words can undermine any provisional classification of those expressions as singular terms. However closely number-words may mimic the syntactic behaviour of singular referring expressions, the two-way flow allows any provisional syntactical classification to be over-ridden by semantic, metaphysical, or epistemological considerations. I shall return to this point at the end of the essay.

III

Even if it cannot ultimately stand alone, it remains of interest to determine how far Hale and Wright's "relatively formal" criterion takes us towards a satisfactory demarcation of the category of singular terms.

As Hale now explains it (in Chapters 1 and 2), the test for termhood rests on "Aristotle's dictum that whereas a quality has always a contrary, a (primary) substance does not" (p. 52). Accordingly,

18. Note that Hume's principle directly specifies identity conditions for the *objects* that are cardinal numbers: in stating it, the 'number of' operator is used and not mentioned.

singular terms may be distinguished from predicates by appeal to the consideration that whereas for any predicate there is always a contradictory predicate, applying to a given object just in case the original predicate fails to apply, there is not, for singular terms, anything corresponding to this—we do not have, for any given singular term, another 'contradictory' singular term such that a statement incorporating the one is true if and only if the corresponding statement incorporating the other is not true (*ibid.*).

As Hale well appreciates, however, this test will deliver the desired results only if the class of statements "incorporating" the expression whose status is in question is carefully constrained. For consider the expression 'is wise'. We want this not to qualify as a singular term, and there is indeed a contradictory predicate, viz. 'is not wise', which applies to an object just in case 'is wise' fails to apply. On Hale and Wright's theory, however, the notion of an object is to be explained in terms of the notion of a singular term, so the notion of an expression's applying (or failing to apply) to an object cannot be invoked in formulating the tests for termhood. And while Hale's formulation in terms of incorporation avoids this kind of circularity, the statements that "incorporate" 'is wise' cannot be allowed to include 'Everyone is wise'. For the statements 'Everyone is wise' and 'Everyone is not wise' can both be false, showing that a complete statement in which 'not wise' has replaced 'wise' is not always the first statement's contradictory, and hence refuting the claim that the expressions 'is wise' and 'is not wise' are so related that any "statement incorporating the one is true if and only if the corresponding statement incorporating the other is not true". Similarly, the statements incorporating 'is wise' cannot be allowed to include 'Some man is wise', for 'Some man is wise' and 'Some man is not wise' can both be true. "Intuitively," Hale thinks, "what is wanted is a general restriction which, when our candidate [expression] is a (first-level) predicate, ensures that the [relevant incorporating material] does not include substantival expressions functioning as quantifiers" (p. 53). In a more standard linguistic idiom, then, he thinks he needs to exclude quantifier-phrases such as 'everyone' and 'some man'.

He tries to do this by using revised versions of the Dummettian inferential tests mentioned in the previous section. "Whilst the Dummettian tests do not, by themselves," he writes, "provide the means of discriminating singular terms from expressions of *all* other kinds" (which is why they need to be supplemented by the Aristotelian criterion), they are said to "accomplish the more modest goal of marking off singular terms within their grammatical congruence class, thus distinguishing them from substantival expressions functioning as (first-order) quantifiers" (p. 46). Thus a substantival expression *t* (which could be a singular term or a quantifier-phrase in the singular) is said to

function as a singular term in a sentential context '*A*(*t*)' [which is presumed to be English] iff

(I) the inference is valid from '*A*(*t*)' to 'Something is such that *A*(it)'
(II) for some sentence '*B*(*t*)', the inference is valid from '*A*(*t*)', '*B*(*t*)' to 'Something is such that *A*(it) and *B*(it)'

(III) for some sentence '*B*(*t*)', the inference is valid from 'It is true of *t* that *A*(it) or *B*(it)' to the disjunction '*A*(*t*) or *B*(*t*)'. (p. 68)[19]

Having thus "excluded from the category of singular terms all those substantival expressions that are not the genuine article, but are capable of occupying sentential positions where genuine singular terms can stand, we can then apply the [Aristotelian] necessary condition for termhood", but now on the understanding that substantival expressions that fail the Dummettian tests are excluded from the "incorporating" material. And this, Hale thinks, gives us what we want. For the substantival expression 'everyone' fails test (III), while 'some man' fails test (II). The Dummettian tests, then, exclude each of these expressions from appearing in the incorporating material in the Aristotelian test, so we can appeal to that test (as a necessary condition for termhood) to explain why 'is wise' is not after all a singular term.

It seems to me, though, that the Dummettian tests do not accomplish even the "modest goal" of distinguishing genuine singular terms from other substantival expressions. To show this, however, we need first to consider what can be meant, in the context of these tests, by saying that the inference from a set of English sentences *X* to a single English sentence *A* is valid. I do not think that we can gloss this as meaning that *A* is a logical consequence of *X*, if this is understood in any Tarskian or Bolzanian way—namely, as adverting to different possible interpretations of, or admissible substitutions for, the non-logical expressions in *X* and *A*. For we can only decide which interpretations of an expression are possible, or decide which substituents for it are admissible, after we have determined its logical classification, whereas the Dummettian tests need to be applied before the sentences in question have been articulated into their logically relevant parts. They are, after all, supposed partly to underpin that articulation, and hence cannot presuppose it. For a similar reason, I do not think we can gloss 'The inference from *X* to *A*' is valid as meaning that it is possible to infer *A* from *X* using the rules of some specified logical calculus. For whether the calculus is directly applicable to English sentences, or whether it must be applied in the first instance to a formalised language into which the relevant English sentences are translated, its application will again presuppose an articulation of the sentence into its logically relevant parts, an articulation which the tests in question are supposed to underpin, not take for granted. Indeed, it seems that the only notion of validity that is available at the stage at which we are to imagine ourselves applying the tests is one whereby the inference from *X* to *A* is valid iff *X* strictly implies *A*: there should be no possibility of all the members of *X* being true and *A*'s not being true.

If the tests are understood in this way, however, there is a problem in using them to distinguish between singular terms and certain quantifier-phrases. For consider a quantifier-phrase—such as 'Some even prime number'—whose

19. In fact, Hale formulates four further conditions on the application of these tests—in particular, on the selection of the context '*B*(ξ)'. He requires, for example, that '*B*(*t*)' should neither entail nor be entailed by '*A*(*t*)'. The interested reader may verify that the counter-example I am about to give to the tests, insofar as they are intended to provide a method for distinguishing singular terms from other substantival expressions, meets these further conditions.

predicate is, as a matter of necessity, uniquely instantiated. When such a quantifier-phrase is substituted for '*t*' in tests (I) and (III), they are clearly passed, and if validity is understood in the way just suggested, so is test (II). Indeed, whatever context '$B(\xi)$' might be, there is no possibility that 'Some even prime number is *A*' and 'Some even prime number is *B*' should both be true while 'Something is both *A* and *B*' is not true; for there is no possibility that there should be more than one even prime number.[20] Tests (I) to (III), then, do not fulfil the role Hale proposes for them—that of distinguishing between singular terms and all other substantival expressions.

Some philosophers may resist this counter-example by distinguishing between notions of necessity. It is, they may say, merely an arithmetical necessity that there should be one and only one even prime, whereas the modality used to explicate the notion of strict implication must be purely logical necessity. Putting such weight on this distinction, however, hardly coheres with the general project of reviving a version of arithmetical logicism. If, as Hale and Wright believe, 'There is one and only one even prime' is a logical consequence of an analytic proposition, it is surely logically necessary in the 'broad' sense in which 'Bachelors are unmarried' is logically necessary. And while there may be some contexts in which that broad notion of logical necessity is usefully distinguished from a narrower notion exhibited only by instances of formally valid schemata, this distinction cannot be drawn at the stage at which we are to imagine Hale's tests being applied. For we can identify the formally valid schemata only after sentences have been articulated into their logically significant parts, whereas Hale's tests are supposed to underpin such an articulation, not presuppose it.[21]

This is, in fact, an unnecessary error on Hale's part: he does not need to make the claim to which we have found a counter-example. He is led to seek a "general restriction" for excluding quantifier-phrases because he wants to ensure that the Aristotelian test for singular termhood excludes first-level predicates. But in fact the counter-exemplary quantifier-phrases can be allowed to form the "incorporating material" in the Aristotelian test without disrupting its intended operation. For let us suppose that a predicate '*F*' is uniquely instantiated as a matter of broadly logical necessity. Then, for any predicate '*G*', 'Some *F* is *G*' will strictly imply, and will be strictly implied by, 'Every *F* is *G*', so that 'Every *F* is not *G*' implies and is implied by 'Not every *F* is *G*', and 'Some *F* is not *G*' implies and is implied by 'It is not the case that some *F* is *G*'. When the predicate '*F*' is uniquely instantiated as a matter of necessity, then, the quantifier-phrases 'some *F*' and 'every *F*' may be allowed to form the incorporating material in the Aristotelian test without compromising the test's power to exclude first-level predicates from termhood.

20. Note (cf. the previous footnote) that these tests are passed even when '$A(\xi)$' and '$B(\xi)$' are selected so that 'B(t)' neither entails nor is entailed by '$A(t)$'. Take, e.g., '$A(\xi)$' to mean 'ξ is Fred's favourite number' and '$B(\xi)$' to mean 'ξ is Mary's favourite number'.
21. Note, too, that the counter-example will be reproducible in *any* logical system which resembles Frege's in containing the first-order existential quantifier alongside a predicate whose unique instantiation is a matter of a logical necessity. For where '$F(\xi)$' is any such predicate, we can consider the quantifier-phrase 'Some *F*'.

This observation may, indeed, lead Hale to hope that after minor tinkering his test may yet yield the results he wants. Even if he must retract his claim that the Dummettian tests succeed in distinguishing singular terms from *all* other substantival expressions, our counter-example does not refute the hypothesis that they distinguish singular terms from those quantifier-phrases which are *problematical* in having to be excluded from incorporating material in the operation of the Aristotelian tests. And, Hale might argue, so long as they exclude the problematical quantifier-phrases, the Dummettian tests will do the work required of them, for the Aristotelian test can then itself be applied to exclude 'some even prime' and 'every even prime' from termhood. For to each of these quantifier-phrases there will stand a contradictory quantifier-phrase.[22] It seems to me, though, that the theory has by this point strayed too far from the syntactic intuitions it is supposed to codify. On the view being envisaged, the quantifier-phrase 'some odd prime' is not a singular term because it fails the Dummettian test (II); whereas the quantifier-phrase 'some even prime' is not a singular term because it fails the Aristotelian test. But whilst I agree with Hale that these expressions are not singular terms, it passes belief that the explanation of why they are not singular terms should be completely different in the two cases. We are entitled to expect an explanation of why instances of 'some . . . prime' are not singular terms which applies whether the lacuna is filled by 'odd' or by 'even'. Indeed, we are entitled to expect a uniform explanation of why, quite generally, quantifier-phrases in the form 'some *F*' do not qualify as singular terms.

IV

I do not despair of eventually finding an essentially syntactical—and essentially Fregean—explanation of why no such expression qualifies as a term. In §47 of *Die Grundlagen*, Frege suggests construing such expressions as 'some' and 'every' as *binary* quantifiers.[23] The sentence 'Some whale is a mammal' is to be parsed as 'Some *x* (whale (*x*); mammal (*x*))'. On this analysis, the apparent phrase 'some whale' is not a constituent of the sentence. The fact that we get another well formed string of words if we replace 'some whale' by 'Leviathan' will be an entirely superficial phenomenon, which does not indicate any congruence between 'Leviathan' and 'some whale' at the level of syntactic description that is relevant to the validity of arguments. It would take me too far afield to assess the prospects of this treatment of quantified sentences, though the treatment is far more than just a programme vaguely sketched by me here.[24] But under this style of syntactic analysis, genuine singular terms are kept apart

22. Compare Hale's own use of the Aristotelian test to exclude singulary quantifier-phrases on pp. 70–71.
23. *Gl*, p. 60. For commentary on this passage, see David Wiggins, '"Most" and "All": some comments on a familiar programme, and on the logical form of quantified sentences', in Mark Platts (ed.), *Reference, Truth and Reality* (Routledge, 1980), pp. 318–346.
24. Syntactic descriptions of this kind have been developed in some detail by van Benthem, Westerståhl and their followers.

from all quantifier-phrases right from the outset. Indeed, since the theory does not parse quantifier-phrases as constituents of sentences, the question of whether their category overlaps with that of singular terms does not even arise.

The briefest contemplation of this sort of theory, however, brings out an oddity of Hale and Wright's enterprise. Hale expressly notes that his tests are intended to provide criteria for being a singular term in Frege's "broad sense" (p. 31). And the relevant sense is indeed broad. Both in *Die Grundlagen* and in *Grundgesetze*, the category of singular terms or *Eigennamen* subsumes both semantically simple expressions, such as most proper names and most personal pronouns, and semantically complex expressions, notably functional terms such as '2^2' and definite descriptions such as 'the man who broke the bank at Monte Carlo'. At least since 1905, however, and the publication of Russell's 'On Denoting', there have been serious doubts whether this class of expressions is not too broad to constitute a coherent logico-semantic category. Within modal and temporal contexts, after all, proper names and definite descriptions behave very differently. Since names are rigid, the schema 'Necessarily, a is φ; $a = b$; so necessarily b is φ' is valid when 'a' and 'b' hold places for proper names and '. . . is φ' holds a place for an extensional context, but the schema is not in general valid if they may be replaced by definite descriptions. Some writers, indeed, have claimed to discern more subtle differences even in purely extensional contexts. So long as the universal quantifier is understood unrestrictedly, and so long as the letter 'a' is understood to hold a place for a proper name, the schema 'Everything is φ; so a is φ' is valid as it stands. By contrast, the inference 'Everything decays; so the man who broke the bank at Monte Carlo decays' is at best an enthymeme, for the premise does not entail that one and only one man broke the bank at Monte Carlo. This difference reflects the different conditions for understanding names and descriptions. To understand a name is, in the first instance, to know for which object it stands. When there is no object for which the name stands, there is no such knowledge and hence only the illusion of understanding. (This will be why, on discovering that a proper name is empty, we classify our previous attempts to use it as 'misfires' and, in the absence of some special reason to preserve it, allow the name to drop out of the relevant language altogether.) Matters are very different with definite descriptions. To understand a description it is not necessary to know for which object it stands (i.e. which object uniquely satisfies it). And even if the description 'the man who broke the bank at Monte Carlo' turns out not to be uniquely satisfied, there is no question of its thereby ceasing to be a meaningful expression of English. On the contrary, it remains in good semantical order for as long as its component predicates do.

If these differences simply reflect a sub-division *within* the class of singular terms they will not disturb Hale and Wright's position. For—should they eventually succeed in formulating criteria for being a Fregean *Eigenname*—there is nothing to stop Hale and Wright from framing further tests for distinguishing between genuine proper names on the one hand and descriptions on the other. (Indeed, the different ways names and descriptions behave in modal and temporal contexts might be parlayed into the sort of inferential test of which they approve.) Reflection on these differences, however, has led many philosophers and linguists to follow Russell all the way to his more radical conclusion that definite descriptions are not logico-semantic units of the sentences in which they figure. On a proper analysis of the sentence 'The man who broke the bank at Monte Carlo is tall', they will say, there is no unit corresponding to the first nine words. Instead, the sentence has the binary quantificational structure 'The *x* (*x* is a man who broke the bank at Monte Carlo; *x* is tall)'.[25] But if *this* is right, then the enterprise of trying to find criteria for an expression to qualify as a Fregean *Eigenname* is at best quaint, for those expressions form no logico-semantically significant category.

V

I cannot address here the large question of whether Russell was right to reject Frege's category of *Eigennamen*. But even if Hale and Wright are right to follow Frege in allowing complex singular terms (and certainly if they are wrong), reflection on the status of such terms exposes a difficulty for their position.

For let us follow them, and Frege, in allowing semantically complex singular terms. In allowing this, we shall need to provide for the possibility that a (well formed) singular term should possess a sense while lacking a reference. If a semantically complex term possesses a sense, there will be a determinate condition which an object must satisfy in order to be its reference, but there is no general guarantee that one and only one object satisfies the relevant condition. And in providing for the possibility of reference failure, we shall have to emend the rules of the classical, Fregean, predicate calculus: for example, the fact that the term *t* is well formed will not now ensure that '*Ft*' follows from '∀*x Fx*'. We shall need to work, in other words, in a logic that is *free* inasmuch as it allows well formed singular terms to lack a reference.[26]

Frege himself allowed that there are well formed and intelligible singular terms (terms which possess *Sinn*) which yet lack a *Bedeutung*. But he appears to have thought that he could bring a language containing such terms within the ambit of his logical calculus by stipulatively assigning references to them. For reasons which Smiley has explained, this thought (if Frege ever had it) is mistaken.[27] In the context of a formal language such as Frege's, such stipulations will combine to convert each partial function into a total function. But, however much Frege may have disliked them, the importance partial recursive functions have within mathematics, alongside the fact that their properties cannot be recovered from the related total functions, makes them a phenomenon with which the logician has to deal, not an awkwardness which he can legislate away. We need, in other words, to be able to reason about partial

25. For sympathetic exposition of such a view, see Gareth Evans, *The Varieties of Reference* (Clarendon Press, 1982), pp. 51–60.
26. The logic need not be free, however, in allowing the empty domain, or in making a distinction between inner and outer domains.
27. See T.J. Smiley, 'The Theory of Descriptions', *Proceedings of the British Academy*, 67 (1981), pp. 321–37, esp. p. 325.

functions, and free logic provides the canons of such reasoning. It is the logic we need to use when there is a possibility that a singular term might lack a reference, or when a functional expression might stand for a partial function.

Assuming that expressions of the form 'the number of *F*s' are *bona fide* singular terms, do we need to use a free logic in reasoning with them? Expressions of this kind are clearly semantically complex, as indeed are all the terms used to stand for individual numbers in a logicist reconstruction of arithmetic.[28] And given one of Hale and Wright's basic assumptions—and even waiving the question of whether 'the number of *F*s' need refer to anything if vague predicates are allowed to replace '*F*'—a free logic is called for. The assumption I have in mind is their denial that the 'number of' operator is a logical constant.[29] Given that assumption, the logical form of a numerical term provides no guarantee that it will possess a reference. For on this view, the logical form of 'the number of *F*s' will be simply 'Ξx: φx', where 'φ' holds a place for any one-place predicate and where 'Ξ' holds a place for any term-forming operator on one-place predicates. The latter place may be occupied without incongruity by 'the set of', so that 'the number of *F*s' will share its logical forms with 'the set of *F*s', some of whose instances (such as 'the set of non-self-membered sets') certainly do not succeed in referring. To be sure, the supposition that every concept determines a number does not generate a contradiction as does the supposition that every concept determines a set. *Ex hypothesi*, however, this difference flows from the differences in meaning between two term-forming operators which are not logical constants. So the underlying *logic* must allow for the possibility that 'the number of *F*s' lacks a reference, and that the 'number of' functor stands for a partial function from concepts to objects. It must, in other words, be a free logic in the sense specified two paragraphs back.

Given the predominant modern conception of the nature of logic, this consequence of denying to 'number of' the status of a logical constant ought, I think, to be uncontroversial. It results from applying to the present case the principle that if logic guarantees a reference to a given expression, then that guarantee must extend to any other expression which share all its logical forms. But it engenders a serious difficulty for Hale and Wright's position. Central to that position is, as we have seen, the claim that Hume's principle is analytic,[30] but while there is no difficulty reproducing the content of Hume's principle within a free logic, in such a context it bifurcates in an interesting way. On the one side, we have that part of Hume's principle which gives the criterion of identity for numbers when a number belongs to a concept, viz.:

28. Hale does write at one point of "simple numerals" in apparent contradistinction to "various complex numerical expressions" (p. 31), but given his neo-logicist ambitions this is clearly a slip. For theorems involving particular numerals will only be provable from Hume's principle if the numerals are construed as definitional abbreviations of complex expressions.
29. "It would be plausible to regard the limits of second-order logic as crossed just at the point where substantial principles concerning specific types of [abstract] objects begin to feature" (p. 279). It is clear from the context that Hume's principle is supposed to be a paradigm case of such a substantial principle.
30. In the sense explicated in the quotations from Wright in §I above. Hale and Wright officially deny that Hume's principle is analytic in Frege's sense, let alone in Kant's. See, however, *n.*43 below.

