The Unity of the Declarative Sentence

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1

The problem of the unity of the sentence is to explain how a sentence manages to say something, to 'make a move in the language-game'. In the particular case of the declarative sentence, which is characterized essentially by its ability to say something *true or false* (cf. Aristotle *De Int*. ch. 4), the challenge is to explain how the sentence as a whole manages to attract this property, given that its components do not have it.[1] In his book *Language, Thought and Falsehood in Ancient Greek Thought*,[2] Nicholas Denyer implicitly commits himself to two conflicting accounts of what this unity consists in. The conflict is illuminating because it can be seen as giving expression to two attractive but apparently opposed thoughts on the nature of the sentence: the thought that all significant components of a sentence must have reference, or semantic role (a position which in the writings of Donald Davidson and Michael Dummett is truistic)[3], and the thought that the semantically significant components of the sentence cannot all be *names*, since then the sentence would lose its peculiar unity—its ability to *say* something (true or

[1] Cf. L. Linsky, 'The Unity of the Proposition', *Journal of the History of Philosophy* **30** (1992), 243–273, at 264; B. Russell, *Essays in Analysis*, D. Lackey (ed.) (London: Allen and Unwin, 1973), 28. The traditional name of the problem I am concerned with is 'The Unity of the Proposition', but since one nowadays needs the term 'proposition' for the referents of sentences, I have decided to modernise the name. I shall, for convenience, use 'sentence' as an abbreviation for 'declarative sentence' (restoring the epithet occasionally as a reminder that it is in force). Note that I shall speak indifferently of declarative sentences' saying something true or false and of their having assertoric force. Assertion is, of course, in the first instance a pragmatic rather than a semantic category, but the systematic use of particular conventionally recognized linguistic forms to make assertions has a semantic precipitate, so that we can say that *a sentence* says something, or is assertoric, if it is of the appropriate form for the effecting of assertions. It is this semantic feature of declarative sentences that I shall be concerned with in what follows.

[2] *Language, Thought and Falsehood in Ancient Greek Thought* (London: Routledge, 1991).

[3] See, e.g., D. Davidson, *Inquiries into Truth and Interpretation* (Oxford: Clarendon, 1984), 221–2; M. Dummett, *Frege: Philosophy of Language*, 2nd edn (London: Duckworth, 1981), 171–2.

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false)—and degenerate into a mere list. In this paper I shall try, using Denyer's text as my point of departure, to resolve the conflict by suggesting how a unified sentence can, after all, be composed of names.

The first account which Denyer gives of the unity of the sentence is not offered as such, but it emerges from his discussion of the differences between three primitive kinds of language, which he calls Agglomerative (**A**), Orthographic (**O**) and Sentential (**S**).[4] The three languages have in common that their basic 'sentences' all consist of linear strings of unambiguous names of primary elements, themselves arranged linearly. Thereafter they diverge in the following respects. In **A** and **O**, these basic 'sentences' are, according to Denyer, only by courtesy so called, for they are really complex names; but whereas in **A** the order in which the names are listed is insignificant, in **O** it is significant. Thus '*ab*', for example, would in **A** merely designate a complex object composed of the simple objects designated by '*a*' and '*b*', and is not semantically distinguishable from '*ba*'; in **O**, on the other hand, these two composite names additionally signify two different ways in which the complex consisting of *a* and *b* may be composed, for example, that *a* is to the left of *b*, and that *b* is to the left of a, respectively. In **S**, by contrast with both **A** and **O**, the basic 'sentences' are said to be genuine *sentences*: they are not merely lists of names, but are suitable for the making of assertions. In **S**, a symbol such as '*ab*' says that (for example) *a* is to the left of *b*.[5]

Denyer's characterization of the three languages naturally prompts the following observations. In the first place, although the basic sentences of **S** are not *merely* lists of names, they are *at least* lists of names. That does not, however, prevent them from being sentences; it does not prevent them from enjoying just that kind of unity which mere lists of names lack. What does their unity consist in? Denyer does not seek to answer this question directly. But some of his incidental remarks seem to me to point to a response. He notes that if we try to supplement **S** with words specifying how the designated objects are configured, we risk falling into a regress. If, for example, we try to say that *a* is to the left of *b* by writing '*a left b*', we now need to be told how to read this sign (what the significance of placing '*a*' to the left of '*left*' is, etc.). If we try to meet that need by writing in further signs, the same problem arises. At some point, how a composite sign is to be construed must rest on a con-

[4] Op. cit. note 2, 117–27.

[5] The parallels between **S** and the fully analysed language of Wittgenstein's *Tractatus*, noted by Denyer (op. cit. note 2, 125 n. 3), are obvious. I shall recur to this point below (§5).

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vention, governing the configuration of its components, which remains unspecified in the sign itself. As Denyer puts it: 'How [the] names [of **S**] are interwoven will of course be significant. But a manner of interweaving is not itself one of the ingredients'.[6]

Denyer does not mention the possibility of generating such a regress in the cases of **A** and **O**, but it is clear that it obtains there too. That is because the 'sentences' of these languages, and hence the referents of their component signs, are structured. Since the generation of **S**'s regress depended not on its special feature—that it contains genuine sentences—but merely on the fact that both its sentences and the referents of their component signs are structured, and since the regress arises just when one tries to specify the relation between structuring and structured elements (either of the sentence itself, or of the referents of its component signs), the fact that the 'sentences' of **A** and **O**, and the referents of their component signs, are also structured (however barely) is enough to ensure the availability of the regress in their case too.

Now Denyer tells us that the 'sentences' of **A** and **O** merely *name* complex objects: they do not *say anything about* those objects, or their elementary components. In **S**, by contrast, the leap to assertoric form has apparently been achieved: its sentences, though composed of names of elementary objects, are not to be conceived as names of complex objects; rather, they are capable of *saying something true or false* about the configurations of elementary objects. But this difference between **A** and **O** on the one hand, and **S** on the other, can only amount to this—that the complex names of **S** are *actually used* by its speakers to make assertions, whereas those of **A** and **O** are not. For there is nothing from the side of their languages preventing speakers of **A** and **O** from using the complex names of their respective languages to make assertions about the objects *a* and *b*: in **A**, '*ab*' could be used to say that *a* and *b* are somehow related; in **O**, the same symbol could tell us not merely that, but also how, *a* and *b* are related.