(HP minus) For any concepts *X* and *Y*, if there is such an object as the number of *X*s, or the number of *Y*s, then the number of *X*s is identical with the number of *Y*s iff there are just as many *X*s as *Y*s.

On the other side, we have the part of Hume's principle that tells us when there is such a thing as the number of *X*s:

(Universal countability) For any concept *X*, there is such an object as the number of *X*s.

The possibility—indeed the inevitability, given that the underlying logic is free—of separating these two elements in what Hale and Wright call Hume's principle was first noted some fifteen years ago by Neil Tennant.[31] Tennant, in fact, rejects the principle of universal countability. But the mere possibility of factoring Hume's principle into Hume minus and Universal Countability creates a problem for Hale and Wright.

As we have already had occasion to note, they deny that Hume's principle is a logical truth. Their neo-logicist claim is, rather, that the principle, "though not analytic in either Frege's official sense or Kant's, can function . . . as an implicit definition of the numerical operator" (p. 14).[32] Its role, we are told, "is to explain . . . the general notion of identity of cardinal number" (p. 279), and hence to be "determinative of the concept it thereby serves to explain" (p. 14). But while it is plausible to hold that Hume's principle could be used to explain the concept *being the number of* to somebody who had not hitherto apprehended it, a principle's being so usable does not suffice for it to be 'a good implicit definition' in the pertinent sense of being "something which we can freely stipulate as true, without any additional epistemological obligation" (p. 133). For if we are to be able to stipulate the truth of a principle without needing to provide any justification for it, then the principle in question must *do no more than* explain the concept which it purports to define. It must not smuggle into the putative explanation any further claims about the concept. In particular, in the case when the concept being explained is the meaning of a first-level predicable, it must not smuggle into the putative explanation the claim that the concept is instantiated. And the worry about Hume's principle is that does make precisely such a further claim. It does not merely explain what it would be for an object to be the number belonging to a given concept, and hence for an object to be a

31. See Neil Tennant, *Anti-Realism and Logic: Truth as Eternal* (Clarendon Press, 1987), esp. pp. 290ff; cf. also his 'The necessary existence of numbers', *Noûs*, 31 (1997), pp. 307–336 at pp. 312–13. In his book, Tennant characterises the meaning of 'number of' using a pair of introduction and elimination rules rather than directly via a free-logical version of Hume's principle. But Hale and Wright can hardly find that objectionable: on the contrary, they cite (see e.g. pp. 116–17) such systems of rules as paradigm cases of the sort of implicit definition which Hume's principle is said to exemplify. In view of Tennant's criticisms of Wright, it is somewhat surprising to see no discussion of his work in the book under review (although the two pieces just cited are listed as "Further Relevant Reading" on p. 446).

32. Hale and Wright devote a valuable chapter (Ch.5) to defending the coherence and value of implicit definitions against some current animadversions and misunderstandings.

number.[33] It also entails that the concept *number* is instantiated. Indeed, it entails the strong thesis that each (Fregean) first-level concept has a number belonging to it.

At various points in the book under review, Hale and Wright attempt to head off challenges along these lines; their replies, however, are unconvincing. Considering a related challenge by Hartry Field—namely, that the strongest proposition in this vicinity that can be said to be a conceptual truth is the conditional

If numbers exist, then $\forall X \forall Y (Nx{:}Xx = Nx{:}Yx \leftrightarrow$ there are just as many Xs as Ys)

—they point to "an immediate difficulty. . . . How are we to understand the antecedent condition?" (p. 143). The antecedent presupposes some understanding of what it is to be a number, which (given the basic Fregean assumption[34] that the notion of *being the number of* is prior to the notion of *number tout court*) presupposes in turn some understanding of the 'number of' operator. "But," they continue, "it was the stipulation—unconditionally—of Hume's principle, which was supposed to explain that operator. That explanation has lapsed; but Field has nothing else to put in its place" (*ibid.*).

It is no part of my brief to defend Field's nominalism. The problem for Hale and Wright, though, is that Hume minus—useless as it is for the neo-logicist's purposes unless and until it is supplemented by further principles of numerical existence—*does* seem to provide a perfectly adequate explanation of the notion of an object's being the (cardinal) number of a concept and thereby (given the basic assumption that to be a number is simply to be the number of some things) of the notion of an object's being a number *per se*. Anybody who knows Hume minus knows that, when there is such an object as the number of *F*s, it will be identical with the number of *G*s iff there are just as many *F*s as *G*s. I do not see that somebody who knows so much has a deficient grasp of the notion of a cardinal number. To be sure, he does not know that there *are* any objects answering to this notion. (Indeed, if he thinks about the matter for a few moments, he will see that there cannot be objects that do so answer unless there are infinitely many objects.) But he knows how to judge of the identity or distinctness of any such objects as may exist, and this gives him the understanding needed to entertain the thought that there are numbers, and thereby to understand the antecedent of Field's conditional.

But if Hume minus does give us enough to understand the notion of being the number that attaches to a concept, then the neo-logicist project, as Hale and Wright characterise it, is in trouble. Their aim, as we have seen, is to rehabilitate Frege's epistemology of arithmetic by showing how the basic laws of arithmetic (the Peano postulates) may be derived from a principle which needs no further justification because it does no more than implicitly define a notion. We have found reason, though, for doubting whether any such

33. Hale and Wright accept the Fregean principle that to be a (cardinal) number simply is to be the number that belongs to some concept.

34. Thus *Gl*, §72: "the expression '*n* is a cardinal number' is to mean the same as the expression 'there is a concept such that *n* is the cardinal number which belongs to it' " (p. 85).

principle exists. Hume's principle (as Hale and Wright formulate it) will not serve. For while it certainly entails the Peano postulates, it does not merely implicitly define 'number' as a first-level predicate; it also postulates that there are objects which satisfy that predicate. Hume minus, by contrast, may be said to do no more than to define 'number of' and therewith '(cardinal) number'; but it does not entail the Peano postulates. When the consequences of admitting semantically complex terms into the relevant language are fully thought through, then, we seem to lack a principle which is weak enough to be simply laid down as true and yet strong enough to entail the basic laws of arithmetic.[35]

VI

I do not imagine that this conclusion constitutes an impasse. I shall conclude by briefly considering three possible ways out, arranged according to their distance from the philosophy of arithmetic adumbrated in *Die Grundlagen*.

(A) Back to Die Grundlagen

The argument advanced in the previous section for using a free logic to elicit the consequences of the axioms of a theory of number rested on the assumption (made by Hale and Wright) that 'the number of' operator is not a logical constant. But *Die Grundlagen* contains an argument that it is. On Frege's view, the mark of a logical constant is that the norms which regulate its use "govern" (*beherrschen*) thought about any topic whatsoever. And, Frege argues, 'number' displays this mark: if we try to deny any one of the basic propositions of the science of number, "complete confusion ensues. Even to think at all seems no longer possible".[36] This suggests that one might hope to avoid the use of free logic by claiming for the 'number of' operator the status of a logical constant, and for Hume's principle the status of a logical truth. This view is not properly described as a *neo*-Fregean philosophy of arithmetic. Rather, it is full-blown Fregean logicism. The only difference from the view of the historical Frege is that 'number of' is now taken to be a primitive logical constant, and is not defined in terms of some putatively more basic 'value-range of' operator. Hale and Wright make two crucial deviations from Frege. First, they repudiate any attempt to embed arithmetic within a supposedly more general theory of value-ranges; second, they dilute his logicism in favour of the claim that Hume's

35. The discussion of this section has proceeded on the assumption that Frege was correct to treat semantically complex expressions as singular terms. But the prospects for neo-logicism are even worse if one follows Russell in these matters, and construes sentences involving complex terms as transformations of explicitly quantified sentences. When so construed, the left-hand side of an instance of Hume's principle such as 'The number of knives = the number of forks if and only if there are just as many knives as there are forks' becomes 'There is a unique number which belongs to the knives, and a unique number which belongs to the forks, and the former number is identical with the latter'. This rendering makes it patent how Hume's principle postulates the existence of numbers, as well as explaining the concept of number.
36. *Gl* §14, p. 21.

principle is, in the sense we have been discussing, analytic. The first deviation is a definite improvement, but it remains far from clear that the second is either necessary or (given the argument of §V) leads to a stable position.

The idea that Hume's principle is a logical truth has seemed to many philosophers to be a non-starter. Hume's principle cannot be a logical truth, they say, for it can be true only if there are infinitely many objects, whereas a logical truth must be true no matter what collection of objects is taken to be the domain over which its first-order variables range. Thus George Boolos: "Arithmetic implies that there are two distinct numbers; were the relativisation of this statement to the definition of the predicate 'number' provable by logic alone, logic would imply the existence of two distinct objects, which it fails to do (on any understanding of logic now available to us)".[37]

But are we really unable to attain an understanding of logic which might permit 'There are at least two objects' to qualify as a logical truth? One can hardly fail to notice that the conception of logical truth to which Boolos appeals excludes from the outset any view which (like Frege's) allows that some objects might exist as a matter of logical law. *Of course*, Frege will say, the logical laws postulating the existence of objects will fail to qualify as logical truths if such truths are required to be true even when the domain of quantification is restricted so as to exclude the posited objects. But why should such objects be excluded when—*ex hypothesi*—they exist as a matter of logical law? Frege's position here is, I think, entirely principled given his understanding of the quantifier. In his book—in all his books—the first-order universal quantifier is a genuine logical constant. It is to be interpreted always and everywhere as signifying that second-level concept which is true of all and only those first-level concepts under which absolutely every object falls. Similarly, the first-order existential quantifier is always to be interpreted as signifying that second-level concept which is true of all and only those first-level concepts under which some object falls. (The 'some' here is again unrestricted.) On this understanding of the matter, to consider various possible domains in assessing the logical truth of quantified propositions would be as egregious an error as considering various possible truth-tables for a sentential connective when classifying propositional formulae as tautologous or not. It would be to make the mistake of varying the interpretation of a logical constant.[38]

37. George Boolos, 'Is Hume's principle analytic', in his *Logic, Logic, and Logic* (Harvard University Press, 1998), pp. 301–314 at p. 302.

Why *two* objects, rather than one? Because mainstream logicians allow themselves to exclude from consideration the empty domain, and thereby count certain existentially quantified propositions (such as '$\exists x\, x = x$') as logical truths. However, the undeniable convenience of this ruling does not make it principled.

38. This is not, of course, to deny the significance or importance of propositions involving restricted quantification. But, on Frege's view, a proper logical syntax will make such restriction *explicit*. There is no reason, though, why such explicit restriction should always take the form of a matrix conditional antecedent or conjunct. Indeed, there is no reason why the language should not contain irreducibly binary quantifiers. What is crucial to Frege's conception of unrestricted quantification is not the adicity of the quantifiers, but that the *variables* should range unrestrictedly.

Hale and Wright, I should note, are aware of the importance of unrestricted quantification for Frege's project: see for example p. 316.

It is one thing to complain that the standard modern explanation of logical truth fails to do justice to a logic that posits logical objects. It is another to articulate an alternative explanation that does justice to such a logic. Certainly, the incompatibility between the standard explanation of logical truth and Frege's understanding of the quantifiers runs deep. According to that explanation, a formula that includes first-order quantifiers is logically true only if it is true no matter what *set* of objects its first-order variables are taken to range over. Applying this definition to a language in which first-order variables are understood to range over absolutely everything would mean positing a set of all objects. In standard set theory, however, there is no such set. Zermelo-Fraenkel set theory (ZF) has an axiom of separation (*Aussonderung*) whereby the existence of a set of all objects would imply the existence of a set of all non-self-membered sets; but by the reasoning of Russell's Paradox, there is no such thing.

One response to this difficulty would be to reject ZF in favour of a set theory that posits a universal set; this was in fact the line taken by Quine. Like Frege, he understood the first-order variables (which were of course the only variables permitted in his canonical notation) to range over absolutely every object that there is. (That, indeed, is part of what he meant by his often quoted, but less often understood, dictum 'To be is to be the value of a variable'.) And in the set theory of 'New Foundations' (NF), there duly is a set of all objects.[39] But even if we waive any difficulties that may arise from the fact that NF refutes the axiom of choice, a problem attends this approach to the problem of explaining the notion of logical truth for a language whose first-order variables range over objects unrestrictedly. NF avoids inconsistency (if it does) by restricting the comprehension schema. Not every well formed predicate in the language of NF has an extension; instead, extensions are assigned only to predicates which meet the syntactic condition of being *stratified*.[40] Now if we try to implement the standard explanation of logical truth within this system, we shall say that a formula in which the quantifiers are understood to have an invariant, unrestricted interpretation is logically true if it is true no matter what sets (of objects, or ordered pairs of objects, etc.) are assigned to its non-logical predicate letters. Since, however, a well formed predicate may lack an extension in NF, it will not follow *immediately* from this definition that any substitution instance of a logically true formula is true. Aficionados of NF tell me that it is, indeed, an open question whether this follows at all. But whatever the answer to that question may be, the philosophical problem is evident. It is a plausible constraint on an explanation of the notion of logical truth that a formula's logical truth should immediately and obviously entail its truth and the truth of all its substitution instances. Yet this constraint is violated by the explanation we have just considered.

39. W.V. Quine, 'New foundations for mathematical logic', *American Mathematical Monthly*, 44 (1937), pp. 70–80; reprinted with supplementary remarks in Quine, *From a Logical Point of View* (second edition, revised, Harvard University Press, 1980), pp. 80–101.

40. For the definition of 'stratified' (whose details will not matter for present purposes), and the restriction on comprehension, see *From a Logical Point of View*, pp. 91–2.

But could we not recast the explanation of logical truth so as not to require that the range of the variables forms a set? A recent paper by Agustin Rayo and Gabriel Uzquiano shows how we can.[41] They articulate a notion of consequence for second-order set theory in which a model is not treated as a kind of set (with the limitations of size that standard set theory would then impose upon it), but is regarded instead as a higher-order entity. And although they wish to allow variable domains which are too big to form sets, their method can be applied to define logical truth for languages in which the variables are understood to have a constant interpretation, ranging over absolutely everything that there is. For an example, consider a first-order language whose only predicate letter is a one-place letter '*P*'. Instead of interpreting '*P*' by specifying a set as its extension, we can conceive of a model for this language as a relation *M* (a Fregean *Beziehung*) between the expression '*P*' and all and only those objects to which it applies. We may then construct a definition of truth with respect to *M* which will entail that the formula '$\forall x\, Px$' is true with respect to *M* iff the predicate '*P*' is *M*-related to absolutely every object that there is. Finally, we may employ higher-order quantification to stipulate that a formula is logically true iff it is true with respect to every model for the relevant language.[42] This definition articulates an understanding of logic according to which the existence of two objects—indeed, of infinitely many objects—may be a logical truth.[43]

(B) Restrictions on Hume's principle

I hope that these considerations remove the scarecrow that has kept philosophers from taking seriously the possibility that Hume's principle might be a logical truth. And it seems to me that *if* it is a truth at all, it has good claim to be a logical truth. But *is* it true? One source of anxiety is the fact that it entails the principle of universal countability: for any concept *X*, there is such an object as the number of *X*s. Vague predicates create difficulty for this principle. Most of us, I take it, would wish to avoid commitment to a number of bald men without taking the radical Fregean step of altogether disqualifying vague predicates from instantiating second-order variables. But there are other problems too. The predicates 'set', and 'ordinal' are not vague in the way that

41. 'Toward a theory of second-order consequence', *Notre Dame Journal of Formal Logic*, 40 (1999), pp. 315–25.
42. Of course, the higher-order quantification cannot be understood as quantification over sets. Rayo and Uzquiano suggest interpreting it as plural quantification in the manner of Boolos; but other interpretations cohere with their theory.
43. Although the 'official' neo-Fregean position is that Hume's principle is not a logical truth, Wright, at least, sometimes slips into supposing that it is. "It may happen," he observes on p. 278, "that certain instances of the right-hand side [of Hume's principle] hold a priori, or even of logical necessity. And when that happens, the corresponding abstract objects will also exist a priori, or of necessity." This will only follow, one wants to protest, if the relevant instance of Hume's principle is itself logically necessary, i.e. logically true. But Wright never confronts head-on the contemporary understanding of logical truth that excludes this possibility.

'bald' is: there are no objects on the borders of sethood or ordinality. All the same, our most widely accepted set theory, viz. ZF, precludes a number of sets, or a number of ordinals. Again, there are no objects on the borders of being self-identical: every object is identical with itself. But adherents of ZF are not alone in rejecting the claim that there is a number of all objects.

Despite his official commitment to the truth of Hume's principle, Wright, at least, is receptive to the idea that it might need to be restricted so as to avoid assigning numbers to such concepts as *set*, *ordinal* and (self-identical) *object*. Under pressure from Boolos, he concedes that "we should not assume without further ado that every concept—every entity an expression for which is an admissible substituend for the bound occurrences of the predicate letters in Hume's principle—is such as to determine a number" (p. 314). That is to say, we should *not* after all assume that Hume's principle is true. His idea for excluding assignments to number to the problematical concepts is to invoke Michael Dummett's notion of an indefinitely extensible concept: Hume's principle is to be replaced by the thesis that

$$\forall X\, \forall Y\, (\neg\, I\,(X) \wedge \neg\, I\,(Y) \rightarrow (Nx\colon Xx = Nx\colon Yx \leftrightarrow X \approx Y)),$$

where 'I' symbolises the (second-level) predicate 'is indefinitely extensible' and '$\approx$' symbolises equinumerosity between concepts.[44] Wright, though, confesses that he does not "know how best to sharpen the idea" of indefinite extensibility (p. 316). The status of Hume's principle—to which so much of the book under review is devoted—emerges *in extremis* as moot. The real issue is the status of some emended version of the principle which still awaits a precise formulation.

A theory of number founded on HP minus, and cast in a free logic, gives one rather more room for manoeuvre in seeking principles of numerical existence than does Wright's more classical approach. There will be no need to formulate a generally sufficient condition for an instance of equinumerosity to yield identity (and hence existence) of number. Instead, we can proceed in a piecemeal fashion, laying down such principles of numerical existence as we are sure of, but leaving it open how far numerability might extend. (One might compare the way in which the generally accepted axioms of ZF leave the height of the set-theoretic hierarchy undetermined.) Working in just this spirit, Tennant has proposed two principles of numerical existence:

(1) there is a number of non-self-identical things (i.e. that zero exists)

and

44. See p. 316. Wright also considers restricting Hume's principle to sortal predicates, associated both with a criterion of application and a criterion of identity (pp. 314–5). This suggestion belongs to a rather different area of debate, and it is in any case questionable whether such a restriction is needed to avoid assigning number to such concepts as 'red', and 'gold'. For one might instead follow Dummett in supposing that "a Fregean semantics . . . assumes a domain already determinately articulated into individual objects"—*Frege: Philosophy of Mathematics* (Duckworth, 1991), p. 94. For further discussion, see my article 'Concepts and counting', *Proceedings of the Aristotelian Society*, 102 (2001–2), pp. 41–68.

(2) if the number of *X*s exists, and if there is one more *Y* than there are *X*s, then the number of *X*s exists too (he calls this the Ratchet principle).

He shows how these principles entail the Peano postulates, even though they involve no commitment to the existence of any transfinite cardinal.

All the same, I share Wright's desire to articulate a general condition for numerability, and I have elsewhere suggested a condition which may prove to be more tractable than anything that involves the notion of indefinite extensibility.[45] The intuitive idea is that a cardinal number should belong to a concept when (and only when) it is equinumerous with a concept whose instances could be used to perform an operation which recognisably generalises the operation of *counting* a finite collection.

Whether a number is assigned to a given concept will depend, of course, on how the notion of a count is generalised. My original proposal here was that a number will belong to a concept when it is equinumerous with a bounded initial segment of a concept whose instances may be strictly well ordered.[46] As Timothy Williamson pointed out to me, however, on this proposal there *will* be a number of ordinal numbers. The ordinal numbers are themselves well ordered under the natural ordering relation $<$, and we may define a relation R on them which is just like $<$ on the non-zero ordinals, for which $\neg R00$, and which is such that $Rx0$ and $\neg R0x$ for any non-zero ordinal x. The relation R strictly well orders the ordinals, and the ordinals are in one-one correspondence with a bounded initial segment of its field (the bound under R being 0, which has been 'moved' beyond all the other ordinals). On my proposal, then, there will be a number of ordinals, and Burali-Forti's paradox looms.[47] Indeed, if the axiom of global choice is accepted—so that there is a well-ordering of all the objects that there are—Williamson's device may be deployed to show that there is a number of all objects.

The solution is found when we notice that the original proposal neglects an important element in the ordinary notion of a tally—i.e., a concept whose instances may be used to assign cardinalities to concepts. It is not enough that the instances of the tally should be well ordered; we also require that each successive instance under the ordering should represent a new cardinality. It would not do to assign natural numbers to concepts using the sequence of numerals '1, 2, *a*, 3, . . .' where '*a*' indicated the cardinality of twosomes, for this would destroy the natural relation between a concept's cardinality and the place in the tally of the numeral used to indicate that cardinality. This point is respected in ZF, where the cardinal numbers are identified with *initial*

45. See my article 'Hume's principle and the number of all objects', *Noûs*, 35 (2001), pp. 515–541.
46. See *op. cit.*, esp. pp. 527–9.
47. Actually, it does not infect the theory of 'Hume's principle and the number of all objects'. Not having read Rayo and Uzquiano, I there envisaged NF as the background set theory for the metalogic of unrestricted quantification. But in NF there *is* a number of all ordinals. Assuming it is consistent, NF escapes the Burali-Forti paradox because there is no way of showing that each ordinal number is the number of its predecessors under the natural ordering on the ordinals.

ordinals—ordinals that are not equinumerous with any preceding ordinal under the natural ordering relation $<$. And the point should, and can, be respected by our principle of numerical existence. Let us say, then, that a strict well-ordering R on the instances of a concept T exhibits *cardinal redundancy* iff either (a) there exist distinct objects x and y such that Tx and Ty and the segment of Ts less than x (under R) is equinumerous with the segment of Ts less than y, or (b) there exists an object x such that Tx and the Ts are equinumerous with the initial segment of Ts less than x. We may now lay down the axiom that a number will belong to a concept when it is equinumerous with a bounded initial segment of a concept under a well-ordering which does not exhibit cardinal redundancy. This conforms to the intuitive idea that concepts have numbers when they are (in some general sense) countable, but since Williamson's relation does exhibit cardinal redundancy under head (b), his path to assigning a cardinal number to the concept *ordinal* is blocked. It is still possible to show, however, that this principle of numerical existence entails the existence of all the natural numbers, so that the Peano postulates may be proved in a free-logical system whose axioms are Hume minus and the present existence principle.

(C) Numbers as higher-order entities

I argued in §II that failure to find a suitably general criterion of identity for numbers ought to lead one to revisit the assumption that the explanatorily basic use of number-words is their use as singular terms. Since Wright (at least) now concedes that he has yet to find a definitive formulation of such a criterion, such a visit is overdue.

Number-words, we noted at the outset, play a double role in natural language. There are occurrences of them which seem to be instances of singular terms, as in 'The number four is square'.[48] But in other occurrences they function as adjectives—notably when they form part of a quantifier phrase, as in 'There are four moons of Jupiter'.[49] Hale and Wright hypothesise that occurrences of the second kind are to be explained in terms of the first. (Recall Frege's claim that 'Jupiter has four moons' can always be converted into 'The number of the Jupiter's moons is [i.e., is identical with the number] four'.) But could we not pursue the opposite strategy of explanation?

48. Hodes credits Sally McConnell-Ginet with the observation "that 'the number four' is best viewed as an appositional construction, like 'the philosopher John Dewey', rather than as of the form [Det + NP + Adj]" ('Where do the natural numbers come from?', pp. 403–4). She is right: 'the number four' does not mean 'the four number'. All the same, we should not discard the possibility that 'the number four' is best explained as meaning 'the *fourth* (positive natural) number'.

49. As Hodes again notes, though, number-words "make unusual adjectives: they don't enter into comparative or grading constructions; they form further adjectives (the ordinal-words), and only the latter transform into adverbs; they're mildly archaic as predicate adjectives, a use that requires plural subjects. (In 'She is four', 'years old' was deleted.)" *Op. cit.*, p. 350.

The problem with this proposal has always been its apparent incompatibility with any interesting form of logicism. (At least, this has been a problem for those of us who think that there might be some truth in logicism.) Each number-word, when construed as an adjective, pairs naturally with a numerically definite quantifier—'four' with 'there are exactly four', and so forth—and it is straightforward to formulate the Peano postulates as claims about the natural sequence of these numerically definite quantifiers. When so formulated, however, the postulate that says that any number (i.e., any numerically definite quantifier) has a successor distinct from itself will be false if there are only finitely many objects. If, for example, there are only one hundred objects, then the quantifiers 'There are exactly 101', 'There are exactly 102' etc. will all be empty. That is to say, they will all be second-level predicables which apply to no first-level concept.[50] This was why Russell, who took the adjectival use of number-words to be explanatorily basic, was driven to postulate his axiom of infinity.

I have no space here to investigate whether this problem is really fatal to any version of logicism which takes the adjectival uses of number-words as primary. However, (A) and (B) above do place this old problem in a new light. In (A), we saw how one may attain a conception of logical truth which can accommodate quantifiers free to range over all objects. In (B), we saw how Wright, at least, takes the concept of an object to be indefinitely extensible. This ought to lead one to ask whether there is any need expressly to *postulate* that there are infinitely many objects.

In raising this question, I am not suggesting that the claim that there are infinitely many objects is already derivable from logical principles that are generally accepted as such. It is not. Rather, I am inquiring whether anything significant would be lost from a logicist philosophy of arithmetic if we allowed the supposition that there are infinitely many objects to stand as just that: a *presupposition* of arithmetical discourse. In constructing a science of arithmetic, the thought runs, we may simply take it for granted that we shall never run out of objects to count. Given this presupposition, the definitions of 'zero', 'successor' and 'finite number' as species of numerically definite quantifier will deliver all that the early arithmetical logicists hoped to elicit from them, and we can account for our knowledge of the Peano postulates by reference to those definitions. Of course, this will be no good if the presupposition of infinitely many objects is in any way insecure. But if the concept of an object really is indefinitely extensible, the presupposition seems to me to be completely unassailable. An assault on it would have to take the form of trying to show that there are at most one hundred objects, or five hundred million objects, or googol many objects, or whatever. And the indefinite extensibility of the concept of an object prevents any such assault from getting underway. For if a concept is indefinitely extensible, then no finite number can belong to it.

50. For a full explanation of this problem, see Dummett, *Frege: Philosophy of Mathematics*, pp. 131–40.

Whatever may be the fate of this last suggestion, I think the debate could now benefit from further exploration of all three of these proposals. Evaluating them, and comparing them with Hale and Wright's official position, will return to the centre of the stage questions that they keep in the background: where the bounds of logic lie, and (most importantly) whether logical truths (and they alone) really possess the special epistemological status that Frege claimed for them. But a revival of interest in these questions might be one among the many benefits of this unusually thought-provoking collection of essays.[51]

51. I am grateful to the Leverhulme Trust, whose award of a Philip Leverhulme Prize Fellowship has given me the time to write this critical notice. I am also very grateful to Bob Hale and Crispin Wright, whose generous and thoughtful responses to a previous version of this paper have led to many improvements and have brought us all closer, I hope, to a meeting of minds.

ON THE PHILOSOPHICAL INTEREST OF FREGE ARITHMETIC

WILLIAM DEMOPOULOS
The University of Western Ontario

Traditional 'Fregean' logicism held that arithmetic could be shown free of any dependence on Kantian intuition if its basic laws were shown to follow from logic together with explicit definitions. It would then follow that our knowledge of arithmetic is knowledge of the same character as our knowledge of logic, since an extension of a theory (in this case the 'theory' of second order logic) by mere definitions cannot have a different epistemic status from the theory of which it is an extension. If the original theory consists of analytic truths, so also must the extension; if our knowledge of the truths of the original theory is for this reason a priori, so also must be our knowledge of the truths of its definitional extension. The uncontroversial point for traditional formulations of the doctrine is that a reduction of this kind secures the *sameness* of the epistemic character of arithmetic and logic, while allowing for some flexibility as to the nature of that epistemic character. Thus, it is worth remembering that in *Principles* (p. 457), Russell concluded that a reduction of mathematics to logic would show, contrary to Kant, that logic is just as *synthetic* as mathematics.

Nevertheless, the methodology underlying this approach to securing the a prioricity of arithmetic by a traditional logicist reduction has been challenged. For example, Paul Benacerraf, who focuses on Hempel's[1] classic exposition, tells us that

> . . . logicism was . . . heralded by Carnap, Hempel . . . and others as the answer to Kant's doctrine that the propositions of arithmetic were synthetic a priori. . . . in reply to Kant, logicists claimed that these propositions are a priori because they are analytic—because they are true (or false) merely "in virtue of" the meanings of the terms in which they are cast. . . .
>
> According to Hempel, the Frege-Russell definitions . . . have shown the propositions of arithmetic to be analytic because they follow by stipulative definitions from logical principles. What Hempel has in mind here is clearly that in a constructed formal system of logic (set theory or second-order logic plus an axiom of infinity), one may introduce by stipulative definition the expressions 'Number,' 'Zero,' 'Successor' in such a way that sentences of such a formal system using these introduced abbreviations and which are formally the same as (i.e., spelled the same way as) certain sentences of arithmetic—e.g., 'Zero is a Number'—appear as theorems of the system.

1. C.G. Hempel, 'On the nature of mathematical truth,' in Hilary Putnam and Paul Benacerraf (eds.) *The philosophy of mathematics: selected readings*, 2nd ed. (Cambridge University Press, 1983), pp. 377–393.

> He concludes . . . that these definitions show the theorems of arithmetic to be mere notational extensions of theorems of logic, and thus analytic.
>
> He is not entitled to that conclusion. Nor would he be even if the theorems of logic in their primitive notations were themselves analytic. For the only things that have been shown to follow from the theorems of logic by [stipulative definitions] are the abbreviated theorems of the logistic system. To parlay that into an argument about the propositions of arithmetic, one needs an argument that the sentences of arithmetic, in their preanalytic senses, mean the same (or approximately the same) as their homonyms in the logistic system. That requires a separate and longer argument.[2]

Benacerraf is questioning whether the logicist can claim to have established *any* truth of arithmetic on the basis of a successful reduction. What is required according to Benacerraf, is a supplementary argument showing that the logicist theorems have the pre-analytic meanings of their ordinary arithmetical analogues. But Benacerraf's demand for a further argument is not justified. The charm of logicism derived from the fact that it was thought implausible that the concepts and laws of logic could have an 'arithmetical content'. To have successfully dispelled this belief it would have been sufficient to have shown that the concepts of logic allow for the explicit definition of notions which, on the basis of logical laws alone, demonstrably satisfy the basic laws of arithmetic. The philosophical impact of the discovery that the concepts and laws of logic have an arithmetical content in this sense would not have been in any way diminished by the observation that the pre-analytic meanings of the primitives of arithmetic were not the same as their logicist reconstructions. The sense in which the logicist thesis must be understood in order to be judged successful cannot therefore be the one for which Benacerraf claims Hempel must argue.

Notice also that independently of one's view of meaning and truth in virtue of meaning, it must be conceded that traditional logicism would have provided a viable answer to Kant if it had succeeded in showing that arithmetical knowledge requires only an extension of logic by explicit definitions. Hempel's appeal to these notions addresses a different issue: Frege left the problem of securing the epistemic basis of the laws of logic largely untouched. Benacerraf's Hempel should be understood as proposing to fill this gap by suggesting that the laws of logic are true in virtue of the meanings of the logical constants they contain. Like Frege, Hempel seeks to secure the a prioricity of arithmetic by an argument that proceeds from its analyticity. But Hempel's version of logicism differs from Frege's, for whom 'analytic' merely meant belonging to logic or a definitional extension of logic, by providing a justification for the analyticity of logical laws: logical laws are analytic, not by fiat as on Frege's account, but because they are true in virtue of the meanings of the logical terms they contain. From this it would follow that if logical laws are true in virtue of meaning, so also is any proposition established solely on their basis, where 'established solely on their basis' is intended to encompass the use of

2. 'Frege: The last logicist,' in William Demopoulos (ed.) *Frege's philosophy of mathematics* (Harvard University Press, 1995), pp. 42 and 46.

explicit definitions. The clarity of the thesis that the laws of logic are true in virtue of meaning is therefore central to Hempel's presentation of the view. Also central is the substantive and further claim that the basic laws of arithmetic can be recovered within a definitional extension of logic.

The implied criticism of Hempel's appeal to truth in virtue of meaning gains its force from the difficulties that stand in the way of establishing the traditional logicist thesis that arithmetic *is* reducible to logic in the original sense of the doctrine. Certainly, the failure to sustain this thesis led to more ambitious applications of the notion of truth in virtue of meaning. But if the basic laws of arithmetic *had* been recovered as a part of logic—not merely shown to have analogues that are part of some formal system or other, but to be part of *logic*—what more would be needed to infer that they share the epistemic status of logical laws? Once Hempel is *not* represented as seeking to secure the truth of the basic laws of arithmetic by an appeal to the derivability of *mere formal analogues* or a blanket appeal to the notion of truth in virtue of meaning, it is clear that he simply doesn't owe us the argument Benacerraf claims he does. The difficulties that attend traditional logicism are therefore not the methodological difficulties Benacerraf advances, but the simple failure to achieve the stated aim of showing arithmetic to be a definitional extension of logic. This point is obscured by Benacerraf's suggestion that the reduction might proceed from second order logic with an axiom of infinity or from some version of set theory. Neither theory supports the truth in virtue of meaning account that underlies Hempel's formulation of logicism. A reduction to second order logic with Infinity would mean a reduction to a system augmented with an axiom like Whitehead and Russell's; but *no one* ever thought such a system was true in virtue of meaning. As for a reduction to set theory, set theory is properly regarded as the arithmetic of the transfinite. Why should a reduction of the natural numbers to such a generalised arithmetic be regarded as a means of establishing its a prioricity on a less synthetic footing? The only coherent logicist methodology would therefore seem to be the one just outlined: to reduce arithmetic to a theory like *Begriffsshrift*'s. Unfortunately, such a theory is either too weak, or in the presence of Frege's theory of classes, inconsistent.

The renewed interest in logicism is based on the fact that the second order theory having Hume's principle[3] as its only non-logical axiom—'Frege arithmetic'—has a definitional extension which contains the Dedekind-Peano axioms. The neo-Fregean program of Crispin Wright and Bob Hale seeks to embed this logical discovery into a philosophically interesting account of our knowledge of arithmetic by subsuming Hume's principle under a general method for introducing a concept by an 'abstraction principle'.[4] This program explains

3. *Hume's principle* tells us that for any concepts F and G, the number of Fs is identical with the number of Gs if, and only if, the Fs and the Gs are in one-one correspondence.

4. By an *abstraction principle*, Hale and Wright mean the universal closure of an expression of the form $\Sigma(X) = \Sigma(Y) \leftrightarrow X \mathfrak{R} Y$, where $\mathfrak{R}$ is an equivalence relation, the variables X and Y may be of any type, and the function Σ may be of mixed type. In the case of Hume's principle, the equivalence relation is the (second order definable) relation on concepts of one-one correspondence, and the 'cardinality function' is a type-lowering map from Fregean concepts to objects.

the epistemological interest of the discovery that arithmetic is a part, not of second order logic, but of Frege arithmetic, by the program's account of concept-introduction. The key to achieving this goal is the idea that abstraction principles have a distinguished status: they are a special kind of stipulation. Their stipulative character shows them to be importantly *like* explicit definitions even if their creativeness suggests an affinity with axioms, and it is a central tenet of neo-Fregean logicism that abstraction principles are sufficiently like definitions to yield an elegant explanation of why arithmetical knowledge is knowledge a priori.

The neo-Fregean program has a methodological dimension that parallels the role of the theory of definition in traditional logicism. Frege accords a statement the status of a *proper* definition if it meets conditions of eliminability and conservativeness. The classical theory of definition is supplemented by the neo-Fregean methodology of *good abstractions*. Thus, the theory of definition mandates that a definitional extension must be conservative in the familiar sense of not allowing the proof of sentences formulated in the unextended vocabulary which are not already provable without the addition of the definitions which comprise the extension. But 'extensions by abstraction' need not be conservative in this sense; indeed interesting extensions are interesting precisely because they are *not* conservative in the sense of the theory of definition. The neo-Fregean theory of good abstractions allows for classically non-conservative extensions—extensions which properly extend the class of provable sentences—but imposes a constraint on the consequences an extension by good abstraction principles can have for the ontology of the theory to which they are added. This methodology is constrained and principled, it is just not constrained in the same way as the classical theory of definition. We can, perhaps, put the difference by saying that the constraints on definition have a more purely epistemic motivation than do the constraints the neo-Fregean imposes on good abstractions.

In my view, the reticence of the classical theory of definition to allow a mere definition to properly extend the theory to which it is added is well-founded, and should also inform the epistemic basis of a principle as rich as Hume's. My goal here is to consider whether the neo-Fregean account of Hume's principle as a kind of stipulation can support the epistemological claim of neo-Fregeanism to have secured the a prioricity, if not the analyticity, in one or another traditional sense of the notion, of our arithmetical knowledge. The matter is taken up by Hale and Wright in their paper 'Implicit definition and a priori knowledge'—and by Wright in his 'Is Hume's principle analytic?' which, notwithstanding its title, is not concerned to secure the analyticity of Hume's principle but to address the question of its epistemic status within the neo-Fregean program and the light that can shed on our arithmetical knowledge.[5] Wright and Hale use the stipulative character of Hume's principle as a premise in an argument for the a prioricity of our arithmetical knowledge. This becomes clear when we reflect on the fact that they are concerned to show that our knowledge of arithmetic can be represented as resting on a principle

5. Both reprinted in The *Reason's Proper Study*, as Chs. 5 and 13 respectively.

that introduces the concept of number. In acquiring the concept of number, we acquire a criterion of identity for *number*—a criterion for saying when the same number has been given to us in two different ways as the number of one or another concept. This criterion of identity—Hume's principle—affords the only non-logical premise needed to derive the basic laws of arithmetic. Our arithmetical knowledge is secured, therefore, with our grasp of the concept of number and is based on nothing more than what we acquire when we are introduced to the concept. But since this knowledge rests on a stipulation, it is unproblematically knowledge a priori.

This is essentially the same account of the philosophical interest of Frege's arithmetic that is elaborated by Fraser MacBride in two thoughtful papers[6] that address this issue. For MacBride the neo-Fregean explanation of the a prioricity of our knowledge of arithmetic runs as follows: We first stipulate a criterion of identity for a special kind of objects; call them cardinal numbers. That certain fundamental truths about these objects are established on the basis of a stipulation guarantees that our knowledge of those truths is knowledge a priori. This is to be contrasted with an account which would seek to infer the a prioricity of our knowledge of arithmetic from theses about meaning or truth in virtue of meaning. The neo-Fregean account does not depend on a traditional notion of analyticity; since neo-Fregeanism demands only the relatively uncontentious concession of the a prioricity of a stipulation, it can claim that its explanation of the a prioricity of arithmetic need not address the difficulties associated with defending traditional conceptions of analyticity.