Any sentence must be structured: otherwise it could not be true or false. That structure seems in some way to be embodied in a regress which arises when we try to specify the relation between

[6] Op. cit. note 2, 125. Russell's 1913 manuscript *Theory of Knowledge* contains an unsuccessful attempt to make the 'manner of interweaving' one of the ingredients of a unified sentence (*Theory of Knowledge: the 1913 Manuscript*, E. Eames and K. Blackwell (eds) (London: Routledge, 1984), 105ff.): see S. Candlish, 'The Unity of the Proposition and Russell's Theories of Judgement', *Bertrand Russell and the Origins of Analytical Philosophy*, R. Monk and A. Palmer (eds) (Bristol: Thoemmes, 1996), 103–133, §4.

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configuring and configured components (either of the sign or of its referent). For the moment we can leave the nature of this embodiment vague; I shall return to it in §5. But an initial conclusion we can draw is this: as long as a candidate sentence is structured, and so has the capacity to generate such a 'configuration' regress, the fact, if it is one, that its component words are all *names* does not deprive it of the possibility of being used to make an assertion, and hence of enjoying the semantic property of sentencehood. None of the sentences of **S** (or of **A** or **O**, supposing their complex names are used to effect assertions) contains any special, non-nominal, linguistic component whose job it is to effect the unity of the sentence.

2

According to the second account of the unity of the sentence which Denyer offers, that unity resides in the special nature of the verb: it is essential to that nature, however we specify it in detail, that verbs are not names.[7] The position is redolent of the early Russell,[8] and of Plato's *Sophist* (262ff.). Denyer argues that verbs cannot be taken to be names because they fail what he calls the Replacement Test. Any genuine name '*a*' can, according to this test, be replaced by the circumlocution 'the object designated by the word "*a*"'. Applying this test to

(1) Socrates runs

yields the result that 'Socrates' is a name, for

(2) The object designated by the word 'Socrates' runs

is a perfectly intelligible, if stilted, English sentence, with recognizably the same purport as (1), whereas

(3) Socrates the object designated by the word 'runs'

is nonsense.

But we are now in a position to see what is wrong with this argument. Of course (3) is nonsense *as a sentence of English*, but that is merely because English adopts the arbitrary convention of employing a separate symbol for the copula (here figuring as the finite verb ending), rather than absorbing its function into the concatenation of the other sentential components, like **S**. (That English chooses to have a word, or inflection, which we may identify as the grammati-

[7] Op. cit. note 2, 164ff.

[8] *The Principles of Mathematics* (Cambridge University Press, 1903), 49f.

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cal copula, instead of allowing its function to be discharged by suitable concatenation, does not mean that concatenation plays no role in English: on the contrary, this grammatical component must itself be significantly concatenated with the other components of the sentence. The *grammatical* copula must be embedded within a *logical* copula: see §5 below.) Once a language chooses explicitly to symbolize some of the structural relations which are present in any significant statement, instead of allowing concatenation to take over the entire job, it is unsurprising that asymmetries among sentential components, reflected in the failure of some of these components to pass the Replacement Test, will be generated, given that those structural relations represent *universals*, so that the move from symbolizing just particulars to symbolizing universals as well introduces, in explicit form, the asymmetry of the particular/universal (participation) relation. The ontological asymmetry will then be reflected in the asymmetric subject/predicate structure of the enriched language. But the grammatical asymmetries so generated have no tendency to show that the items connected by the relation of participation are not objects. They must of course be different *types* (valencies) of object; otherwise the relation of participation would be symmetric. But that difference is adequately acknowledged in the traditional distinction between particulars and universals. Denyer, naturally enough, rejects the claim of universals to be objects.[9] But there is nothing in the way he draws out the asymmetry between name and verb which cannot be modelled in the distinction between particular and universal, so that there is no reason, on the basis of the name/verb asymmetry, to refuse to recognize universals as objects, so long as we are not disturbed by the consequence that our ontology will contain different types (valencies) of object.

One of Denyer's arguments depends on the point that verbs, unlike names, are true or false of objects;[10] but the essential distinction was, after all, expressed by Aristotle in ontological terms: a substance has no contrary (*Cat*. 3b24–5). (But many universals do.) Again, Denyer presses the related point that a verb, but not a name, can be significantly negated.[11] The name of a *universal* can, however, be significantly negated: here the point is that a universal has, whereas a particular does not have, a 'contradictory' (i.e., to any universal *F* there corresponds another universal *G* which is true of just those objects of which *F* is not true).[12] What our reflections in

[9] Op. cit., note 2, 165.
[10] Op. cit. note 2, 170–76.
[11] Op. cit. note 2, 176–79.
[12] Cf. Dummett, op. cit. note 3, 63–4. But note that Dummett here wrongly identifies 'contrary' with 'contradictory'.

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§1 showed was that there is no call, out of a misplaced fear for the unity of the sentence, to cavil at the suggestion that all of its component words are names; we can now add that there is equally no need to make a philosophy out of superficial grammar by refusing to accord verbs the status of names, and their referents therefore the status of objects.