The fact that the reduction to Frege arithmetic requires more than a merely definitional extension of second order logic suggests an objection to neo-Fregean logicism that is closely related to the one we saw Benacerraf urge against to Hempel: How, one might ask, does our knowledge of the truths that hold of the objects the neo-Fregean has singled out—the Frege-numbers—bear on our knowledge of the *numbers*, on the subject matter of *ordinary* arithmetic? In so far as the epistemological issues are issues concerning ordinary arithmetic, have they even been addressed by the neo-Fregean? In this form, the objection presupposes only preservation of subject-matter—a minimal requirement that it would be difficult to justify not meeting—and says nothing about preservation of meaning.

There are two neo-Fregean responses to this objection that I wish to review. The first response holds that it is because ordinary arithmetic can be 'modelled' in Frege arithmetic that the epistemological status of the truths of Frege arithmetic is shared by the truths of ordinary arithmetic. Wright remarks (p. 322) that this answer is too weak. And although MacBride does not endorse this response, neither does he reject it as altogether unlikely. Nevertheless, I think it is worth recording exactly why such a straightforward answer, couched in terms of the relatively unproblematic relation of modelling, can't be right.

It is clearly possible to stipulate the conditions that must obtain for the properties of a purely hypothetical and imaginary 'abstract' physical system

6. 'Finite Hume,' *Philosophia mathematica*, 8 (2000), pp. 150–159, and 'Can nothing matter?', *Analysis*, 62 (2002), pp. 125–134).

to hold without in any way committing ourselves to the existence—or even the dynamical possibility—of such a system. Our knowledge that such abstract systems are configured in accordance with our stipulations is no less a priori than our knowledge that, for example, the four element Boolean algebra has a free set of generators of cardinality one. But it sometimes happens that abstract configurations 'model' actual configurations, in the sense that there is a correspondence between the elements of the two systems that preserves fundamental properties. It is clear that in such circumstances we take ourselves to know more than that an imaginary example has the properties we stipulate it to have: if the example is properly constructed, we know the dynamical behaviour of a part of the physical world. But of course the fact that an actual system is 'modelled' by our imaginary system, together with the fact that our knowledge of the properties of our imaginary system is a priori knowledge because it depends only on our free stipulations, are completely compatible with the claim, obvious to pre-analytic intuition, that our knowledge of the dynamical behaviour of the actual system is a posteriori. Whatever role stipulation may have in fixing the properties of the abstract system by which the behaviour of some real process is modelled, it lends no support to the idea—and would never be regarded as lending support to the idea—that our knowledge of the real process is knowledge a priori.

There is a disanalogy between the number-theoretic case and our example that might seem to undermine its effectiveness as a criticism. In the number-theoretic case the existence of the correspondence between ordinary numbers and the 'Frege-numbers' that model them is known a priori. But the correspondence between the abstract system of our example and the actual system is not known a priori; it depends on the a posteriori knowledge that there are in reality configurations of particles having the postulated characteristics. This is of course entirely correct. However it is of no use to the neo-Fregean, since to know a priori that there is a mapping between the ordinary numbers and the Frege-numbers it is necessary to have a priori knowledge of the existence of the domain and co-domain of the mapping. To be of any use to the neo-Fregean, the fact that the Frege-numbers model the ordinary numbers therefore requires that our knowledge of the ordinary numbers be a priori. But if the modelling of the ordinary numbers by the Frege-numbers presupposes that our knowledge of the ordinary numbers is a priori, it cannot be part of a non-circular account of why ordinary arithmetic is known a priori.

The general point may be put as follows: The fact that M models $\mathcal{N}$, so that for any sentence s, s is true in M if and only if s is true in $\mathcal{N}$, does not entitle us to infer that because the sentences true in M are known a priori, the sentences true in $\mathcal{N}$ are known a priori. Indeed it is perfectly possible that (with the obvious exception of the logical truths) our knowledge of sentences true in $\mathcal{N}$ is wholly a posteriori. So even if we grant that an assumption rich enough to secure an infinity of objects is correctly represented as a stipulation, it remains unclear how the neo-Fregean can use this fact to answer the question which motivates his account of arithmetical knowledge—it remains unclear how it yields an account of *our* knowledge of the numbers, knowledge that we have independently of the neo-Fregean analysis. Notice that this objection depends only on an observation about the modelling of one domain by another, and that, in particular, it does not require the resolution of various difficult issues in the theory of meaning.

Let us turn now to the second neo-Fregean response. This is an alternative to the response based on modelling. Here the idea is that since Frege arithmetic captures the 'patterns of use' exhibited by our ordinary number-theoretic vocabulary, both in pure cases and in applications, we are justified in inferring not merely that the Frege-numbers *model* the ordinary numbers but that the Frege-numbers *are* the ordinary numbers. Clearly, showing that Frege arithmetic captures the patterns of use of our ordinary number-theoretic vocabulary constitutes a considerable strengthening of the claim that ordinary arithmetic is merely modelled by Frege arithmetic.

Suppose we grant, both that Frege arithmetic captures the patterns of use of our ordinary number-theoretic vocabulary and that because of this, ordinary arithmetic and Frege arithmetic share the same subject matter. Vindicating the claim that ordinary arithmetic and Frege arithmetic share a subject matter is only one of the difficulties that the weaker understanding of the view in terms of modelling fails to address. If we are only modelling ordinary arithmetic, it is unproblematic to hold that as a statement of the modelling theory Hume's principle is known a priori because it is a mere stipulation. The difficulty, as we saw, is that this fails to transfer to the a prioricity of the truths we are modelling—to the truths of ordinary arithmetic. Is this difficulty removed when the neo-Fregean account is extended to one which claims to capture the patterns of use of our ordinary number-theoretic vocabulary? And does it illuminate the epistemic status of the basic laws of arithmetic to observe that in the neo-Fregean reconstruction of our patterns of use Hume's principle has the status of a stipulation?

When neo-Fregeanism is understood to preserve our 'patterns of use', it becomes virtually indistinguishable from the traditional idea that the account of the numbers given by Frege arithmetic is *analytic of* the ordinary notion of number, so that a major burden of the account now falls on establishing the adequacy of an analysis in something very much like the traditional sense. This is something the neo-Fregean had sought to avoid, since once the neo-Fregean has to defend the idea that the patterns of use of ordinary numerical expressions have been captured, the simplicity of urging the stipulational character of Hume's principle, and then basing the a prioricity of arithmetic on this footing, has been lost: the principle no longer governs the introduction of a new concept but is constrained to capture an existing one. But let us grant both that sameness of pattern of use implies sameness of reference and that Frege arithmetic does in fact capture the pattern of use of our ordinary arithmetical vocabulary and, therefore, articulates a successful reconstruction of our arithmetical knowledge. Since we have given up the idea that Hume's principle is being used simply to introduce a new concept, but forms part of an attempt to articulate principles that capture our numerical concepts as they are given by the patterns of use of our ordinary number-theoretic vocabulary, its justification does not consist merely in its being laid down as a stipulation. Rather, Frege arithmetic is justified because it captures the fundamental features of the judgements—pure and applied—that we make about the numbers.

For the neo-Fregean, the reconstruction must not only capture an existing concept by recovering the patterns of use to which our arithmetical vocabulary conforms, it must also illuminate the epistemic status of our pure arithmetic knowledge. Having the status of a stipulation is not, of course, a characteristic of Hume's principle that is recoverable from our use of our arithmetical vocabulary, but is something the reconstruction imposes on the principle in order to illuminate the basis for our knowledge of the propositions derivable from it. But it is unclear what is achieved if one has captured the pattern of use of an expression by a principle that—in the reconstruction of the knowledge claims in which that expression figures—is regarded as a stipulation. Does this confer the epistemological characteristics that the notion of a stipulation is supposed to enjoy on the knowledge claims that have been reconstructed? To establish that the epistemic basis for the knowledge these judgements express resides in the stipulative character the neo-Fregean analysis assigns to Hume's principle, it is not enough to show that Frege arithmetic captures patterns of use. The essential point is not all that different from what we have already noted when discussing the response based on modelling, and can be seen by an example that is not all that different from the one cited in that connection. To see this, suppose the world were Newtonian. We could then give a reconstruction of our knowledge of the mechanical behaviour of bodies by laying down Newton's laws as stipulations governing our use of the concepts of force, mass and motion.[7] But the fact that in our reconstruction the Newtonian laws have the status of stipulations would never be taken to show that they are in any interesting sense examples of a priori knowledge. Why then should the fact that the neo-Fregean represents Hume's principle as a stipulation be taken to show that arithmetic is known a priori? The neo-Fregean reconstruction of the patterns of use of expressions of arithmetic leaves the epistemic status of the basic laws of arithmetic as unsettled as it was on the suggestion that Frege arithmetic merely *models* ordinary arithmetic. Neither reconstruction supports the epistemological claim of the neo-Fregean to have accounted for the a prioricity of our knowledge of arithmetic. Whether that account is put forward as a theory within which ordinary arithmetic can be modelled, or whether it is said to capture the patterns of use of our number-theoretic vocabulary, it fails to have the direct bearing on the epistemic basis of our arithmetical knowledge that the neo-Fregean supposes it to have. Showing that Hume's principle is correctly represented as a stipulation may be one route to securing it as a truth known a priori, but it is questionable whether, proceeding in this way, the task of revealing the proper basis for the a prioricity of arithmetic is made any easier than it would be by general reflection on why Hume's principle is plausibly represented as a truth.

7. There are of course well-known historical examples along these lines. Cp. Ernst Mach's *The Science of Mechanics* (Open Court, 1960, 6th American edn., translated by Thomas J. McCormack), whose famous definition of mass (p. 266) even has the form of an abstraction principle. Thanks to Peter Clark for calling my attention to Mach's rational reconstruction of Newtonian mechanics.

Putting to one side the problem of establishing the a prioricity of arithmetic on a correct basis, a compelling argument that Frege arithmetic captures pre-analytic intuitions about the numbers can be extracted from the neo-Fregean corpus: Since the Dedekind-Peano axioms codify our pure arithmetical knowledge, their derivability constitutes a condition of adequacy which any account of our knowledge of number should fulfil. By Frege's theorem, Frege arithmetic fulfils this condition of adequacy. But what makes Frege arithmetic an interesting *analysis* of the concept of number is that it not only yields the Dedekind-Peano axioms, but derives them from an account of the role of the numbers in our judgements of cardinality—from our foremost *application* of the numbers. As such, it is arguably a compelling *philosophical* analysis of the concept of number since, as Wright has observed, one can show that the Frege-number of Fs = n if, and only if, there are, in the intuitive sense of the numerically definite quantifier, exactly n Fs.[8] But once the project of securing a correct analysis is divorced from the project of securing a body of truths as analytic or a priori, neither the fact that Frege arithmetic satisfies our condition of adequacy nor the fact that it connects the pure theory of arithmetic with its applications—essential as each is to securing it as a correct analysis of number—addresses the question of the *epistemic status* of our knowledge of arithmetic. This conclusion is not particularly surprising. Both neo-Fregean strategies we have been considering are variants on the methodology of reconstruction associated with Carnap. For Carnap the thesis that arithmetical knowledge is non-factual, and therefore, a priori, was not in serious doubt. And since the aim of a reconstruction is simply to delimit more precisely the extension of a predicate, we should never have expected that a Carnapian reconstruction of arithmetical knowledge would in any way *justify* the claim that our knowledge of arithmetic is a species of a priori knowledge. It is precisely in respect of their epistemological significance that Fregean logicism and neo-Fregean logicism—reduction to logic by explicit definition vs. reconstruction by Frege arithmetic—come apart.[9]

8. See *The Reason's Proper Study*, p. 251 and pp. 330ff.
9. Support for research by the Social Sciences and Humanities Research Council of Canada is gratefully acknowledged.

PLATONISM, SEMIPLATONISM AND THE CAESAR PROBLEM

GIDEON ROSEN
Princeton University

1

We learn arithmetic in the ordinary way, and somehow we emerge convinced that Julius Caesar is not a number. This is moderately remarkable, since no one ever *tells* us anything of the sort. And for this reason alone it would be moderately interesting to know where this conviction comes from and whether it can be justified.

In *The Foundations of Arithmetic* Frege makes a stronger claim for what has come to be called 'The Caesar Problem'. Frege apparently holds that any philosophical account that fails to vindicate common sense on this point is automatically, for that very reason, unacceptable. But why should this be? To be sure, insofar as we are convinced from the outset that no concrete object is a number, any account that fails to ratify this verdict will be to some degree revisionary. But so what? It would be one thing if our emerging philosophy called for significant revision in mathematics. But our conviction on the Caesar problem—however firm—is almost completely epiphenomenal. Within mathematics the question never arises and our answer to it never matters. So while a philosophy that left it open whether Caesar was a number would admittedly clash with received opinion, in this case the clash would be maximally benign. It would leave everything that matters just as it is, or so it seems.[1]

To make this vivid and to set the stage for a discussion of the significance of the Caesar problem, let me begin with a sketch of a moderately appealing philosophy of arithmetic which yields a deviant a verdict about the status of the Caesar sentences,[2] but which is otherwise non-revisionary.

2

You are doing fieldwork for your degree in comparative mathematics when you stumble across a hitherto unstudied tribe. They call themselves the Semiplatonists, and they appear to speak a version of English. Indeed, they appear to believe almost everything we believe. Their science and mathematics, in

1. It should be stressed that Frege's main objection to definitions that fail to secure the falsity of the Caesar sentences is not that they are revisionary, but rather that they fail to define a *sharply bounded* concept of Number. The account to be sketched below fails to satisfy this Fregean demand. So much the worse for the demand, in my opinion.
2. I.e., sentences of the form 'The number of Fs = q', where 'q' names an ordinary concrete object.

particular, are completely orthodox. They count and calculate just as we do. Our scientists and mathematicians can read their journals with profit and vice versa. Given their longstanding isolation, you conclude that you have hit upon a stunning case of convergent cultural evolution.

In an effort to gauge the extent of this convergence you decide to probe around the margins. After eliciting orthodox verdicts on the Peano axioms and the like, you ask your informants to assign a truth-value to 'Caesar is the number 0', and they respond without hesitation: 'The sentence has no truth-value.' Upon further investigation it emerges that their attitude towards the Caesar sentences is much like our attitude towards trans-theoretic mathematical identities of the sort first emphasised by Benacerraf: '$3 = \{\{\{\phi\}\}\}$', '$(a, b) = \{\{a\}, \{a, b\}\}$', 'The real number 2.00000 . . . = the rational number 2/1', etc.[3] Mathematical common sense does not regard such claims as clearly false. It regards them as somehow optional. Nothing in our understanding of the language of mathematics forces such identifications upon us, and yet nothing precludes them either. In these cases the identifications are sometimes useful, and so they are sometimes 'adopted', for example in the early chapters of a textbook. But the adoption of such identities always has the character of a temporary stipulation, and it is natural to suppose that in the absence of any such convention the Benacerraf sentences are neither true nor false.

Our intuitive reactions thus distinguish between the Caesar sentences (clearly false) and the Benacerraf sentences (somehow optional). The Semiplatonists see no difference. They may say that in the Caesar case they can see no point whatsoever in adopting the identity, just as they can see no point in settling a precise geographical boundary for the Matterhorn. But their official view is that such stipulations are always 'permissible', never 'obligatory'.

3

This is a remarkable divergence, and so you wonder what might explain it. You have no useful insight (as yet) into how we come by our view about the Caesar sentences. But you have no trouble discovering how they come by theirs. It turns out that mathematical pedagogy among the Semiplatonists follows the logical order of things as they conceive it. Before they learn arithmetic their children are introduced to a second-order formal language for the purposes of regimenting the English they already know. They learn rudimentary aspects of its (classical) model theory and a system for constructing proofs, so that they can begin to assess arguments as valid and invalid. At this stage, however, they know nothing of arithmetic.

Arithmetic is introduced in the neo-Fregean style by the explicit stipulation that $N^{=}$ is to hold:

3. Paul Benacerraf, 'What Numbers Could not Be', *Philosophical Review*, 74 (1965), pp. 47–73; for the case of ordered pairs, see Philip Kitcher, 'The Plight of the Platonist', *Nous*, 12 (1978), pp. 119–136. I owe the last example to John Burgess, who objects to the use I make of it. (Burgess thinks that all of these transtheoretic identities are simply false.)

($N^=$) $(F)(G)\ \mathrm{N}xFx = \mathrm{N}xGx$ iff there is a one-one function $f: F \rightarrow G$.

The young Semiplatonists are instructed to regard the stipulation as an *implicit definition* of the new word 'N'. They already understand the right hand side, and it is supposed to be a consequence of this stipulation that the left hand side and the right hand side come to *say the same thing*, in some sense of this phrase. This in turn is supposed to guarantee that $N^=$ expresses a necessary truth. Indeed it is supposed to guarantee that $N^=$ has the status of an analytic truth as traditionally conceived. As the Semiplatonists understand the matter, to doubt whether '$\mathrm{N}xFx = \mathrm{N}xGx$' is true when there are just as many *F*s as *G*s is to fail to appreciate the main rule governing the use of the symbol 'N'. And yet it is also supposed to be a consequence of the stipulation that the N-terms are syntactic singular terms and that statements of the form '$\mathrm{N}xFx = \mathrm{N}xGx$' are genuine identities with '=' meaning what it has always meant.

The most important questions about Fregean Platonism concern the cogency of this transition. But, rightly or wrongly, the Semiplatonists do not balk. They accept $N^=$; they regard it as an implicit definition; they insist that a nominalist who doubts a numerical identity while accepting the corresponding claim of one-one correspondence is simply confused about how the N-terms are to be used, and so on.

At this point the initiates are introduced to the standard explicit definitions of the numerals and the basic arithmetical operations, and they are shown the proof of Frege's theorem: the derivation of the Peano axioms from $N^=$. From then on, their mathematical education converges more or less with ours. They learn the standard algorithms for calculation and the various routines for applying arithmetic to empirical problems. The difference is that unlike most of us, they are always in a position to analyse these applications and to prove the soundness of the underlying reasoning from first principles.

4

Having mastered all of this, the initiate is encouraged to reflect. He knows that $N^=$ is supposed to be an implicit definition of 'N'. But what does it really tell him about this word? $N^=$ tells him that 'N' denotes a function that satisfies the following condition:

$\phi xFx = \phi xGx$ iff there is a one-one function $f: F \rightarrow G$.

Call any such function a **numerator**. The Semiplatonist notices that numerators exist only if the domain of individuals is infinite, and since he accepts $N^=$ he concludes that the domain must indeed be infinite. However he also notices that if there is one numerator, there are many. And on reflection he can see no basis for privileging one over the others. As Quine might have said, any numerator will do. He acknowledges that if the community could pick out a single numerator it might supplement $N^=$ with the stipulation that henceforth 'N' is to denote it. But he has heard nothing about such a stipulation, and he can't see what point it would serve. So he concludes (and his teachers confirm the fact) that the choice has not been made, and hence that 'N' exhibits a form of semantic indeterminacy.

In order to cope with this indeterminacy, he constructs a (to us) familiar sort of semantic theory. Begin with the harmless idealisation that his original language L had a single correct interpretation M: a function from L-words to denotations of the appropriate types. Say that an interpretation of the new language is admissible just in case it agrees with M on the old words and assigns a numerator—any numerator—to 'N'. There will be admissible interpretations on which the range of 'N' consists exclusively in necessarily existing abstract objects. But there will also be admissible interpretations on which at least some of the N-terms denote ordinary concrete things.

According to the Semiplatonist, the class of admissible interpretations encodes everything there is to know about the referential semantics of the extended language. It therefore makes no sense to ask what a term like 'Nx $x \neq x$' *really* denotes. It denotes different objects on different admissible interpretations, and that's that. This is a familiar thought in other areas. To revert to Quine's old example, the name 'Mont Blanc' *divides its reference* over a range of massively overlapping mounds of rock and snow, and there is no straight answer to the question, 'Which of these things does the name denote?'. The difference is that in this case the range of indeterminacy is unlimited.[4] For each object A there is an admissible interpretation relative to which 'N$xx \neq x$' denotes A. This reflects the fact that in conjunction with the underlying facts about one-one correspondence, the stipulation that $N^{=}$ is to hold constrains *joint assignments* of denotations to N-terms while placing no constraints on individual assignments. If there are just as many sheep as goats, then $N^{=}$ tells us that M^{*} is admissible only if it assigns the same denotation to 'Nx sheep x' and 'Nx goat x'. It tells us nothing, however, about which object these terms must refer to.

The Semiplatonist has several options when it comes to defining truth for a language involving semantic indecision of this sort. The most straightforward is to say that a sentence in the extended language is true (false) if true (false) on every admissible interpretation, and that otherwise it lacks a truth-value. On this view, $N^{=}$ and the old truths of L will all be true, as will their (classical second order semantic) consequences. The Caesar sentences, however, will uniformly lack a truth-value.

The Semiplatonists *officially accept* this interpretation of the language of arithmetic. That's why they agree that the Caesar sentences lack a truth-value. As they see it, this verdict is the natural upshot of the fact that $N^{=}$ says

4. That's one difference. Another is that in this case the class of admissible interpretations is sharply bounded. There is no higher order 'indeterminacy' in the picture.

Incidentally, it should be said that 'indeterminacy' is strictly speaking a misnomer in the present context. The facts concerning which models are admissible are all perfectly determinate, as are the facts about denotation relative to a model. A question of the form, 'Does 'NxFx' denote A?' is not a well-formed question whose answer is somehow unsettled. It is a question with a false presupposition. In a metalanguage adequate to the case, all such questions are refined as follows. Instead of asking whether t denotes A, we ask whether t denotes A relative to all (some/some particular) admissible interpretation. Well-formed questions of this sort all have perfectly determinate answers.

all there is to say about how the N-terms are to be interpreted, together with certain natural (though perhaps not inevitable) assumptions about how to cope with semantic indecision.

5

By his own admission the Semiplatonist cannot *solve* the Caesar Problem. He cannot argue from principles implicit in his understanding of the arithmetical vocabulary that Julius Caesar is not a number. But that does not trouble him. He doesn't think the problem needs a solution in this sense. And that raises two questions: Is the Semiplatonist right to think that the problem cannot be solved on the basis of the Fregean materials available to him? And is he right in thinking that no solution is required?

The Fregean Platonist thinks that the Semiplatonist is wrong on both counts. Wright and Hale believe that not only is it urgent to ratify common sense about the Caesar sentences, the stipulation that $N^=$ is to hold provides the resources for doing so all by itself.

More specifically, the Fregeans maintain that the explanation of 'N' by means of $N^=$ suffices to introduce a **pure sortal concept** of Number, which determinately excludes concrete objects from its extension. This is perhaps the most crucial point of contrast between Fregean Platonism and its Semiplatonist variant. The Semiplatonist grants that the open sentence '$\exists F\, y = \mathrm{N}xFx$' ('Number y', for short) is perfectly meaningful. It figures in a range of true sentences and valid inferences. There is a clear difference between the competent adult who knows how to use it and the child who does not. So there is a skill or competence that deserves to be called 'understanding the word 'Number''. And since the word is a syntactic predicate, the Semiplatonist may allow that there is a sense in which it may be said to *express a concept*. But on reflection, it's not much of a concept. It is certainly not a pure sortal in the sense in which Wright and Hale understand the notion. Relative to an admissible interpretation, 'Number' has a determinate extension. But these extensions are utterly heterogeneous. Obviously enough, there is no real principle of classification here. Given any object A, there will be no (non-relative) fact of the matter as to whether 'Number' applies to it. For the Semiplatonist 'Number' clearly fails to pick out a genuine *kind of thing*. It makes no good sense to ask, of an object X, what makes it a Number. It makes no good sense to ask what explains the identity of X and Y when X and Y are Numbers, and so on, where these questions are understood *de re*. It follows that if $N^=$ does serve to introduce a pure sortal concept of Number, the Semiplatonists have radically underestimated the force of the original stipulation.

There is of course a weak sense in which 'Number' expresses a sortal concept even for the Semiplatonist. It functions grammatically as a 'count noun'. Given any condition ϕ, it always *makes sense to ask*, 'How many numbers satisfy ϕ?', and in many cases the question will have an answer. (Not always: Consider, 'How many numbers have conquered Gaul?'.) The Semiplatonist allows, for example, that we can always meaningfully ask and answer questions about the number of Numbers satisfying any purely arithmetical condition. So if that's all it takes for a predicate to express a sortal, then 'Number' expresses a sortal even for the Semiplatonist. The Wright/Hale notion of a *pure* sortal is however clearly more demanding. A pure sortal picks out an essential feature of the objects to which it applies (p. 387). Its associated criterion of identity tells us *what it is* for X and Y to be identical when X and Y are instances of the concept in question, and so on. However these metaphysical notions are to be understood, it should be clear that 'Number' as the Semiplatonist conceives it does not come close to expressing a pure sortal concept in this sense of the notion.[5]

I have suggested that arithmetic among the Semiplatonists is just like ours in every respect worth fighting for. Not only is the substance of pure arithmetic preserved. The techniques for counting and calculating and applying arithmetic are just the same; the truths of pure arithmetic are intelligibly necessary

5. Wright and Hale distinguish between mere syntactic count nouns and count nouns that correspond to sortal concepts even in this weak sense. 'Yellow object' is a count noun in the syntactic sense. It has a plural; statements of the form 'There are n yellow objects in the bowl' are well-formed, etc. Nonetheless, they maintain, such predicates do not express sortals even in the weak sense because they are not 'associated' in the right way with criteria of identity. They write:

> [I]t is the lack of (the right kind of) association with contexts of identity of the concepts expressed by noun phrases such as 'yellow thing', 'large object', and so on, which may make for indeterminacy in questions like 'How many yellow things are there (in this bowl)?' The question requires antecedent understanding of what kind of yellow thing—bananas, maybe—is relevant to its intent. In a case where no such specification is given . . . an expression of the form 'The number of Fs' need have no specific reference. This is just a corollary of Frege's famous insight . . . that statements of number are predications of—are relative to—a concept:
>
> > While looking at one and the same external phenomenon I can say with equal truth both "It is a copse" and "it is five trees".
>
> Since both copses and trees may be correctly describes as "green", the question, "How many green things are there over there?" takes on the same relativity and is disambiguated only by an understanding that it is green Fs that are relevant, for some appropriate sortal F. (pp. 386–7)

This is a familiar line of thought, but I should like to go on record as suggesting that it is a muddle. Statements of the form 'There are n green things over there' may indeed be indeterminate, *but only because the adjective 'green' is vague*, and not because 'green thing' is not a sortal predicate. Statements like 'There are four people over there' (gesturing towards the personal identity lab) can be indeterminate for exactly the same reason, even though 'person' is indisputably a sortal term. Prescinding from this vagueness, the question 'How many green things are there over there?' (gesturing copseward) has a perfectly determinate answer. There are five trees—that's five—and the copse—that makes six. Since the copse is not identical with any of the trees, and since the copse and the trees are all green things, there must be at least six green things in the vicinity. But of course we're not done. Consider the leaves, the chloroplasts, the arbitrary undetached green parts of these green things, etc. These things are also green, and they are all distinct from one another. So if we prescind from the vagueness of 'green' the answer to the question is perfectly definite, albeit large. Indeed, it is almost certainly infinite. I don't believe there are examples of syntactic count nouns for which the question 'How many Fs are there?' *makes no sense* unless some background sortal is specified. So I reject the distinction between mere syntactic count nouns and sortal predicates in the weak sense.

and a priori and for all intents and purposes analytic (if the story about the force of implicit definition is correct), and so on. To say this is to say that it would be no disaster if it turned out that *we* possess no pure sortal concept of number, or that Fregean Platonism cannot supply one. But still we should ask the question: Does the stipulation that $N^{=}$ is to hold suffice for the introduction of such a concept? If it does, Semiplatonism is incoherent.

6

In an astonishingly fertile essay[6] Wright and Hale offer two arguments for the conclusion that someone who has been introduced to 'N' by way of $N^{=}$ is in a position to refute the Caesar sentences a priori. The second of these two arguments—the sortal exclusion argument of sections 6 and 7—*assumes as a premise* that 'Number' picks out a pure sortal. Since that is what we are presently seeking to establish, this argument has no force against the Semiplatonist. The first argument—the so-called "Simple Argument"—involves no such presumption and is therefore of greater interest for present purposes.

As I shall reconstruct it, the Simple argument has two parts. The first part seeks to show that *modal common sense* about the Numbers is incompatible with the assumption that Julius Caesar is a number. The second part seeks to show that the Fregean explanation of Number entails the relevant modal verdicts. Together these two parts entail that information available to the Semiplatonist suffices for a demonstration of the determinate falsity of the Caesar sentences.

Here's a rough sketch of part 1. Suppose that in the actual world there are four knives on the table, and suppose for reductio, that Julius Caesar is the number 4. This means that in the actual world, the number of knives on the table is Julius Caesar. Now consider a world W in which Julius Caesar does not exist, but in which the knives are arranged just as they actually are. Then we know that in W, the number of knives is 4, and that in W, the number of knives is not Julius Caesar. But we also know—don't we?—that if W had been actual, the number of knives would then have been exactly what it actually is. So if the number of knives is not Julius Caesar in W, it is not Julius Caesar in actuality. Contradiction.

The argument is completely general. If it is sound it refutes any determinate identification of the numbers with contingently existing entities. It is hard to know how it should be formalised, since the crucial modal premise is not expressible in the standard modal languages. But here is a slightly more explicit version to get us started.

As background assumptions we have:

(1) In α, $\mathrm{N}xKx = 4$

(2) In W, $\mathrm{N}xKx = 4$

6. 'To Bury Caesar', in Crispin Wright, and Bob Hale, *The Reason's Proper Study* (Oxford University Press, 2001).

(3) In α, Julius Caesar exists.

(4) In W, Julius Caesar does not exist.

Now suppose for reductio:

(5) In α, Julius Caesar = N*xKx*

From (2) and (4) it is supposed to follow that:

(6) In W, Julius Caesar ≠ N*xKx*.

This transition assumes an important principle, to which we shall return:

(EX) If in W, N*xFx* = *A*, then in W, *A* exists.

Now (1) and (2) together intuitively entail (7):

(7) There are just as many Ks in α as there are Ks in W.

This is the thought whose formalisation is not straightforward. If we had a binary predicate 'x is K in w', then we might try to speak of one-one correspondence between two concepts: *x is K in* α and *x is K in W*. But the present formulation treats 'K' as a one-place predicate and W as part of what is in effect a world-indexed modal operator. Alternatively, we might say that some substitution instance of '$\exists_n xKx$' or 'N*xKx* = *n*' is true both in α and in W. But intuitively neither semantic ascent nor explicit quantification over transworld functions is required to express the crucial thought that if W had been actual, there would then have been just as many knives as there actually are, and that is what (7) is meant to convey.

The crucial modal principle is then as follows:

(8) If there are just as many *F*s in *w* as there are *G*s in *w**, then there exists a (unique) object *A* such that in *w*, N*xFx* = *A* and in *w**, N*xFx* = *A*.

From (7) and (8) we have (9):

(9) There exists a (unique) object *A* such that in α, *A* = N*xKx* and in W, *A* = N*xKx*.

But now we have a contradiction (on the assumption that 'Julius Caesar' is rigid). (5) and (9) together entail (10):

(10) In W, Julius Caesar = N*xKx*

And that is inconsistent with (6).

The crucial premise is clearly (8). (8) is an object language analogue of the semantic claim that terms of the form '$\text{N}xFx$' function as what Wright and Hale call **quasi-rigid** designators. On any reasonable account, the N-terms are not in general rigid. The denotation of '$\text{N}xFx$' will sometimes vary from world to world. But intuitively it cannot vary unless the cardinality of F also varies. This suggests the following semantic principle:

(QR) If there are just as many *F*s in w as there are *G*s in w^*, then the denotation of '$\text{N}xFx$' relative to w is the same as the denotation of '$\text{N}xGx$' relative to w^*.

Since the other assumptions we have made are relatively uncontroversial (and indeed congenial to the Semiplatonist), we may conclude that the Simple Argument is sound if and only if (QR) is true.

7

This reconstruction raises a number of questions: How compelling are (8) and its semantic correlate (QR)? What would be the cost of abandoning them? And is the Fregean Platonist in a position to endorse these principles?

At this point it may help to have in mind a philosophical position that emphatically rejects QR. Unlike Semiplatonism, the position I am about describe is not meant to sound plausible or attractive. It is just meant to show how much common sense modal verbiage we can retain even if QR is rejected.

The **accidental Platonist** accepts $\text{N}^=$ as a preliminary explanation of 'N', and so regards $\text{N}^=$ itself as a necessary truth. He notes (with the Semiplatonist) that these commitments do not settle which objects the N-terms denote. He accepts (EX), and so holds that '$\text{N}xFx$' denotes A relative to w only if A exists in w. This is a further constraint, but it's still not enough impose determinate reference upon the N-terms. Loathe to accept rampant indeterminacy, the accidental Platonist therefore posits a **selection function** which assigns world-relative denotations to the N-terms *arbitrarily but in conformity with* $N^=$. At any given world, '$\text{N}xFx$' and '$\text{N}xGx$' determinately denote the same existing object iff the *F*s and *G*s are equinumerous *at that world*. But there is no requirement of transworld uniformity in the mapping. So it may be (and let's suppose it so) that '$\text{N}xKx$' and '$\text{N}x\ x \leq 3$' both refer to Julius Caesar relative to the actual world, but to the empty set relative to W. This capriciousness ensures that the accidental Platonist rejects QR.

Now the accidental Platonist will simply reject the Simple Argument. He believes that in the most straightforward sense, the object that is in fact the number of knives would not have been the number of knives if Julius Caesar had not existed, *even if there would then have been just as many knives as there actually are*. Still he is not without resources for expressing versions of the thought that the number of knives depends only on how many knives there happen to be.

Note first that the Accidental Platonist can accept a *metalinguistic surrogate* for quasi-rigidity. Suppose there are four knives in α, and hence that in α, N*xKx* = 4. Now consider a world W in which Julius Caesar does not exist but the knives are just as they are in α. The Accidental Platonist does not believe that there is a single object that is the number of knives in both worlds. But he does believe that there is a single *numeral*—namely '4'—which *names* the number of knives relative to both worlds. So one sense he can attach to the modal claim that **the number of knives would have been what it actually is even if Julius Caesar had not existed** is as follows:

> The numeral that denotes the number of knives would still have denoted it even if Julius Caesar had not existed.

More generally, in place of (QR) he may assert the following:

(QR*) If there are just as many *F*s in *w* as there are *G*s in *w**, then for some canonical numeral **n**, the denotation of **n** relative to *w* = the denotation of 'N*xFx*' relative to *w*, and the denotation of **n** relative to *w** = the denotation of 'N*xGx*' relative to *w**.

It is not clear to me that (QR*) does not capture all or most of the indisputable intuitive content of (QR). But the issue is not crucial: the accidental Platonist can do better.

The accidental Platonist thinks of the numerals as associated with roles. Just as different objects play the role of *the shortest spy* at different worlds, so different objects play the 4-role—the role defined by the open sentence '$\forall F\, y = $ N*xFx* iff $\exists_4 xFx$'—at different worlds. The original modal intuition was supposed to be that that the object that is N*xKx* in α is *identical* with the object that is N*xKx* in W. The accidental Platonist cannot say that. But he can say that the object that is N*xKx* in α *plays the same number-role as* the object that is N*xKx* in W. The relation that holds between *A* in *w* and *A** in *w** when *A* plays the same number-role in *w* as *A** plays in *w** is not identity on the present view. But it might be conceived as a sort of counterpart relation. Seemingly *de re* claims of transworld identity and distinctness involving numbers could then be understood in counterpart theoretic terms. Instead of saying 'The object that is N*xFx* in *w* = the object that is N*xGx* in *w**', we might as well say: 'The object that is N*xFx* in *w* is the **numerical counterpart in w** of the object that is N*xGx* in *w**.' If it is important to be able to make transworld comparisons of cardinality, then I can see no reason (apart from simplicity) why they must be formulated in terms of identity rather than in terms of sameness of role. We do *sometimes* speak in ways that suggest this sort of mechanism:

> The US president is the leader of the free world, and he would still have been the leader of the free world even if George W. Bush had never existed.

> The shortest spy is the most effective agent, and he would still have been the most effective agent if Ortcutt had not existed—and by the way, that Ortcutt is in fact the shortest spy.

In a similar vein the Accidental Platonist may say:

> The number of knives is 4, and it would still have been 4 even if Julius Caesar had not existed—and by the way, Julius Caesar is in fact the number of knives.

It should be evident that the Semiplatonist is in a position to understand transworld cardinality comparisons in a similar spirit. I conclude that to the extent that such comparisons are important, nothing crucial hangs on the (admittedly natural) reading in terms of strict identity presupposed in the Simple Argument. It should therefore be no embarrassment to the Semiplatonist if he cannot supply such a reading.

8

Turn now to Part 2 of the Simple Argument: the demonstration that the Fregean is in a position to ratify the common sense modal verdict and the corresponding semantic thesis of quasi-rigidity. Wright and Hale concede from the outset that strictly speaking this is not so. If 'N' is explained by means of the standard non-modal version of $N^=$, then it may follow that $N^=$ expresses a necessary truth. But this is compatible with Semiplatonism (suitably extended to the modal case) and with Accidental Platonism, which is to say that it does not by itself entail QR.

This fact is not in dispute. Wright and Hale comment as follows:

> It seems entirely intuitive and obvious that if there had been as many knives as there actually are forks, then *the number of knives there would then have been* is the same as *the actual number of forks*. If someone alleges that such a claim receives no mandate from our ordinary understanding of sameness of number, he is wrong. If they allege that there is such a mandate, but that it is independent of Hume's principle, the surely correct reply is that **Hume's principle has an intuitive content that exceeds that of its canonical second-order formulation**. (p. 359, my boldface)

Wright and Hale do not formulate the stronger principle that would do the work. But it's not hard to see what they have in mind. Let's consider a language L in which modal distinctions are represented by explicit quantification over worlds rather than by sentential modal operators. Instead of introducing a monadic function from concepts into objects, as with $N^=$, we may introduce a binary function from concept-world pairs into objects:

Modal $N^=$: $N(F, w) = N(G, w^*)$ iff there exists a one-one function *f: x is F in w* $\rightarrow$ *x is G in w**.

In a framework of this sort, concepts that are normally conceived as monadic—*x is a knife on the table*—are better conceived as dyadic—*x is a knife on the table in w*. When we ask after the number associated with an ordinary concept, we are really asking for the number associated with that concept relative to a world.

Modal $N^=$ guarantees a form of quasi-rigidity for the N-terms. The truth value of "$N(F,w) = N(G,w^*)$" will depend only on whether there are just as many *F*s in *w* as there are *G*s in *w**. So with modal $N^=$ in place as an implicit definition of "N", the contortions of the previous section are beside the point.

But now a question arises: Does modal $N^=$ solve the Caesar problem? Does it entail the determinate falsity of statements of the form "$N(F,w)$ = Julius Caesar"?

The answer turns on the status of the principle we called (EX). *If we agree that N(F,w) must exist in w, then yes, Modal $N^=$ entails the falsity of the Caesar sentences.* In a framework of the sort we are considering, claims of world-relative existence are most naturally represented not by the existential quantifier governed by the standard rules, but by a two-place existence predicate, $E(x, w)$. (If $\exists$ is governed by the usual rules, '$\exists x\ \phi x$' is then best read as expressing a claim of possible existence.) In the case relevant to the Simple Argument, we then have the following facts.