3

It might be held that the difference beween particulars and universals is striking enough to warrant rejecting the claim of the latter to be objects, and of their corresponding linguistic expressions to be names. That is, in effect, Denyer's point.[13] With the Fregean hierarchy of levels in mind,[14] one might say that proper names such as 'Socrates', which are allocated to level 0 in the hierarchy, are *complete*, whereas predicates (such as 'runs'), which are allocated to level 1 or above, are *incomplete*.[15] The basic principle of the hierarchy is this: complete expressions are assigned to level 0; then, in general, an incomplete expression of level $n + 1$ is formed from a complete expression by omission of an expression of level n. (If we omit more than one expression, the resulting expression will be of a level one greater than the number of the highest-level expression omitted.) Frege recognized two kinds of complete expression: proper names and sentences. With the allocation of *sentences* to level 0 there can presumably be no quarrel, for however we construct the hierarchy, we must start from the completeness of sentences. But what is the motivation for locating *proper names* at level 0? Certainly, (first-level) predicates take a proper name to yield a complete sentence; but so far that looks like a symmetrical relation. Why not locate proper names like 'Socrates' at level 1, and the predicates like 'runs' which, on Frege's approach, count as first-level expressions at level 0 instead? On this alternative approach, such predicates would count as complete, proper names as incomplete. Is there any principled way of adjudicating between the Fregean hierarchy and the hierarchy which this alternative approach generates?

Casting around for a way of vindicating Frege, one might be impressed by a point which Ramsey raised, following Russell.[16] Ramsey suggested that among the senses of the predicate 'wise' we

[13] Cf. op. cit. note 2, 170.

[14] See here Dummett, op. cit. note 3, ch.3.

[15] Cf. P. Geach, 'Subject and Predicate', *Mind* **59** (1950), 461–482, at 462–3.

[16] *The Foundations of Mathematics* (London: Routledge and Kegan Paul, 1931), ch. 4.

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can discern a predicate of the form 'ξ is wise', to be completed in only one way, by filling its argument-place with a name, so forming an atomic sentence. A name such as 'Socrates', on the other hand, appears not to carry with it any specific number or type of argument-places. Ramsey expressed this point as follows: whereas 'Socrates' determines only one range of sentences—those in which it occurs in any way at all—'wise' determines two ranges—a wider range of sentences in which it occurs in any way at all, and a narrower range in which it occurs as the predicate 'ξ is wise' (i.e., in which it carries exactly one argument-place completion of which yields an atomic sentence). Now this distinction between wider and narrower ranges of the predicate 'wise' is of dubious significance: we can surely cater for all contexts in which the predicate 'wise' occurs by regimenting it as the predicate 'ξ is wise' (it being understood that 'ξ' may be replaced by a bound variable as well as by a name). Certainly Ramsey's example of an occurrence of 'wise' in its allegedly 'wider' sense—'Neither Socrates nor Plato is wise'—can be quite satisfactorily handled by adopting this strategy. But, leaving that point aside, and allowing Ramsey his distinction between the two ranges, the distinction still fails to motivate the Fregean asymmetry between names and predicates.[17]

Firstly, observe that while *differences* of level in the hierarchy (in either the Fregean or the alternative version) are absolute, there is no absolute sense in which expressions belong to just one level. In general, an expression located at level n of the hierarchy (in either version) may with equal right be allocated to level $n + 2$ (or $n - 2$, for $n \geq 2$): proper names, for example, which on the Fregean version of the hierarchy are classified as complete expressions at the zeroth level, may equally be regarded as second-level predicates, which are said to be incomplete.[18] Secondly, the absolute characterization of zeroth-level expressions as complete and expressions of all other levels as incomplete must itself, in the context of Ramsey's test, be subject to revision: for while expressions located at the zeroth level *eo ipso* count as complete, there is no absolute sense in which expressions located at any given higher level count by that test as incomplete. Take first-level predicates. According to Ramsey's test, first-level predicates are incomplete in relation, generally, to expressions of level 0; but according to the same test they count as complete in relation, generally, to expressions of level 2: for whereas second-level predicates each carry essentially a determinate number of argument-places for first-level predicates (so counting as incom-

[17] As Ramsey himself argued, though for a different reason from the one I shall offer: see note 21 below.

[18] Cf. Geach, art. cit. note 15, 474–5; Dummett op. cit. note 3, 62.

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plete, in respect of first-level predicates, by the Ramsey test), first-level predicates do not carry a determinate number of argument-places *for second-level predicates*, and so count as complete, in respect of second-level predicates, by the same test.[19] Hence, above level 0, the complete/incomplete distinction is not absolute—not even as fixed to a given level of the hierarchy—but is relative to one's *orientation* at a level. That then incidentally enables us to solve what is a problem for Frege's application of the complete/incomplete distinction:[20] how can one incomplete thing—a first-level predicate—put together with another incomplete thing—a second-level predicate—yield something complete? The answer is that, while first-level predicates count as incomplete in relation to level 0, in relation to level 2 they count as complete.

The first objection to adducing the point raised by Ramsey in defence of Frege was that, even within the terms of the Fregean version of the hierarchy, proper names are not unequivocally zeroth-level expressions; the second objection was that, still within those terms, expressions are not unequivocally complete or incomplete, even as fixed to a given level of the hierarchy (other than level 0), and hence in particular that first-level predicates are not unequivocally incomplete. Obviously we can put the two objections together. Since proper names can themselves be classified as second-level predicates, it follows that the complete/incomplete distinction does not apply in a unique direction to the relation between first-level predicates and proper names, but rather applies in opposite directions depending on whether one allocates proper names to level 0 or level 2 of the hierarchy. It follows from these considerations that the Fregean version of the hierarchy receives no special endorsement from the Ramsey test over an alternative version which reverses the relative positions of Frege's proper names and those predicates which Frege allocates to the first level.[21]

But nor do these considerations tell *against* the Fregean version of the hierarchy. They do not, after all, upset the fundamental position

[19] I am indebted to William Stirton for impressing this point on me.

[20] See here Linsky, art. cit. note 1, 265.

[21] Ramsey himself rejected the attempt to distinguish between 'Socrates' and 'wise' on the basis of an alleged failure of 'Socrates' to introduce a narrower range of predications comparable with the one introduced by 'wise'. The ground of his rejection was that 'it can be shown to be theoretically possible to make a similar narrower range for Socrates' (op. cit. note 16, 136). Indeed it can: the second-level predicate '(Φ) Socrates' determines just such a narrower range in respect of first-level predicates. (But the *point* of distinguishing between wider and narrower ranges in the case of either 'Socrates' or 'wise' remains unclear.)