(1) E(Julius Caesar, α)

(2) ~ E(Julius Caesar, W)

(3) There is a one-one function *f*: $(Kx, \alpha) \rightarrow (Kx, W)$

(4) So by modal $N^=$, $N(Kx, \alpha) = N(Kx, W)$.

Now suppose for reductio that

(5) $N(Kx, \alpha)$ = Julius Caesar.

Then from (4) and (5) we have

(6) $N(Kx, W)$ = Julius Caesar

Given (2), we would then have (7)

(7) ~ E $(N(Kx, W), W)$

In words: The number of knives on the table in W does not itself exist in W. So given (EX), we have a sound reductio of the Caesar sentence.

There is no doubt that (EX) is intuitively plausible, but principles governing world-relative existence are esoteric, and intuitions about them should be taken with a grain of salt. But how bad would it really be if we were obliged reject it? Well, for the development of arithmetic there had better be a sense in which '$NxFx = NxGx$' entails '$NxFx$ exists'. But within the present (possibilist) framework we are in a position to supply such a sense without endorsing (EX). We have a distinction between '$E(N(F,w), w^*)$' and '$\exists x\ x = N(F,w)$' The former is true relative to w^* iff $N(F,w)$ exists *in* w^*. The latter is true relative to w^* iff $N(F,w)$ exists *in some world or other*—i.e., if the number *might* have existed. Within the present framework, there is no reason to resist the inference from numerical identities to existence claims of the latter sort. And on the face of it, this should be enough for the formal development of arithmetic. Existential quantification over numbers would then emerge as possibilist quantification: quantification over things there might have been.

If the Fregean now says, 'Ah, but that's not good enough. Ordinary mathematical understanding assures us that *a number exists in any world in which it is the number of Fs*', then I say that ordinary mathematical understanding says nothing of the sort. It is a perennial thought in the philosophy of mathematics that existential claims about the numbers are somehow unlike ordinary existential claims, and that they are somehow akin to claims of possible existence. This thought is at odds with Quine's dogmatic uniformitarianism with regard to the existential idioms. But that doesn't make it wrong. In any case, it has a natural home in the present context. And if something like it is acceptable, the pressure to accept (EX) is diminished. Interpreted as a claim about world-relative existence, (EX) is not all that obvious. Interpreted as a claim about existence in this more general sense, it is acceptable to the Accidental Platonist, but it does not serve the purpose of the Simple Argument.

It may help to say a bit more about how all of this bears on the cogency of Semiplatonism. Assume that the original language contains the resources for quantifying over worlds and for expressing world-relative concepts like *x is a knife on the table in w*. As before, assume that the original language has a single correct interpretation M. The symbol 'N' is then introduced by means of Modal $N^=$. This constrains the interpretation of the new symbol. It entails that 'N' must pick out a function that satisfies the following condition:

> $\phi(F, w) = \phi(G, w^*)$ iff there exists a one-one function f: *x is F in w* $\rightarrow$ *x is G in* w^*.

At this stage there is no requirement that the value of this function for (F,w) exist in w. Call any such function a **modal numerator**. Call a model of the new language admissible if it agrees with M on the old vocabulary and assigns a modal numerator to 'N'. Say that a sentence is true (false) if it is true (false) on all admissible interpretations; truth-valueless otherwise.

It is easily verified that the N-terms obey a version of quasi-rigidity on this account. For any admissible model, '$N(F, w)$' and '$N(G, w^*)$ denote the same object iff there are just as many *F*s in w as there are *G*s in w^*. It is also easily verified that (EX) and the Caesar sentences are uniformly indeterminate on this account. Despite all this the truths of pure arithmetic are true at every world in every admissible model, and so count as true simpliciter.

9

The upshot is that the Simple Argument requires both (QR) and (EX), and that both principles are separately rejectable despite their evident intuitive appeal. The move to a modal version of Hume's principle may enforce a version of (QR); but in the version I have considered, it cannot enforce (EX). I want to close by considering what would follow if both principles were accepted.

If (QR) and (EX) are granted, the Simple Argument is sound. We are in a position to demonstrate that Julius Caesar is not a number. The Caesar problem is thereby *solved*, and Semiplatonism stands refuted.

Is this enough to establish the characteristic Fregean claim that some version of $N^{=}$ suffices to introduce a pure sortal concept of Number? I don't think so. The Simple Argument is crucially limited in scope. It establishes that no contingent entity is the denotation of an N-term. So let's concede that point and consider **modified Semiplatonism**. This view regards the N-terms as massively ambiguous designators whose range of candidate designata is restricted to necessarily existing entities. A **kosher numerator** is a function from concepts to necessary objects that satisfies (modal) $N^{=}$. An admissible interpretation assigns a kosher numerator to 'N', and truth and falsity are defined by means of the familiar supervaluation. Sentences of the form '$NxFx$ = Julius Caesar' are now determinately false. But the N-terms are still massively ambiguous. Relative to one admissible interpretation, '$Nx\ x{\neq}x$' denotes God; relative to another it denotes the empty set; relative to a third it denotes the proposition that all men are created equal; relative to a fourth it denotes the key of E-flat minor; relative to others it denotes any one of the countless necessarily existing objects for which we do not and perhaps even cannot have a name. 'Mont Blanc' may divide its referent over a large finite number of mounds of rock. But the vast majority of things are determinately excluded as candidate referents. By contrast, numerical terms divide their reference over *nearly everything*.[7] The concrete contingent world is a drop in the bucket next to the unspeakable abundance of the realm of necessary beings. So a term that 'might as well' pick out any necessary object whatsoever is almost as bad as the maximally indiscriminate designators we have been discussing. Clearly, if this is how 'N' works, the concept expressed by 'x is a Number' is not a pure sortal concept. Numbers do not constitute a distinctive kind of thing. There is no sense in which equinumerosity constitutes a criterion of identity for a distinctive class of objects, and so on.

So far as I can see, 'To Bury Caesar' contains no consideration that would tell against this sort of view. Whether the arithmetical vocabulary is introduced by means of the original $N^{=}$ or by some more ambitious modal version

7. If Williamson and Linsky and Zalta are correct, we can drop the 'nearly'. (See Timothy Williamson, 'Existence and Contingency', *Proceedings of the Aristotelian Society*, Supp. Vol. 73 (1999), pp. 181–203; and B. Linsky and E. Zalta, 'In Defense of the Contingently Non-Concrete', *Philosophical Studies*, 84 (1996), pp. 283–294.)

of the same idea, this sort of modified Semiplatonism will always be available, and frankly I don't see what could possibly rule it out. I conclude that the Fregean Platonist has not described how stipulations of the sort he favours could possibly supply us with a pure sortal concept of Number.

10

The suggestion is that Fregean Platonism cannot in principle solve a certain generalised version of the Caesar problem, and so cannot be said to introduce a sortal concept of number. Suppose I'm right. Would that be the end of the world?

If it were simply a matter of ratifying a fragile consensus of modal and semantic intuition, surely not. But Wright and Hale have a reason for insisting upon a solution to the Caesar problem—a reason that goes well beyond the concern to vindicate common sense. In 'On the Harmless Impredicativity of $N^{=}$', Wright takes up a challenge derived from Dummett to explain how exactly the stipulation that $N^{=}$ is to hold could possibly constitute an adequate explanation of the singular term forming operator 'N'. Wright's protagonist, Hero, begins as our Semiplatonist begins. He understands a second order language in which claims of one-one correspondence can be formulated, but he lacks the resources for making claims about the numbers. When he is introduced to $N^{=}$ he is immediately in a position to understand claims of the form 'NxFx = NxGx' where 'F' and 'G' were part of his original vocabulary. But in order to get the construction of arithmetic off the ground, Hero must somehow come to understand claims of this form where 'F' and 'G' contain occurrences of 'N', for instance '$Nx\,(x = Ny\ y{\neq}y) = Nx\,(x = Ny\ y{\neq}y)$'. But how is he to make sense of this identity? He knows that it is equivalent to the following:

There is a one-one function f: $x = Ny\ y{\neq}y \rightarrow x = Ny\ y{\neq}y$.

But how is he to make sense of *this*? The open sentence '$x = Ny\ y{\neq}y$' is not part of his original language. Moreover the stipulation that $N^{=}$ is to hold gives him no way to translate it into a language he understands. So with what right does the Fregean maintain that the stipulation of $N^{=}$ puts Hero in a position to understand such embedded occurrences of 'N'?

In response, Wright maintains that

> Since it ought to be reckoned sufficient for an understanding of any predicate that someone should understand any statement that results from it by completing it with any term he understands . . . it should be sufficient for an understanding of ['$x = Ny\ y{\neq}y$'] that for each term t that he understands . . . Hero understands the corresponding ['t = $Ny\ y{\neq}$y'].

Now focus on the case in which 't' is part of Hero's original vocabulary.

> [Hero] will be able to know the truth-conditions—indeed the truth-value—of ['t = Ny y≠y'] provided, and so far as I can see, only provided he has a general solution to the Caesar problem.

Thus far we have taken it for granted that once $N^=$ is in place, the neophyte is in a position to operate with it as if it had the syntax it appears to have. But this is tenable only if the neophyte is in a position to *understand* open sentences of the form 'y = Nx $x≠x$'. The suggestion is that this condition is met only if the neophyte is in a position to solve the Caesar problem.

This is compelling, to a point. It would indeed be sufficient for understanding a predicate that one understand every completion of it by means of the singular terms one understands. Indeed a qualified version of the same condition would appear to be necessary. And yet it is tendentious in the present context to suppose, as Wright does, that understanding a sentence like 't = Ny y=y' consists in "knowledge of its truth conditions", where this in turn requires being in a position to assign a determinate truth value to it. Our Semiplatonists understand their Caesar sentences because they know *what it takes for such a sentence to be true in a model, what it takes for a model to be admissible, and how truth simpliciter is defined in terms of these materials*. Surely this explicit knowledge is sufficient for understanding. But if that's right then you can understand a Caesar sentence without being in a position to assign it a truth-value.

It is of course unclear what this sort of understanding might consist in for subjects who do not possess the Semiplatonist's explicit semantic knowledge. But consider our own understanding of the predicate 'x is an ordered pair'. We certainly do understand it. But when we are asked whether {{a}, {a,b}} is an ordered pair, we sense immediately that the question has no straight answer. On some 'construals' of the ordered pair notation, it is; on others, it isn't, and that's all there is to say. The thought is that the Semiplatonists understand 't = NxFx' in the same sense in which we understand '{{a}, {a, b}} = (a, b)'. And, surely, that is understanding enough. To understand an expression is to have a decent grasp of the constraints on its correct use *such as they are*. If for each term 't' that he understands, a Semiplatonist Hero is in a position to determine whether 't = NxFx' has a truth-value on some (all, or some particular) construal of the language, and if so, which facts about one-one correspondence are relevant to its truth-value, then he understands the corresponding open sentence well enough. If this is correct then Wright's story about the harmless impredicativity of $N^=$ does not require a solution to the Caesar problem. And if *that's* right, then I don't see why anyone should be disappointed if it turns out that the Fregean materials are insufficient for introducing a pure sortal concept of Number. And that's a good thing, in my view, since so far as I can see, even the modal strengthening of $N^=$ envisioned in 'To Bury Caesar' is manifestly insufficient for the purpose.

RESPONSES TO COMMENTATORS

BOB HALE
The University of Glasgow

CRISPIN WRIGHT
The University of St Andrews and New York University

We are most grateful to our three commentators for their thoughtful and challenging reactions. Limitations of space force us to attempt responses here only to what we take to be those of their considerations bearing in the most fundamental ways on the themes of *The Reason's Proper Study*.

Demopoulos

Bill Demopoulos rightly contrasts the neo-Fregean attempt to secure the a priority of arithmetic with Hempel's more traditional logicist attempt. The latter's claim was that "logical laws are analytic, not by fiat as on Frege's account, but because they are true in virtue of the meaning of the logical terms they contain" (p. ***) and that arithmetic follows from logic together with explicit definitions of arithmetical primitives, and so is likewise analytic. Following Fraser MacBride,[1] Demopoulos sees us as seeking to secure the a priority of our knowledge of arithmetic on the basis of Hume's Principle taken, not as analytic (in some more or less traditional sense) of some pre-existing concept of number but, as a stipulation—thereby avoiding the need to appeal to, and so to defend, any traditional conception of analyticity. In reply to the objection brought against Hempel by Benacerraf—that even if it is granted that "the theorems of logic in their primitive notations [are] themselves analytic", Hempel requires, but fails to provide, a further argument to justify the claim that "the sentences of arithmetic, in their pre-analytic senses, mean the same (or approximately the same) as their homonyms in the logistic system"—Demopoulos claims that the demand is unfair, or at least not clearly warranted:

> . . . independently of one's view of meaning or truth in virtue of meaning, it must be conceded that traditional logicism would have provided a viable answer to Kant if it had succeeded in showing that arithmetical knowledge requires only an extension of logic by explicit definitions. To see this, recall that Frege left the problem of securing the epistemic basis of the laws of logic largely untouched. Benacerraf's Hempel should be understood as proposing to fill this gap by suggesting that the laws of logic are true in virtue of

1. 'Finite Hume', *Philosophia Mathematica*, 8 (2000) pp. 150–159; and 'Can nothing matter?', *Analysis* (forthcoming).

the meanings of the logical constants they contain. From this it would seem to follow that if logical laws are true in virtue of meaning, so also is any proposition established solely on their basis, where 'established solely on their basis' is intended to encompass the use of explicit definitions. . . . Once Hempel is not represented as seeking to secure the truth of the basic laws of arithmetic by an appeal to the derivability of *mere analogues*, it is not at all clear that he owes us the argument Benacerraf claims he does. . . . The difficulties that attend this [i.e. Hempel's] program are not the methodological difficulties Benacerraf advances, but the simple failure to achieve the stated aim of showing arithmetic to be a definitional extension of logic. (p. 221f.)

But while Benacerraf's objection to Hempel is thus, in his view, misdirected,[2] Demopoulos contends that our approach—precisely because it does *not* claim to work with (explicit) definitions presented as capturing the essential meaning or content of *ordinary, pre-existing arithmetical notions*, but proceeds instead through a *stipulation*—*is* vulnerable to a form of Benacerraf's objection:

How, one might ask, does our knowledge of the truths that hold of the objects the neo-Fregean has singled out—the Frege-numbers—bear on our knowledge of the *numbers*, on the subject matter of *ordinary* arithmetic? (p. 224)

He considers that effectively just two responses lie open to us—the 'modelling' response and the '(preservation of) patterns of use' response, and argues that neither works.

The modelling response appeals to the fact that ordinary arithmetic can be modelled in *Frege arithmetic*—the result of adjoining Hume's Principle to suitable second order logic—and claims that in virtue of this, the truths of ordinary arithmetic share the same epistemological status as those of Frege arithmetic. Since none of us thinks this response effective, we shall not discuss it in detail—save to emphasise one point about it which will be important in the sequel. If the modelling response is to even look plausible, it has to be granted that our knowledge of the truths of Frege arithmetic is a priori—if they aren't, the truths of ordinary arithmetic could hardly inherit that status via the fact about modelling. Demopoulos's objection is exclusively directed against the claim that epistemological status is *transmitted* across the modelling relation. He concedes, at least for argument's sake, that our knowledge of the truths of Frege arithmetic, at least, is a priori—and anyway, he makes no objection to the claim that that is so.

Before we look at the detail of Demopoulos's objection(s) to the second, 'patterns of use' response, it is worth saying something about how the point just emphasised might bear on the way in which that response might be

2. Incidentally, it isn't clear to us that Benacerraf's objection isn't appropriately directed at Hempel—it's one thing to establish the analyticity of the traditional logicist's translations of the Peano Axioms, and another to establish that those translations are just that, i.e. that they do indeed capture the intuitive content of those axioms as pre-theoretically understood.

thought to work. So suppose it granted that we can stipulate Hume's Principle as an implicit definition and that this, together with Frege's Theorem, explains how we can know the truths of Frege arithmetic a priori. Someone now asks: 'Fine, but what does that tell us about *ordinary* arithmetic?' We then draw attention to the facts wrapped up in the claim that Frege arithmetic captures the 'patterns of use' exhibited in ordinary arithmetic talk. How does that help to answer our question? Well, why can't the thought be, quite simply, that in virtue of those facts, we may justifiably regard Frege arithmetic as not *merely* a *model* but as a *rational reconstruction* of ordinary arithmetic, so that our a priori knowledge of the truths of Frege arithmetic *is* knowledge of the truths of ordinary arithmetic thus reconstructed?

Simple as this account of how the 'patterns of use' response might work is, it is not clear to us that Demopoulos really engages with it. He writes:

> For the neo-Fregean, the reconstruction must not only capture an existing concept by recovering the patterns of use to which our arithmetical vocabulary conforms, it must also illuminate the epistemic status of our pure arithmetic knowledge. Having the status of a stipulation is not, of course, a characteristic of Hume's Principle that is recoverable from our use of our arithmetical vocabulary, but is something the reconstruction imposes on the principle in order to illuminate the basis for our knowledge of the propositions derivable from it. But it is unclear what is achieved if one has captured the pattern of use of an expression by a principle that—in the reconstruction of the knowledge claims in which that expression figures—is regarded as a stipulation. Does this confer the epistemological characteristics that the notion of a stipulation is supposed to enjoy on the knowledge claims that have been reconstructed? To establish that the epistemic basis for the knowledge these judgements express resides in the stipulative character the neo-Fregean analysis assigns to Hume's Principle, it is not enough to show that Frege arithmetic captures patterns of use. (p. 227)

This is puzzling—at least it puzzles us. For the obvious reply to the question raised in the fourth sentence just quoted is: 'Yes, obviously it does, *if* it is conceded that we really do have a *reconstruction* of the knowledge claims in question.' Demopoulos seems to grant that we have a reconstruction, but to complain that this is insufficient. It is "not enough to show that Frege arithmetic captures patterns of use"—his thought seems to be—to establish that ordinary arithmetic truths can be known a priori (by the route on offer), because even granted Frege arithmetic captures the relevant patterns of use, it is a further question whether ordinary arithmetic truths may enjoy the same epistemological status as those of Frege arithmetic. But this thought seems to miss, or at least to mislocate, the supposed bearing of the point about preservation of patterns of use. The bearing of that point is, to repeat, that it justifies the claim that Frege arithmetic *reconstructs* (and so doesn't *just* provide a *model* of) ordinary arithmetic. Once that claim is conceded, there is no room for a *further* question whether, given that we know truths of Frege arithmetic a priori, our knowledge of the ordinary arithmetic truths they reconstruct can be likewise a priori.

Demopoulos appears to go right past this, when he goes on to claim that the 'patterns of use' response is open to essentially the same objection as the modelling response:

> The essential point is not all that different from what we have already noted when discussing the response based on modelling, and can be seen by an example that is not all that different from the one cited in that connection. To see this, suppose the world were Newtonian. We could then give a reconstruction of our knowledge of the mechanical behaviour of bodies by laying down Newton's laws as stipulations governing our use of the concepts of force, mass and motion. But the fact that in our reconstruction the Newtonian laws have the status of stipulations would never be taken to show that they are in any interesting sense examples of a priori knowledge. Why then should the fact that the neo-Fregean represents Hume's Principle as a stipulation be taken to show that arithmetic is known a priori? The neo-Fregean reconstruction of the patterns of use of expressions of arithmetic leaves the epistemic status of the basic laws of arithmetic as unsettled as it was on the suggestion that Frege arithmetic merely *models* ordinary arithmetic. (p. 227)

We grant, of course, that mere stipulation of Newton's laws as implicitly definitive of the concepts of force, mass and motion cannot give us knowledge —a priori or otherwise—that the mechanical behaviour of actual bodies conforms to them. Thus if the invidious parallel Demopoulos suggests were good, we should conclude that stipulation of Hume's Principle cannot give us knowledge—a priori or otherwise—of arithmetic. But is the parallel good? It seems to us that Demopoulos has done much less than enough to make it stick, and that in fact it doesn't. Let 'Newton' denote the conjunction of Newton's laws as ordinarily understood, and 'NewStip' denote the (perhaps typographically indistinguishable) conjunction of the corresponding stipulations taken as introducing certain concepts of force, mass and motion. Then Demopoulos's claim is—or ought to be, if the parallel is to be damaging— that while we may, by laying down NewStip, acquire some a priori knowledge (in some sense, knowledge about (some things we are calling) force, mass and motion), we obviously do not thereby acquire a priori knowledge of Newton— as we ought to do, if we can, in just or essentially the same fashion, acquire a priori knowledge of truths of ordinary arithmetic by stipulating Hume's Principle, etc.