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of the zeroth level—the (definitional) fact that expressions allocated to that level count as (absolutely) complete. And it might still be held that the preferential allocation of *proper names* to level 0 can be given an independent justification. On the strategy I have in mind, it need not be questioned that proper names *can* for certain purposes be allocated to upper levels of the hierarchy; but it would be held that their allocation, in the first instance, to level 0 is not thereby superseded, and hence that the Fregean way of determining the relative locations of proper names and predicates in the hierarchy—the claim that proper names are *basic* to the hierarchy—is fixed. This is Dummett's position. Dummett accepts that, for certain inferential purposes, we may need to allocate proper names to level 2. But he holds that this allocation is essentially secondary: it depends on a prior allocation of proper names to level 0. For

> we could not explain the conditions under which a sentence which resulted from putting a first-level predicate in the argument-place of [the] second-level predicate ['Φ (Socrates)'] was true or false unless we already understood those conditions via the construal of that same sentence ['Socrates is wise'] as resulting from putting the name 'Socrates', viewed as standing for an object, in the argument-place of the first-level predicate (op. cit. note 3, 66–7).

But what is the motivation for this claim?

Let us try to imagine a world-view which systematically reverses the order in the Fregean hierarchy of his zeroth- and first-level expressions, by systematically reallocating the expressions which he locates in the zeroth level to the second level, and then renumbering the levels accordingly. It is at least clear that under this transformation the valencies of different types of expression would not be affected. Proper names such as 'Socrates', now located at level 1, would still be constructed with predicates like 'wise', now located at level 0, and their corresponding referents would still engage with one another in the appropriate way, though now with reversed ontological allegiance: that is, the erstwhile basic particulars would constitute the new first level universals, and the erstwhile first level universals the new basic particulars. Would anything be lost by the transformation?

Dummett claims, in effect, that the alternative world-view would lack explanatory perspicuity, but he offers no argument for his claim, and it is difficult to think of one. Metaphysically speaking, the two purportedly different ways of looking at things would surely just be doublets of one another. In other words, there would be no absolute difference between the rival hierarchies: the only

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absolute would be the *difference of level* (i.e., the different valencies of expression or object) respected by both hierarchies. Dummett agrees that for the purposes of some inferences we need to appeal to an alternative analysis which locates proper names at the second level. But he denies that we otherwise need to make this appeal, so that if a language *as a matter of fact* does not contain the resources for engaging in higher-level predication (quantification), there can be no reason to appeal to the alternative analysis in dealing with its (necessarily atomic) sentences. This claim can only rest on the supposition that—quite apart from the question of modelling inference patterns—there is a significant metaphysical difference between 'two' world-views, 'old' and 'new', where the 'old' takes as first-level universals, and as basic particulars, what the 'new' regards, respectively, as basic particulars and as first-level universals. But what is the metaphysical difference between these world-views supposed to be? The one view says that the particular Socrates instantiates (among other universals) the universal wisdom, the other that the particular wisdom instantiates (among other universals) the universal Socrates. But the two views are surely just using different notations to calibrate the same facts.

The upshot of these considerations is that the Fregean version of the hierarchy is not actually distinct, other than notationally, from the alternative version. The different ways of annotating the same facts are systematic permutations of one another, so that the question which hierarchy is correct does not admit of a determinate answer. In default of any other way of motivating the Fregean way of modelling the asymmetry between proper names and predicates—the insistence that the former are basic to the hierarchy—we should conclude that there is no such asymmetry. No *such* asymmetry: there remains of course the absolute distinction of logical valencies which I have stressed. That is to say, there is a hierarchy of distinct levels, and, in particular, there is an absolute distinction between zeroth and first levels, whether we populate these levels in the way Frege envisaged, or in the alternative way. But the conclusion is that the fact that there is more than one level in the hierarchy—that there are distinct logical valencies—does not carry, as such, any implications concerning the relative ordering of different sorts of object or expression. There is no absolute sense in which Socrates is a particular and wisdom a universal rather than *vice versa*.

But the fact that the zeroth and first levels can be subjected to a permutation of their memberships (and the other levels correspondingly) does not, as I have emphasized, diminish the significance of the categorial distinction between particulars and univer-

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sals: this distinction survives any such permutation. Hence the question which ought to concern us is not so much whether the differences between the particular *Socrates* and the universal *wisdom* speak against construing the latter as an object—the availability of the permutation settles that question in the negative—but rather whether the inevitability (at least for natural language) of the *general* categorial distinction between particulars and universals, however these are constituted 'on the ground', warrants our refusing to count universals as objects. This larger question is naturally indifferent to the actual composition of the memberships of the levels in the hierarchy, and so is unaffected by the availability of a permutation of the Fregean hierarchy. In tackling this question, therefore, we can, for ease of exposition, confine our attention to the Fregean hierarchy, bearing in mind that the conclusions reached will also apply to any permutation of it.

Observe first that, when allocated to level 2 of the Fregean hierarchy, proper names are *true or false* of the Fregean first-level properties, and can take significant negation (cf. §2).[22] In view of this, we may reject as spurious a disanalogy which Dummett seeks to establish between the reference relation as it applies to proper names and as it applies to predicates (and relations).[23] The disanalogy is said to

[22] It is important not to misunderstand the claim that names, when located at level 2 of the hierarchy, can take significant negation. In his 'Names and Identity', *Mind and Language*, S. Guttenplan (ed.) (Oxford: Clarendon, 1975), 139–158, Geach shows that while there is no distinction between negating a sentence and negating its main predicate, there is a distinction between negating a sentence and negating its constituent names: conflating these latter two negation operations leads to inconsistency (144). What Geach's proof in effect shows is that the negation of any syntactic component of a sentence which may occur more than once (as the main predicate by definition may not) cannot be identified with a global negation operator attaching to the sentence as a whole. This is not a point I am required to deny. But the point does not, as Geach supposes, favour any particular analysis of any given sentence: it does not adjudicate against an analysis of the sentence 'Raleigh smokes' which treats 'smokes' as the subject and 'Raleigh' as the main predicate of this sentence. The point about negation is more correctly expressed not in terms of names but in terms of levels of the hierarchy (however they are populated): for any given analysis of a sentence, there is no distinction to be drawn between the global negation operator and the negation operator attaching to the expression of *highest* level (the 'main predicate') discerned within the sentence; negation of expressions of *lower* levels cannot consistently be identified with the global negation operator.