The obvious question is: what has become of the point about preservation of 'patterns of use'? This was, after all, precisely what was supposed to be playing a crucial role in the story about Frege arithmetic and ordinary arithmetic, and yet Demopoulos says nothing whatever about it, or anything analogous to it, in his Newtonian example—it has just dropped out of sight. Clearly the mere possibility of regarding (the sentences which formulate) Newton's laws as stipulations introducing concepts of force, mass and motion (as distinct from generalisations to which bodies conform) does not, and cannot, by itself justify the claim that NewStip 'captures a pattern of use' exhibited by 'ordinary' statements of Newtonian dynamics. It is, further, equally clear that such a claim could not be justified without appeal to the results of further, empirical, investigation of the motions of actual bodies of empirically ascertained masses, etc. It thus seems that Demopoulos's charge that the 'patterns of use' response falls to essentially the same objection as the modelling response relies on ignoring the crucial difference between them!

The crucial point is this. Suppose that NewStip is indeed something we are free to stipulate as a means of fixing certain concepts of force, mass and motion, and that we can thereby acquire some a priori knowledge concerning force, mass and motion *in the senses thereby established*. Nothing so far connects force, mass and motion in the senses fixed by NewStip to any properties of actual bodies ascertained or ascertainable by ordinary procedures involving observation and measurement. We have, so far, absolutely no reason to believe that actual physical bodies exemplify the functional relationships between force, mass and motion expressed in NewStip. This is just a consequence of the fact that we lack any reason to suppose that NewStip captures in significant respects a pattern of use exhibited by ordinary dynamical statements deploying concepts which—for all we know so far, by a potentially very misleading linguistic accident—we happen also to express by the words 'force', 'mass' and 'motion'.

In sum, Demopoulos's example crucially ignores the consideration that preservation of patterns of use requires preservation both of the uses of the target concepts in an appropriate general theory *and* of their uses in empirical application. The neo-Fregean's contention is that the stipulation of Hume's Principle, in the context of an appropriate logical education, will issue in principle in an arithmetical competence, both for pure and applied purposes, indistinguishable from ours. That is the force of the neo-Fregean claim that what is on offer is a *reconstruction* of our knowledge of arithmetic. The essence of our rejoinder to Demopoulos is thus first that a successful reconstruction in this sense leaves no room for any epistemologically significant residual distinction between the contents of the reconstructed and reconstructing contexts; and second, that no case for NewStip as a successful reconstruction of Newtonian mechanics has in any case been made out.

Rosen

Gideon Rosen's commentary focuses exclusively on 'To Bury Caesar', the long Chapter 14 of *The Reason's Proper Study* in which we evaluate and try to solve the notorious 'Julius Caesar Problem'—the problem posed by the apparent insufficiency of abstraction principles to resolve the truth-values of identity contexts in which only one of the configured terms is of the kind introduced by the principle in question while the other is a term whose sense qualifies it to refer to an object of an already understood sort.

One question is whether the apparent insufficiency matters. Rosen's view is that it does not: that a society is possible—his 'Semi-platonists'—which successfully operates with a neo-Fregean understanding of pure and applied arithmetic, based on Hume's Principle and second order logic, yet quite coherently regards the question whether Caesar is identical with the number four, for example, as indeterminate. Since this society operates successfully—since they need fall foul of or mishandle no issue in pure or applied arithmetic which we treat of satisfactorily—there is, in Rosen's view, no Caesar *problem* and the only question is whether we succeeded, in our chapter, in calling attention to aspects of the explanatory content of abstraction principles, and of Hume's Principle in particular, which do resolve Caesar-type issues in any case—so that the Semi-platonists make a *philosophical* mistake in regarding Caesar questions as indeterminate—even if a solution is not needed since innocence of that resolution is no practical or theoretical handicap.

Rosen speedily dismisses any claim of the principal line of thought in the chapter—that developed in its final three sections—to resolve Caesar issues as relying on the question-begging assumption that *natural number*, as explained by Hume's Principle, is a sortal concept. We suggest that Rosen importantly mistakes the dialectical situation in making this charge, and will say more about that below. However the effect is that he concentrates most of his fire on an argument—the so-called Simple Argument—which we developed earlier in the chapter. The Simple Argument, it merits emphasis, was explicitly targeted not at a general solution to the Caesar problem but as a response to a particular kind of nominalism: the suggestion of Michael Dummett that nominalism can take abstraction principles in stride, since they may always be interpreted in such a way that the novel terms refer back into the domain of their abstractive relation—so that Direction terms, for instance, introduced via the Direction Equivalence

$$\mathrm{D}a = \mathrm{D}b \text{ iff } a /\!/ b$$

may always be construed as referring to lines. (The corresponding claim about the numerical terms introduced via Hume's Principle would be that they may harmlessly be construed as referring back into a domain of concrete objects by which the concepts ranged over by the higher order variables on its right-hand side are instantiated.) Some of Rosen's remarks about the Simple Argument suggest that he has overlooked the limited significance we wanted to claim for it. But there is a specific objection which demands attention.

The nerve of the Simple Argument is the thought that, once the intended but—in the usual formulations—suppressed *modal generality* of abstraction principles is made explicit, it will not be consistent with their requirements to identify their proper abstracts with any contingently existing concrete particulars. Suppose for instance that the direction of line *a* is identified with some object—as it might be, Julius Caesar—that might not exist in circumstances where *a* still existed and remained unaltered in its orientational characteristics. A properly modal abstraction principle for directions should say that in those circumstances, *a*'s direction would be the same as it actually is—notwithstanding the fact that the object, Caesar, that was supposed to *be* its direction would not be around. So much the worse, therefore, for that supposition. If, on the other hand, *a*'s direction is identified with *a* itself, then the problem will be that *a* might not exist in circumstances where another line, *b*, which has the same direction as *a*, retained that direction.

More explicitly. Let parallelism relate lines both *within* and *across* possible worlds, and take 'the direction of' as an operator on <line, world> pairs, refashioning the Direction Equivalence as:

$$D\langle a,w\rangle = D\langle b,w^*\rangle \text{ iff } a,w \mathbin{//} b,w^*$$

where w, w* range over possible words, including the actual world, and *a* and *b* are any actual or possible lines. Intuitively the gist of the generalised principle is thus that the direction of *a* as it is/would be in w is the same as the direction of *b* as it is/would be in w* just in case *a* as it is/would be in w is parallel to *b* as it is/would be in w*. Using this fully modalised abstraction, we may then capture the gist of the Simple Argument as follows:

Let α be the actual world. Let *a* be any line and let q be any contingent existent distinct from *a*. Suppose for reductio that:

(i) $D\langle a, \alpha\rangle = q$

Assume (ii) $\langle a, \alpha\rangle // \langle a, w\rangle$ (that is, that *a* in w would be parallel to *a* as actually oriented).

Then (iii) $D\langle a, \alpha\rangle = D\langle a, w\rangle$, by the modalised direction equivalence.

Assume however that

(iv) q does not exist in w

Then (v) $D\langle a, \alpha\rangle$ does not exist in w, by (i) and (iv)

But this, apparently, contradicts (iii).

To defeat the supposition that $D\langle a, \alpha\rangle = a$, we then suppose a line *b*, distinct from *a*, such that *a* and *b* are parallel, and run the argument again substituting '*a*' for 'q' and '*b*' for '*a*'.

Rosen's rather more elaborate discussion focuses on numbers and Hume's Principle and moves though some skirmishing that we need not rehearse. But the nerve of the objection on which it comes to rest may be brought out nicely in connection with the above. It is, in brief, that there is no contradiction between lines (iii) and (v) unless we make an additional and, to Rosen's mind, unproved assumption: the assumption, which he dubs (EX), that any term of the form, $D\langle a, w\rangle$, if it denotes at all, will denote an entity that exists in w. Without this assumption, the truth of (iii) does not require $D\langle a, \alpha\rangle$ to exist in w—it is enough that it, and $D\langle a, w\rangle$, *actually* exist. And without any requirement that $D\langle a, \alpha\rangle$ exist in w, q's identity with $D\langle a, \alpha\rangle$ but non-existence in w is no problem.

The point is well made. But it was, in effect, anticipated and discussed in *The Reason's Proper Study* (pp. 361–3) and it is puzzling that Rosen does not refer to that discussion. In effect, and informally, leaving aside the play with worlds, the thought comes to this: the truth of the identity,

> the direction *a* actually has is the same as the direction it would have in circumstances w,

requires *a*'s actual direction to exist in the envisaged counterfactual circumstances w only on the assumption that to speak of the direction a line would have in certain circumstances must involve reference among the things that would exist in those circumstances: that the reference of what we might call a *subjunctive descriptive term*—like 'the man who would have been Provost if the College had voted five days sooner'—must be within the invoked subjunctive ontology, so to speak, and actual only in so far as that ontology overlaps with that of the real world. On this assumption, an identity like that above refers both to an actual object—on its left-hand side—and a subjunctive object—on its right-hand side and thus demands the existence of the actual object in the subjunctive scenario, just as the Simple Argument says. But what enforces that reading of subjunctive descriptive terms?

In 'To Bury Caesar', we confronted this objection in the setting of Michael Dummett's particular proposal that directions be taken as actual lines, specifically as representative members of equivalence classes of lines under parallelism. On this proposal, every direction is some line parallel to all lines of which it is the direction. We pointed out that the proposal gets into difficulty when it comes to construing the reference of the italicised descriptive term in an example like:

> If *a* were to exist in circumstances w but were to be orientationally altered in such a way that no actual line has the orientational characteristics *a* would then have, then *the direction of a as it would be in w* would not be that of any actual line.

If it be insisted that the italicised term refers, first and foremost, among actual lines, then there is no way of finding a referent for it consistent with Dummett's proposal. But if its reference is allowed to be within the subjunctive ontology—the ontology of circumstances w—then there can be no good objection to the corresponding construal of the subjunctive descriptive terms in the Simple Argument, which will therefore then succeed.

Rosen's objection is that we have not proved the assumption (EX). We agree: we said nothing to prove that assumption, and we grant that in general, subjunctive descriptive terms always allow of a *wide scope construal*: we may always read 'the F that would be G in circumstances w' as, roughly:

> among actual objects, that one which would be G in w.

But in writing as though there was something here that a proponent of the Simple Argument should *prove*, Rosen seems to have taken his eye off the dialectical ball. Rather, what his discussion brings out is merely that if a fully modalised abstraction principle is to be at the service of the Simple Argument, it must be received as part of its explanatory content that the subjunctive descriptive terms it governs are to be read *narrowly*; roughly as:

> among objects that would exist in w, that one which would then be G.

This too is in general a possible reading.[3] It is to be of the truth-conditions of identity contexts featuring terms *so understood* that the abstraction is to be received as a stipulative explanation. Our point was then that, when abstraction principles in general are so construed, the particular kind of nominalist response envisaged—that of welcoming them but insisting on construing the new terms as referring within the domain of actual contingent existents—will not be an option. To be sure, that leaves the nominalist free to try to accept a version of the principle with its new terms invariably given wide scope construal. We do not think the prospects in general for the move are good: it failed, for example, in the context of Dummett's specific proposal (and would be likely to fail for directions in any case, whatever the detail proposed, purely because the orientational characteristics of lines allow for continuous variation). But however that may be, ordinary subjunctive thought makes free use of terms read narrowly. It is therefore entirely natural that subjunctive thought of abstracts should be no exception and that their governing abstraction principles should be read accordingly. When they are, there is no option but to construe the abstracts in question as distinct from concrete contingents. That is not a problem for other kinds of nominalism—but it does preclude any which proposes hospitality for abstraction principles in their natural, fully modal form. Since Rosen lodges no objection to the legitimacy of abstraction principles so construed, he should allow that the would-be hospitable nominalist is in an unstable position, and that the Simple Argument does make the modest inroads into the Caesar problem which were advertised.

Only modest inroads though. Since Rosen does so with some flamboyance, there is no need for us here to stress again, what was anyway quite explicit in 'To Bury Caesar', that the general approach illustrated by the Simple Argument—that of seeking to distinguish abstracts from other objects by differences in their persistence-conditions across worlds—has and can have no grip when confronted by versions of the Caesar problem among necessary existents. That was the deficiency we sought to address in the argument of the last three sections of the chapter, in which the fully general solution is attempted which Rosen dismisses as question-begging.

How is it question begging? Rosen's interest is in the question: what if anything in Hale's and Wright's chapter can *force* his Semi-platonist out of his indeterminist slumbers, can *impose* a resolution of Caesar questions? His contention is that the sortal exclusion argument presented in *The Reason's Proper Study* sections 6 and 7 cannot possibly do so. The Semi-platonist allows that "there is a sense in which ['Number'] may be said to express a concept". But it is not, Rosen adds, "much of a concept" (p. 233). He will even allow that there is "a weak sense in which 'Number' expresses a sortal concept", since it functions grammatically as a count noun. But this is a weak sense because for the Semi-platonist, 'Number' has a determinate extension only relative to one or

3. Not always: consider 'If there were no numbers, then the number of numbers that there would be would be zero'. But it is rare for content, as opposed to context, to force a wide, or a narrow reading at the expense of the other.

another admissible interpretation, and such interpretations can vary wildly over what kinds of objects they assign to its extension. 'Given any object A, there will be no (non-relative) fact of the matter as to whether 'Number' applies to it. For the Semi-platonist 'Number' clearly fails to pick out a genuine *kind of thing*'. So even if sortal in a sense, the concept introduced by means of Hume's Principle is at best a 'role' or 'office' sortal—a concept under which objects fall simply in virtue of their playing a certain role, rather than in virtue of their intrinsic nature—so that Julius Caesar can play the role of a number, much as a brick or a sledge hammer can function as a doorstop, or a piece of string or dollar bill as a bookmark. Specifically, he will not grant that the concept so introduced is a *pure* sortal concept. Since—or so Rosen claims—the sortal exclusion argument "*assumes as a premise* that 'Number' picks out a pure sortal" it can have "no force against the Semi-platonist" (p. 235).

The question is whether this abrupt dismissal of the sortal exclusion argument is justified. If that argument really did assume it as premise that *number* is a pure sortal, it would indeed be useless—not just against the Semi-platonist, but for its intended purpose, which was precisely to rebut the charge that an explanation of *number* via Hume's Principle must fail to provide the resources to settle Caesar questions and cannot therefore be used to introduce *number* as a sortal concept. That is, if Rosen were right, the strategy explored in those sections of our essay would be fatally and fundamentally misconceived—irrespective of any detailed criticisms that might be made of our attempt to implement it, it would be bound to fail, because it would simply and quite blatantly beg the very question at issue.

Naturally, we do not accept that that is so. As suggested above, we think Rosen's claim that the sortal exclusion argument relies upon the premise that number is a (pure) sortal concept badly misrepresents the dialectical situation. What *is* certainly true is that the sortal exclusion argument *presupposes* that Hume's Principle is *put forward as explaining* or implicitly defining a (pure) sortal concept—rather than a 'role' or 'office' sortal, comparable with *doorstop*, *bookmark*, and the like. And this, we claim, is something which we are entitled to presuppose. For we are surely entitled, in offering an explanation of a concept, to make clear what kind of (sortal) concept we are seeking to explain, and no question is begged by our stipulating that it is a (pure) sortal concept that we are aiming to introduce. Whether the proffered explanation succeeds in introducing such a concept is, of course, a further question. The sortal exclusion argument takes the Caesar problem as presenting a challenge to the claim that it can do so—a challenge, that is, *not* to the claim that number *is* a pure sortal concept, but to the claim that an explanation by way of Hume's Principle succeeds in introducing it as such. The challenge consists in observing that the proffered explanation apparently fails to provide the resources to determine whether Caesar does or does not fall under the concept it purports to introduce. And the general form of the response to the challenge is: assuming that the proposed explanation does not fail *for some other reason* to introduce a sortal, the Caesar problem does not give a reason to think it must fail anyway—because contrary to appearances, the proposed explanation *does* provide the needed resources.

Rosen's rapid dismissal of the sortal exclusion argument thus rests upon a refusal to confront it on the terms on which it is presented. He simply fails to distinguish, firstly, between the questions whether *number* is a pure sortal concept and whether it can be adequately explained as such by means of Hume's Principle; and secondly—and in consequence—between the assumptions that *number* is a pure sortal concept and that the proffered explanation via Hume's Principle is put forward as explaining such a concept. The sortal exclusion argument makes the second assumption, but not the first, and then seeks to dispose of one putative reason for thinking that the answer to the second question must be negative. If it succeeds in that, it encourages an affirmative answer to the first question, precisely because the Caesar problem provides what may well appear the most compelling reason for a negative answer to the second—but it neither entails nor presupposes that number is indeed a (pure) sortal concept.

Rumfitt

Ian Rumfitt treats us to a painstaking scrutiny of the neo-Fregean case for taking numbers to be objects. Central to that case is what one of us first called the *syntactic priority thesis*:

> . . . the thesis that . . . no better general explanation of the notion of an object can be given than in terms of the notions of singular term and reference; and that the truth of appropriate sentential contexts containing what is, by syntactic criteria, a singular term is sufficient to take care, so to speak, of its reference.[4]

A considerable part of Rumfitt's extended essay is taken up with arguing that the syntactic priority thesis, along with the syntactic criteria in terms of which we have sought to interpret and apply it, is implausible, both as an interpretation of Frege and as a position considered on its own independent merits. Our disagreement with this assessment of the thesis—as Rumfitt interprets it—is far less extensive than his discussion might lead an innocent reader to suspect. For whilst the early formulation just quoted certainly lends colour to the uncompromising interpretation Rumfitt puts on the thesis, there is no doubt that significant qualifications are in order, as our own subsequent writings have sought to make clear.

One such qualification involves distinguishing between the questions: 'What is it for something to be an *object*?' and: 'What objects *are* there?'. It remains our view that the best general answer to the first is that objects are what (actual or possible) singular terms refer to. But since there is equally, at this level of generality, no better answer to the question: 'What is a *singular* term?' than: 'an expression whose function is to convey reference to a particular

4. Crispin Wright, *Frege's Conception of Numbers as Objects* (Aberdeen University Press, 1983), p. 24.

object', the relation between the general notions of object and singular term is not one of priority, but of *mutual dependence*. It is, rather, in answering the second question that a significant issue of *priority* arises—our view being that we do best to approach it by asking whether there exists an appropriate class of singular terms, functioning as such in true statements. It is in tackling this question, obviously—rather than in saying what it is to be an object—that we must have recourse to criteria of a broadly syntactic kind for discriminating singular terms from expressions of other logical types. Seeing matters in this way requires a distinction—to which Rumfitt, in our view, pays insufficient heed—between two quite separate questions about singular terms. One is: 'What is it for an expression to be—or better, function as—a singular term?', which we think should be answered as already indicated. The other—which, in contrast with the first, can only sensibly be posed in relation to a given language—is: 'Which expressions function as singular terms?'. It is here that criteria of the kind which we, following Dummett, have sought to develop come into play.[5]

Contrary to what Rumfitt suggests, we neither interpret Frege as thinking nor ourselves think that there is simply "no room for the possibility that there should be whole categories of expression (such as number-words) which 'mislead' us in exhibiting the surface inferential characteristics of singular terms without actually being such" (p. 200). The satisfaction of broadly inferential criteria of singular termhood by number-words figuring in true statements makes—as we have often ourselves stressed[6]—at best a presumptive, prima facie case for taking numbers to be among the objects the world contains. Had we supposed otherwise, we would have thought ourselves entitled—as we most certainly did not—to dismiss out of hand a whole battery of objections to the effect that, for one or another reason,[7] genuine reference to numbers and other abstracta is impossible. Since there is no real disagreement between us here on matters of substance—as opposed to (mis)interpretation of our position—we shall not pursue the matter further. But there is one significant point, concerning our preferred approach to criteria for singular terms, with which we should take issue before moving on to the objections developed in the latter sections of Rumfitt's commentary.