[23] Op. cit. note 3, 243–4.

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arise from different ways in which reference-failure purportedly affects proper names and predicates: Dummett claims that there is no analogy, for the case of predicates, to the fact that atomic sentences containing bearerless names are truth-valueless. The third-level description operator

$$I\,\Phi\ [\Xi x\,[\Phi x]]\,(y)$$

where 'Φ' holds place for a first-level predicate, and '$\Xi x\,[\Phi x]$' for a second-level predicate, cannot supply the needed analogy, because any given completion, such as

$$I\,\Phi\ [Mx\,[\Phi x]]\,(a)$$

where '$[Mx\,[\Phi x]$' is some particular second-level predicate, admits of a Russellian 'parsing out' of that operator, to yield an expression for which there can be no question of failure to take a truth-value, in this case

$$\forall\Phi\,[Mx\,[\Phi x] \rightarrow \Phi a].$$

But this argument fails. A bearerless name is only a *name* by courtesy, just as (Aristotle's example) a dead man is only a *man* by courtesy. It is really a nonsense-word[24]—which is itself only a *word* by courtesy. The analogy in the case of predicates will then not indeed be an empty predicate—not even, as Dummett rightly stresses, a self-contradictory predicate—for in neither of these cases is there any question of lack of sense, but rather a nonsense-item which is only by courtesy called a predicate. (In suitable sentences—say, 'Mumbo-Jumbo brings thunder', or 'They gimbled'—we can identify whether a component counts as a nonsense-*name*, or whether as a nonsense-*predicate*, by reference to the other—it is to be hoped, not nonsensical—components of the sentence.) And such nonsense-items will indeed infect sentences in which they figure with failure to take a truth-value.

In allocating proper names to level 2 of the Fregean hierarchy, we in effect envisage an assimilation of names to predicates. But the assimilation of predicates to names, and so of their referents to objects, can also be motivated within the terms of the Fregean hierarchy: for all expressions therein admit of significant quantification over their referents. The referents of first-level predicates, for example, can be quantified over by ascending to the third level. In general, the referent of an expression at level n counts as an object from the point of view of expressions at level $n + 2$. But what can be quantified over is objectual: no better conception of objecthood

[24] Cf. J. McDowell, 'On the Sense and Reference of a Proper Name', *Mind* **86** (1977), 159–185, §8.

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is available to us (see further §6 below). Hence we should dismiss as unavailable a certain intermediate position between the horns of the dilemma set out in the opening paragraph of §1—a position which holds that all semantically significant components of a sentence have indeed reference (that is a truism), but not all on the model of proper name and bearer. There is nothing more to the supposedly paradigm referentiality of the relation between proper name and bearer than the susceptibility of the relation to first-order quantification: so there is nothing in the paradigm which other components of the sentence, whose referents equally admit of being quantified over, may fall short of.[25]

We have, then, two possible assimilations, running in opposite directions. Neither cancels the other, for they have different motivations and serve different purposes. The point of deploying the first assimilation (names to predicates) is to emphasize that all items in the Fregean hierarchy may be housed at level 1 or above, and hence, given that an expression at level *n* counts as incomplete in relation to level $n - 1$, to stress that expressions which, from one point of view, are regarded as complete may, from another point of view, be regarded as incomplete. The point of deploying the second assimilation (predicates to names) is to remind us that all items in the hierarchy can be regarded as referring to objects, for they all admit of significant quantification in respect of their referents.

4

The extension of the constituency of names not merely to verbs but, more generally, to predicates, as envisaged in the second assimilation (§3), now needs to be situated within the context of a distinction which Dummett has made between what he calls 'simple'

[25] This familiar Quinean point (but jettisoning Quine's own hostility to second-order quantification) is admirably expressed by Dummett at op. cit. note 3, 218 (cf. 223). But the clarity of the formulation Dummett here achieves is unfortunately muddied by his repeated insistence (e.g., 199–203, 210, 223–6) that the doctrine of reference as it applies to proper names contains two ingredients: the conception of reference as semantic role, and the identification of the referent of a name with its bearer. But there is no warrant for the inclusion of the second ingredient, which purports, but fails, to supplement the former: for application of the conception of reference as semantic role to a name is precisely what will tell us what the bearer of that name *is*. There is no gap, as Dummett seems to suppose (528), between ascribing reference to expressions of a language in virtue of their semantic significance, and admitting quantification over the entities in the reference-class of such expressions.

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and 'complex' predicates.[26] The notion of a simple predicate is needed in specifying constraints on a correct theory of meaning for a language: the simple predicates of a language are those predicates which an empirically adequate theory of meaning would discern as the basic predicative units composing the well-formed sentences of the language. The notion of a complex predicate, on the other hand, is required for the exhibiting of correct inference patterns involving quantified sentences: complex predicates are just those which can be formed from a complete sentence by the omission of one or more occurrences of a singular term. As it stands, this characterization covers only complex first-level predicates. To capture the general notion of a complex predicate in which we are interested, we need to say that a complex predicate is formed from a sentence by the omission of an expression which counts, from the point of view of the remaining expression, taken to be of level *n*, as an expression of level $n - 1$.

With the distinction in place between simple and complex predicates, understood in this general way, consider the following gloss on the results of §3. What has in effect been shown, it might be said, is just that *simple* predicates can, without detriment to the unity of the sentence, be regarded as names; *complex* predicates, on the other hand, are a quite different matter. That is because complex predicates are so defined that they carry with them the copulative and concatenative structure, and hence the essential unity, of the complete sentence: in Fregean terms, they are unsaturated. As Dummett puts it, the slots for singular terms are *external* to simple predicates, but they are *internal* to complex predicates.[27] Of course, as we have just remarked, this characterization is insufficiently general. But the adaptation that was required to generalize it does not affect the present point, which is that the unsaturatedness of complex predicates surely unfits them to be names, at least in any

[26] Op. cit. note 3, 27–33 and *passim*.

[27] Op. cit. note 3, 32. There is a danger of confusion from the side of the terminology: complex predicates in Dummett's sense are 'complex' in a quite different sense from that in which complex names are 'complex'. A complex name is complex in the relatively untechnical sense that it is put together from more than one semantically simple component. But a complex predicate, in Dummett's sense, may be semantically simple: what marks it out as complex is that it contains its argument-places essentially. 'ξ runs' is a complex predicate; 'runs' is simple. For names the complex/simple division is exclusive and exhaustive; for predicates, the Dummettian complex/simple division is exclusive but not exhaustive. The inference from 'John admires Hegel' to 'Someone admires Hegel' requires us to discern the complex predicate 'ξ admires Hegel'; the predicate 'admires Hegel', on the other hand, is neither complex nor simple.

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straightforward sense. For to nominalize that unsaturatedness is surely to destroy its ability to *unify*—to reduce it to an element of a mere list. Should we accept this qualification to the conclusion of §3?

Nominalizing the complex predicate generates a regress with whose general character we are familiar from the tradition,[28] and which has already surfaced in the present discussion (§1). Take the schematic sentence '*Fa*', from which we can form two alternative complex predicates: '*F*(ξ)' and '(Φ)*a*'. What distinguishes these complex predicates from *mere* lists of names? The Fregean response to this question is that these predicates are to be so read that they *contain* within themselves the unity of the sentence, for they are after all formed from a complete sentence by the mere omission of a name, whose place is marked by a Greek letter of suitable type. That is, the complex predicate contains within itself not just names and placeholders for names, but also the copulative structure of the unified sentence. Within the above complex predicates we may discern a strictly conceptual component, '*F*' and '*a*' respectively, to which at least the argument of §2 presumably applies, and a strictly copulative component of both complex predicates, which we may represent schematically as 'ξ is Φ' in the case of the former complex predicate, and as 'Φ is ξ-instantiated' in the case of the latter. This copulative component, which we may call the copula in the logical sense, is to be distinguished from the copula in the grammatical sense, which consists of a word or words (or inflections of words). The logical copula includes, but is not confined to, the grammatical copula (if any).

How are we now to treat the logical copula? If it is a name, what is its referent? Here the problem immediately strikes us that when we try to specify a referent for the logical copula, we find that our attempt seems to fall short of its target. We might start with 'the relation of participation (instantiation)'. But that cannot be the end of the story, for then our base sentence will degenerate into a *mere* list: its components will designate an object (or second-level property) *a*, a first-level property *F*, and the relation of participation or instantiation; but we will have failed to capture the *connection* between these three elements. So we must add that the logical copula designates not merely the relation of participation (instantiation), but also the further participation of *a* and *F* in that relation (the further instantiation by *a* and *F* of that relation). But now all we have done is introduce a fourth object—the further relation of participation (instantiation)—which sits as much aloof from the

[28] F. Bradley, *Appearance and Reality* (Oxford: Clarendon, 1893), chs 2 and 3; Russell op. cit. note 8, chs 4, 9; G. Frege, *Translations from the Philosophical Writings of Gottlob Frege*, 3rd. edn, P. Geach and M. Black (eds) (Oxford: Clarendon, 1952), 54–5.

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other three components, to borrow Frege's phrase,[29] as did those components from each other before we adduced the further relation. To connect these (now four) components, we require a fifth relation of participation (instantiation). And so on. At each stage of this regress we arrive at a set of saturated entities, and so lose the essential unity of the sentence which the *unsaturated* complex predicate carries with it.[30] At each stage of the regress we try, but fail, to specify the total referent of that predicate (specifically of its copulative component). I shall return to the implications of this failure for the referential prospects of the copula in §6. Before that, I wish to examine the significance of the regress for the unity of the sentence (thereby redeeming the promise made at the end of §1).

5

The 'participation' regress not only gives concrete form to the metaphor of unsaturatedness; it also provides the metaphysical ground of the unity of the sentence. In the first instance, what distinguishes a sentence from a mere list is that a sentence has the capacity to say something *true or false*, whereas a list does not. What constitutes this distinction? The 'participation' regress arose in an attempt to specify the referent of the crucial unsaturated component which renders the base sentence a unity. But it can also be regarded as, at each stage, specifying a *necessary condition* of the application of that crucial component to the previous stage: for it is a necessary condition of *a*'s participating in *F* that *a* participate in participation with respect to *F*, and so on. Since the initial stage of the regress comprises a complete, unified sentence, constituted in the first instance by its capacity to be true (false), a capacity which it enjoys in virtue of its possession of an unsaturated component, it follows that the regressive specification of the necessary conditions of possession of that component is constitutive of the base sentence's unity. In that sense, the regress *itself* is ultimately constitutive of that unity.

It may be objected here that the generation of the participation regress cannot be *fully* constitutive of the unity of the sentence, because although it yields a specification of the *necessary* conditions of possession of that unity, it does not yield *sufficient* conditions. That is shown, according to this objection, by the fact that the attempt to specify the unity of a complex name[31] generates a paral-

[29] Op. cit. note 28, 55.
[30] Cf. Dummett op. cit., note 3, 251–2, 256.
[31] On the terminology see note 27 above.