As Rumfitt appreciates, the idea underlying the criteria proposed in Chapters 1 and 2 of *The Reason's Proper Study* was to use modified Dummettian inferential tests in tandem with the Aristotelian test—the former were to discriminate singular terms from other expressions within the wider, but still

5. The distinction, while not explicitly stated, clearly underlies the opening remarks of *The Reason's Proper Study*, Essay 1. Its importance is stressed in Hale 'Frege's Platonism', *Philosophical Quarterly*, 34 (1984), pp. 225–41 and *Abstract Objects* (Blackwell, 1987), pp. 42–4.
6. See, for example, *Abstract Objects*, pp. 12–14, *The Reason's Proper Study*, pp. 7–11.
7. These include reasons for doubting that Hume's Principle can, after all, be regarded as providing—the crucial consideration on Frege's view of the matter—a satisfactory criterion of identity for numbers. Rumfitt grudgingly acknowledges that we recognise the importance of this condition, but mistakenly supposes this recognition to be in tension with our reliance upon broadly syntactic criteria for singular termhood. That there is no such tension should be clear from what we have already said here; for some further relevant remarks, see Essay 2, p. 69 and fn. 22.

restricted, class of substantival expressions, thereby excluding various kinds of quantifier-phrase; then, with quantifier-phrases safely out of the way, the Aristotelian test could be deployed to rule out expressions of other kinds—including, centrally, first-level predicates. Rumfitt points out that, contrary to what was there claimed, the Dummettian tests, even when modified as proposed, fail to accomplish even the limited purpose they were intended to serve. Given that, for the purposes of those tests, the validity of the relevant inference patterns must be understood in intuitive terms—of its being impossible that the premises should be true without the conclusion likewise being true—the Dummettian tests fail to exclude 'some even prime', for example, and more generally, fail to exclude quantifier-phrases of the form 'some F' when 'F' is, as a matter of necessity, uniquely instantiated. He goes on to observe that there was, in fact, no need to make the claim thus counter-exemplified. For since, when 'F' is necessarily uniquely instantiated, 'some F is G' will be strictly equivalent to 'every F is G', the fact that 'some F' and 'every F' are not debarred from figuring as the incorporating material in the application of the Aristotelian test does nothing to compromise its capacity to exclude first-level predicates. Consider, for illustration, the first-level predicate '. . . is prime'. For the test to disqualify it from singular termhood, there has to be an expression, *a*, of the same broad grammatical type such that where *b* is any expression suitable to form a sentence when combined with '. . . is prime' (and so also when combined with *a*), the resulting sentences—schematically, (*a*,*b*) and (is prime, *b*)—are contradictories. The obvious candidate for *a*, namely '. . . is not prime', would indeed be disqualified if just *any* quantifier-phrase of the form 'some F'—e.g. 'some number'—were allowed to play the role of *b*, since 'some number is prime' and 'some number is not prime' are plainly not contradictories. But allowing 'some even prime' to play that role does no harm, because 'some even prime is not prime' *is* equivalent to 'it is not the case that some even prime is prime'. Since such 'rogue' quantifier-phrases will in any case be themselves excluded from the class of singular terms by the Aristotelian test, this congenial observation might encourage the hope that all that is required is some modest adjustment of the original proposal.

Now, Rumfitt takes any such hope of achieving the desired outcome by "minor tinkering" to be dashed by the further observation that while 'some even prime' will be ruled out by its failure to pass the Aristotelian test, other quantifier-phrases of the same general form will be excluded by their failure to pass the quite different Dummettian test which requires that for '*t*' to qualify as a singular term, 'there is something such that A(it) and B(it)' must be validly inferable from the premises 'A(*t*)' and 'B(*t*)'. This he finds incredible. Surely, he protests, we are entitled to a single, *uniform* explanation why 'some even prime' and 'some odd prime'—and, quite generally, quantifier-phrases of the form 'some F'—do not qualify as singular terms (p. 205).

Well, the protest seems to us to have very little force, and certainly far less than Rumfitt contrives to suggest. Here once again is a point where it is of the greatest importance to bear in mind the distinction between, on the one hand, providing—relative to a given language—inferential tests for singular termhood in broadly syntactic terms, and on the other, giving a general account, in broadly semantic (and so language-neutral) terms, of what it is for an expression to function as a singular term. We may certainly agree that, *at some level*, there should be a uniform explanation why quantifier-phrases (of *any* given sort) do not qualify as singular terms. But why suppose that the explanation cannot be given in straightforwardly semantic or functional terms, and that it cannot simply be: because they do not serve to convey reference to particular objects? Why, in other words, should it be assumed that there has to be a uniform 'explanation' *at the level of language-specific* inferential or broadly syntactic *criteria*—a single test that excludes all quantifier-phrases of a given syntactic shape? Rumfitt does nothing to justify such an assumption, and we see no good reason to accept it. It is clearly an assumption of our enterprise that there is no reason why we should not use some inferential tests to exclude quantifier-phrases of the form 'some F' and other, different tests to exclude those of the form 'every F'. And this much, at least, Rumfitt seems to accept—he does not *here* protest: 'But these are all quantifier-phrases, so surely there must be some single test that excludes them all?' So what drives his insistence upon a *single* test which would exclude at least *all* phrases of the form 'some F', if it is not merely a mistake about the level at which uniform explanation is to be expected?

The main thrust of Rumfitt's criticism of the proposals of *The Reason's Proper Study*, however, is contained in section 5 of his paper, where—like Demopoulos—he raises concerns about the *stipulative* role assigned to Hume's Principle in the neo-Fregean reconstruction of arithmetic. Rumfitt's concern, however, is not, like Demopoulos, with the relation of the resulting theory, conceded (if only for the sake of argument) to be knowable a priori, to arithmetic as ordinarily understood, but with its availability to a priori knowledge in the first place.

His argument here may be represented as running via the following seven claims:

(1) When a semantically complex term possesses a sense, there is in general no guarantee that it possesses a reference.
(2) (So) the default logic for a language containing semantically complex terms should be a free logic.
(3) Since the authors of *The Reason's Proper Study* deny that the numerical operator is a logical constant, they should regard claim (1) as germane to the terms that operator enables us to form—so the presumptive logic for a language containing terms of the form 'Nx:Fx' should be free.
(4) In the context of a free logic, Hume's Principle must bifurcate into *Hume Minus:*

> For any concepts F and G, if there is such an object as the number of Fs, or the number of Gs, then the number of Fs is identical with the number of Gs iff there are just as many Fs as Gs.

and the Universal Countability Principle:

> For any concept F, there is such a thing as the number of Fs

(5) A legitimate explanation should do no more than explain the concept concerned.
(6) Hume's Principle offends in respect of claim (5).
(7) *Hume Minus* is an adequate and acceptable explanation of cardinal number.

If all seven claims are accepted, then the strongest legitimate stipulation in the offing is Hume Minus, and the underlying logic providing the context for the stipulation should be a free logic. But in that setting the proof of Frege's Theorem is blocked. So "When the consequences of admitting semantically complex terms into the relevant language are fully thought through . . . we seem to lack a principle which is weak enough to be simply laid down as true and yet strong enough to entail the basic laws of arithmetic" (p. 211).

It is a good question who has failed fully to think things through. To begin with, it is unclear whether Rumfitt's play with the spectre of free logic is of any bearing in this context. To be sure, claim (1) is in general ungainsayable. In particular it is obviously good for definite descriptive phrases defined over an independently specified range of objects—for instance, 'the first man to enter the building after 11.00 but before 12.00', where the date and building in question are understood from the context. Where, however, the expression in question consists of functor and a certain kind of argument term, there may very well be a guarantee of reference if the argument terms in question are guaranteed of reference for their part and the function in question is guaranteed to be total; for instance, 'the square root of k' where 'k' is canonical expression for a positive integer, and the range of the function is a understood to include the reals. It seems to us that there is accordingly no good sense in which a free logic is the *default* for a language containing semantically complex terms—and indeed, correspondingly, no good sense in which standard quantificational logic is the default for a language containing only semantically simple ones. After all, semantically simple terms may yet fail of reference—one has to make the assumption, or justify the claim, that they do not before standard logic is acceptable. And, on the other side of the coin, a family of semantically complex terms may have their reference guaranteed. The only sensible stance is that particular cases have to be taken on merit in assessing which type of logic may be appropriate. In particular, whether free logic should be the default for a language containing semantically complex terms is not a matter of whether or not *logic* guarantees a reference for such terms but entirely depends on whether, their senses being what they are, there is any possibility of reference failure. If there is none, there is no point in the strait-jacket of a free logic. We therefore reject claim (3). Rumfitt needs first to establish that there is a significant possibility that the compound numerical terms introduced by Hume's Principle may lack reference.

What is the resulting dialectical situation? Rumfitt is claiming, in effect, that a free logic should be the logic of choice for any theory that incorporates an abstraction principle, since, the terms it introduces being semantically complex, there can be no guarantee that they refer. We reply that mere semantic complexity is not enough for the point: in particular, that unless there is reason to think that, explained as via Hume's Principle, numerical terms may yet prove to have no reference, there is no good motive for a free logic in this context; and moreover that if Hume's Principle is allowed to stand as their explanation, we can easily *prove* the truth of statements—the identities on the left-hand side—whose truth ensures, to the contrary, that the terms in question do indeed refer. This might seem like a stand-off. It would be one if the proofs in question depended on features of standard logic that go missing in free logic. In fact, however, *they do not*—and the whole issue of free logic is therefore beside the point. It is beside the point because these proofs, and indeed the general proof of the infinity of the number series, make no presupposition that numerical terms refer. If one made that presupposition, one could simply reason without further ado that any identity of the form 'Nx:Fx = Nx:Fx' was true—just by the properties of identity, without appeal to Hume's Principle—and thus to the existence of a number for each (eligible) concept F.[8] Hume's Principle would be required only to distinguish the numbers so arrived at. But that is not the reasoning that the neo-Fregean offers. Rather it is recognised that proof is wanted of basic numerical identities, and such proofs are supplied by reasoning right-to-left across Hume's Principle, using appropriate statements of one-one correspondence among concepts as premises. The crucial issue is therefore not whether the underlying logic may be presumed standard or merely free—if Hume's Principle is available, the needed proofs will go through, even if it is free—but whether Hume's Principle is rightly considered to be available as a legitimate stipulative explanation of the concept of cardinal number.[9]

There is a residual objection here but it is independent of any issue about free logic.[10] It is that Hume's Principle is not available as such a legitimate explanation: that the real, legitimate work of characterising the concept of cardinal number is fully accomplished by Hume Minus. Hume Minus is indeed (prescinding from any other kind of concern) a principle that may be legitimately stipulated—but is, of course, insufficient for the neo-Fregean's purposes. By contrast the additional content carried by Hume's Principle itself, which may be crystallised into the Universal Countability Principle, is substantial content, nothing which may be stipulated, and—given that not every concept ought to be reckoned to determine a cardinal number in any case—problematical.

8. Rumfitt is of course also concerned about the issue of eligibility. We briefly comment below.
9. These remarks are, of course, based on the assumption that the relevant kind of free logic is one which restricts the introduction of identities—the availability of statements of the form 'a = a' without further assumption as premises in proofs. A different mode of constraint may of course be envisaged: identity-introduction may be left unrestricted—with identity-statements, say: 'Pegasus = Pegasus' allowed to be true even when their terms fail to refer—and controls placed instead on their existential generalisation. In that case, it is true, Hume's Principle may be impotent to generate proofs of numerical existence (it depends on the exact form of the controls imposed). But the neo-Fregean will reply that it is not of the truth-conditions of identity contexts of that etiolated kind that he is proposing Hume's Principle as a stipulative explanation, but of contexts whose being true precisely will license straightforward existential generalisation. So there is nothing for the critic to do but forget about free logic and take on its merits the issue whether that is indeed a legitimate form of explanation.
10. See also note 5 above.

If this is Rumfitt's real objection, we have to observe that it is nothing new—that it is the same objection that Field levelled in his review of *Frege's Conception*, and that we cannot respond to it here in greater detail than it already receives in the book Rumfitt is reviewing, most especially in the concluding part of Chapter 5, on implicit definition. Still, a little should be said.

The first thing to say is that we do of course endorse claim (5)—at least in all cases where the intent is that the statement of the explanation be something which it is to be possible to know a priori, just in virtue of its encoding a legitimate explanation. This consideration receives a great deal of attention in Chapter 5 of *The Reason's Proper Study* where the notion of an *arrogant* stipulative definition—one holding out a hostage to the satisfaction of some collateral, non-stipulatable condition—is to the forefront[11] and we present the case for saying that Hume's Principle is not properly viewed as arrogant. Rumfitt, so far as we can see, says nothing that addresses that case.

Some readers may feel that the merit in Rumfitt's complaint is obvious nevertheless. After all, the fact remains that Hume's Principle is deductively equivalent to the pairing of Hume Minus and the Universal Countability Principle. Yet it seems plain that the stipulation of the UCP *would* be arrogant—would amount, at least in the context of a normally generous ontology of concepts, to the naked stipulation of a huge array of so far unmotivated existence claims. But the stipulation of a conjunction must surely be arrogant if the stipulation of either conjunct would be. So how can the stipulation of Hume's Principle be otherwise? Don't we therefore have to agree that the only legitimate explanatory stipulation in the offing is that of Hume Minus?—that it is Hume Minus that fixes the general concept of cardinal number, and that since the residual content of Hume's Principle itself, which boils down to the UCP, is simply no matter for legitimate stipulation, no route of the kind sought by the neo-Fregean to recognition of the existence of numbers has been indicated or is in even dim prospect?

Well, we disagree. There are two crucial considerations to set against this line of thought. First, any impression that Hume Minus is adequate as an explanation of the concept of cardinal number turns on a deflated and unwarranted interpretation of what it is to grasp a sortal concept. Grasp of a sortal concept, Rumfitt and we agree, must involve grasp of a criterion of identity for its instances. But Rumfitt takes this to imply no more than a grasp of what should settle questions of identity and distinctness among instances of the concept *should any exist*. We take it, by contrast, that a satisfactory explanation of a criterion of identity for a concept must explain what should settle *the truth-values of statements of identity linking canonical terms for its instances*. In particular, a satisfactory explanation of cardinal number must provide a *complete* explanation of what settles the truth-values of identity statements linking canonical numerical terms, par excellence terms of the form, 'the number of Fs'. It might be contended that Hume Minus does indeed provide such an explanation: we learn from it that the identity 'Nx:Fx = Nx:Gx' will be true just in case both F and G have numbers and are one-one correspondent. But

11. See *The Reason's Proper Study*, pp. 126–34, 145–50.

the shortcomings of this claim are obvious: while we have been independently told what it takes for F and G to be one-one correspondent, we so far have no explanation whatever of how it might in principle be known that F and G do indeed have numbers. By contrast the whole point of the neo-Fregean proposal that Hume's Principle itself, undiluted, be received as an explanation of the concept of cardinal number is to address that very concern.

Still, it may be persisted, how can it be legitimate to try to address it in this way—when the point has not gone away that the content of the stipulation still includes the nakedly arrogant UCP? The answer—the second consideration—is that, the logical equivalence notwithstanding, that is *not* part of the content of the stipulation of Hume's Principle. The point is one we have made many times before, and which is emphasised in particular in the final section of Chapter 5 of *The Reason's Proper Study*, but it seems it will bear another exposure. It is, in brief, that the stipulation of Hume's Principle is a stipulation not of truth but of *truth-conditions*. The stipulation of the UCP is indeed just the naked stipulation that each term of the form 'Nx:Fx' refers (just provided F is in good standing as an expression for a concept). By contrast, Hume's Principle stipulates what it takes for a statement of numerical identity to be true and *thereby* fixes a sufficient condition for its terms to refer. It does *not* however stipulate that the sufficient condition in question is ever met. That matter is left to the collateral, and in our view, fully objective question whether the condition given on the right-hand side of an appropriate instance of Hume's Principle is satisfied. A key ingredient in the logicist aspect of neo-Fregeanism is that it is indeed satisfied, in basic cases as a matter of pure logic. But here logic is paying the role of *substantial input* into an essentially and properly conditional stipulation of truth-*conditions*, rather than that of a mere tool whereby to elicit consequence. Compare the roles respectively played by the observation that there are indeed straight lines, for which parallelism is a reflexive relation, and ordinary first-order logic in eliciting the consequences of the direction equivalence. No-one would confuse those roles. But then they should not be confused when it is the same body of knowledge which plays them both. The idea that the equivalence between Hume's Principle and the package consisting of Hume Minus and the UCP must lead us to conclude that they are wholly on a par as stipulations embodies just that confusion.

In sum: the comparison between the stipulation of Hume's Principle and that of the package consisting of Hume Minus and the UCP is doubly misconceived. It is misconceived in its attribution to Hume Minus of a satisfactory explanation of a criterion of identity for numbers. And it is misconceived in its imputation to the former of the arrogance that would attend the stipulation of the UCP itself. This distinction of course goes absolutely to the heart of the neo-Fregean epistemology of abstract objects, which has its origins in Frege's thought at *Grundlagen* §64 and the celebrated metaphor of 'recarving' of content. This approach may be vulnerable to serious objection in general, or open to specific difficulties when applied to the case of Hume's Principle. But so far as we can see, Rumfitt has not succeeded in addressing it at all.

One final point on this. There is of course a quite separate concern about the *generality* of Hume's Principle as standardly formulated. For a variety of reasons, we may not want every concept—every admissible instance of the higher-order variables in a suitable logic—to determine a number. The issues here are addressed in several places in *The Reason's Proper Study*. Some classes of awkward customers—non-sortal concepts of various kinds—may be excused by the fact that they are unsuited to serve as the terms of relations of one-one correspondence. But residual hard cases, including *self-identity* and various concepts—like *set*, *ordinal* and *cardinal* itself—whose determination of cardinal numbers is variously controversial, remain. Like Rumfitt, we are sympathetic to the suggestion that further restrictions are wanted on Hume's Principle to exclude these cases (though also open to persuasion that they are not: the issue is still open) But there is no important dispute between us and Rumfitt here. One proposal, canvassed in Chapter 13 of *The Reason's Proper Study*, is that cardinal number is a property of *definite* concepts—concepts that are not indefinitely extensible in Dummett's sense. Rumfitt has another proposal: roughly, as we understand it, that cardinal numbers belong to just those concepts that are equinumerous with the predecessors of an *initial ordinal*—an ordinal k in the series of ordinals under their standard ordering such that for each ordinal j < k, the concept 'less than or equal to j' injects but does not biject into 'less than or equal to k'. This may well give intuitively satisfactory results—though it is hardly at the service of a fully general logicist reconstruction of the theory of cardinal number unless the theorist may help himself to the theory of the ordinals first! But that, in effect, the UCP needs some such restriction is nothing we want to dispute. What we do dispute is whether Rumfitt has given any ground for supposing that there is indeed the prima facie "impasse" (p. 211) which motivates this proposal, along with the two others—which considerations of space, alas, prevent us from discussing here—considered in the final section of his paper. More, he has given absolutely no indication—to supplant the epistemology of abstracta we have here once again defended—of how he thinks we might reasonably take ourselves to know that concepts have numbers at all, even for the good cases that might pass muster under a properly restricted version of the UCP.