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lel regress—the one which, as I mentioned in §1, arises when one tries to give a complete specification of the relation between structuring and structured elements of a complex. But complex names are, though unities in some sense, surely not unities in the sense in which sentences are unities, since they are not true or false. Now it is certainly true that this 'configuration' regress is in all relevant respects parallel to the 'participation' regress we have just been considering. The 'participation' regress is in fact a special case of the 'configuration' regress: it is the form the regress takes in the case of sentences constructed on the pattern of the asymmetrical subject/predicate relation. But the objector is wrong to suppose that complex names are not unifed in the relevant sense. (Here it is interesting to note that in his 1904 discussions of Meinong Russell regarded complex names as indeed unified in the same sense as sentences, and identified the regress we have been concerned with—which however he failed to recognize as containing the solution to the problem of that unity—as arising in both cases.)[32]

In the general case of a complex name there can be no doubt that the availability of the 'configuration' regress is what fully constitutes the unity of that name, in the sense of providing necessary *and* sufficient conditions for unity. For the presence of structure, at both linguistic and ontological levels, is exactly what that unity consists in, and the structure of the complex is just what the regress analyses: if there were no 'configuration' regress, or if it terminated at some point, the elements of the purported complex would fall apart; contrariwise, if analysis uncovers a 'configuration' regress in respect of any given elements, that shows that those elements are unified by the structural relation(s) whose reification generates the regress. What now has to be shown is that the unified sentence simply inherits this feature of complex names. But we have in effect already shown that, in observing (§1) that any complex name is *capable* of being used to effect assertions. Aristotle's example of a complex name lacking assertoric force is 'goat-stag' (*De Int*. 16a16–18): but 'goat-stag' *could* be used to mean (for example) 'a goat is a stag'. No extra linguistic element needs to be added to it to achieve this transformation: it already possesses enough structure to be capable of saying something true or false. Similarly, the 'sentences' of Denyer's primitive languages, though consisting of nothing more than strings of names, could, as observed, to be used to effect assertions, and in the case of **S** we are officially told that they are so used. A fully analysed Tractarian sentence also consists of strings of names, but for all that *says* that

[32] Op. cit. note 1, passim, especially 28, 50–7, 62. Cf. op. cit. note 8, 139. Cf. too L. Wittgenstein, *Geheime Tagebücher*, 3rd. edn, W. Baum (ed.) (Vienna: Turia and Kant, 1992), 18: '$aRb.aRc.bSc = aR[bSc]$ Def.'.

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things stand as they are shown by the sentence to stand.[33] But if the mere act of stringing (structuring) names together so as to form a complex name suffices, in these artificial cases, to yield linguistic items which bear a truth-value, it follows that, at least in these cases, the unity of the sentence *is* the unity of the complex of names composing it. Hence the conditions which are necessary and sufficient for the unity of the complex name are, in these cases, also necessary and sufficient for the unity of the sentence.[34] Those conditions are, as I have argued, given by the availability of the 'configuration' regress.

But now we cannot regard this result as restricted to artificial constructions like **S** or the fully analysed language of the *Tractatus*. We are looking for what constitutes the unity of sentences of natural language. If we successfully model that unity for an artificial language whose expressive resources are *poorer* than those of natural language, it is sound methodology to regard ourselves as having uncovered the mechanism of unity in the richer case too: for if the sentences of the poorer language are already unified, then, whatever is added in moving from the poorer to the richer language, it clearly does not affect (and in particular does not effect) sentential unity. But what constitutes the unity of the sentence in the poorer cases is just the fact that its constituent names are structured, that structure being in turn constituted by the regress which arises when we try to specify completely how the structuring component is itself structured. Hence that structure, and its concomitant regress, will be what unifies *any* declarative sentence. In the case of Denyer's **S**, or the fully analysed Tractarian language, the regress takes just the general form of what I have called a '*configuration*' regress, since in these cases there are no instrinsic distinctions among the names which compose the sentences of the languages. In the case of natural languages containing a grammatical copula, on the other hand, and which in consequence exhibit a subject/predicate asymmetry, the regress takes the more specific form of a '*participation*' regress.[35]

[33] Wittgenstein, *Tractatus Logico-Philosophicus* (London: Routledge and Kegan Paul, 1922), 4.22 and 4.022.

[34] Cf. Wittgenstein, *Notebooks*, 1914–1916, 2nd edn, G. E. M. Anscombe and G. H. von Wright (eds) (Oxford: Blackwell, 1979), 8: 'The proposition is a picture of a situation only in so far as it is logically articulated. (A simple—non-articulated—sign can be neither true nor false.)'.

[35] A corollary of the fact that the participation regress is merely one manifestation of a more general phenomenon is that the asymmetry of the participation relation has no role to play in grounding the unity of the sentence. A language can, like **S** or the fully analysed language of the *Tractatus*, be composed of names of the same type, so that in it no particular/universal distinction comes to expression (though the distinction would be *shown*, in the availability of repeatable combinations of distinct

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Of course there remains a distinction, in the case of languages which employ a grammatical copula, between declarative sentences on the one hand and complex names, such as definite descriptions, on the other. But the distinction, though grammatically significant, is logically superficial. There are two reasons for this. One is that, as we have seen, the incorporation of the *grammatical* copula in a sentence does not effect a unity missing from the sentences of languages lacking such a device: unity is effected by the *logical* copula, which includes the grammatical copula where this is present, but which also embraces the concatenative structure of sentences, and so is as much present to sentences of languages lacking a grammatical copula as it is to sentences of languages possessing one. But secondly, in the case of languages which have a grammatical copula, the surface difference between complex names and sentences, in virtue of which we say that mere complex names, unlike sentences, do not *say* anything true or false, has no logical depth. Definite descriptions, as Russell showed, entail, in context, an analysis which brings out a distinctively sentential content embedded in the nominal form of the description. Definite descriptions are nominal in form, but, in context, they are not merely nominal. The point is general to complex names.[36] Perhaps we do not want to agree with the early Wittgenstein and Russell that a complex name, in context, *says* that the referents of its constituent names are structured in such and such a way: but at the very least it *presupposes* that they are so structured, and that is enough to give the complex name a logically sentential status. We now have an explanation for what ought otherwise to appear to us as a highly surprising fact—that something superficially nominal in form can be deeply sentential, that by saying 'The redness of the rose is *F*' I can mean, or presuppose, that (among other things) the rose *is* red. The explanation is that what unifies a complex name is exactly what unifies a sentence: given that, the fact that a complex name is, in one clear sense, *really* a sentence should no longer be surprising.

[36] See here again Russell, op. cit. note 1, 50–7. Cf. A. Meinong, *Über Annahmen*, R. Haller (ed.) (Graz: Akademische Druck- und Verlagsanstalt, 1977), §10.

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signs), and that without prejudice to its ability to form declarative sentences. (Of course there are familiar and serious objections to regarding the fully analysed Tractarian language as an analysis of *our* language—which is why I rank it with **S** as one whose expressive resources are poorer than those of natural language—but that does not affect the present point.)

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6

I come to the question whether the logical copula—in general, the complex predicate—has reference, and, if so, what the nature of that reference is. According to David Wiggins,[37] the conclusion to be drawn from our inability to terminate the 'participation' regress is that the (logical) copula has *no* reference. But this response surely blurs a crucial distinction. We need to reserve the characterization 'without reference *simpliciter*' for sheer nonsense. Although the logical copula does not coincide with the grammatical copula, since it is discernible in sentences which lack a grammatical copula, it does incorporate the grammatical copula, where a language disposes of such. Now the grammatical copula is semantically significant; hence it is not open to us to say it has *no* reference, for that would be to class it with so-called nonsense words, i.e., with linguistic items which purport, but fail, to be words. Further, even in cases where a sentence lacks a grammatical copula, we need, as semantic theorists, to discern a logical copula in the principles of semantically significant concatenation drawn on by the sentence in question: for otherwise we should fail to distinguish between semantically significant and semantically insignificant concatenation. That is, we must be prepared to supply, in the metatheory of our semantic theory for the language in question, if not in the theory itself, a clause specifying the semantic significance of the (logical or grammatical) copula.[38]

If we cannot say that the copula lacks reference simpliciter, what sort of reference does it have? Elsewhere I have suggested that we might attribute a *deferred* reference to the copula.[39] The idea here is that, as we have seen (§4), to specify the referent of the copula we would have to give an infinite list, comprising the relation of instantiation, the instantiation of instantiation, the instantiation of instantiation of instantiation, and so on. On this approach to the semantics of the copula, having a deferred reference is to be regarded as occupying an intermediate status between having reference *simpliciter* and lacking reference *simpliciter*: we cannot say that the copula *lacks* reference *simpliciter*, for we can indeed begin to specify that reference; but nor can we say that the copula *has* reference *simpliciter*, for we cannot complete the specification. Why is it so damaging to the copula's prospects for being a *bona fide* referring device

[37] 'The Sense and Reference of Predicates: a Running Repair to Frege's Doctrine and a Plea for the Copula', *Frege: Tradition and Influence*, C. Wright (ed.) (Oxford: Blackwell, 1984), 126–143.

[38] See here my 'Bradley's Regress, the Copula and the Unity of the Proposition', *Philosophical Quarterly* **45** (1995), 161–80, at 174.

[39] Art. cit., note 38.

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that we cannot complete the specification of its referent? The thought here would be the following. Recall first that there is a constitutive connection between reference and quantification (§3). It follows that if the copula—in general, the complex predicate—has reference, it must be possible to quantify over its referent. But here it is plausible to suppose that, if we are to find a role for quantification over the referent of the complex predicate, we need first to separate that predicate into what in §4 I called a strictly copulative and a strictly conceptual component. For quantification is necessarily over entities figuring in inference patterns which are *surveyable*. But the unsaturated 'entity' purportedly designated by the logical copula is infinitistic, in the way explained, so that if we admitted variables ranging over the 'referents' of complex predicates as such, we would, according to the view I am outlining, lose surveyability of relevant inference patterns, in the sense that there would be no determinate answer to the question *what* was being quantified over. It can indeed be regarded as definitional of the saturated/unsaturated distinction that saturated entities are constitutively those over which we can quantify (hence they coincide with objects). The purported referent of the logical copula, on the other hand, not being finitely specifiable, since the attempt to do so leads into regress, cannot, according to this approach, be quantified over, and so is (uniquely) unsaturated. Each stage of the 'participation' ('instantiation') regress (§4) generates exclusively conceptual (and so saturated) entities, consisting of the original strictly conceptual component of the base sentence (*F*), as well as relations of participation (instantiation) of increasing complexity, figuring as the partially specified referent of the logical copula. At each stage, on this approach, the objects generated by the regress will sustain quantification; but the *final* designatum of the logical copula—the pseudo-object lying, unattainably, at the end of the regress—will not.

But can we not collect together all the items in the list generated by the regress and speak of them collectively as *the* referent of the copula? Why not regard the copula as having a determinate, albeit infinitistic, referent, rather than an indeterminate and deferred reference? After all, such an approach would not be obliged to break the constitutive connection between reference and quantification: what we quantify over, in quantifying over the referent of the copula, would be the totality of instantiation relations occurring in the list which I started to specify at the beginning of the previous paragraph. Nor need this view be impressed by the objection, already broached in §4 (where it was presented as a gloss on the results of §3), that in regarding the copula as having a determinate (albeit infinitistic) referent, we countenance the disintegration of the

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declarative sentence into a mere list of names. That objection simply fails to take seriously the special, infinitistic, nature of the referent of the copula which this approach invokes. Why should we not say that the unity of the declarative sentence consists in the fact that, while all of its semantically significant components have a referent, one component has, in the explained way, a different sort of referent from all the others?

The difference between the view that the copula has a determinate, but infinitistic, referent, and the view that it has an indeterminate and deferred reference is that on the former view, but not on the latter, it is possible to talk about the totality of instantiation relations contained in the list I began to specify two paragraphs back. Graham Priest argues that we can indeed talk about the referent of the copula, and certainly the claim that we cannot talk about some totality or other (which we appear to talk about in the very act of saying that we cannot do so) is apt to seem paradoxical.[40] Nevertheless, we should beware of succumbing too readily to the conclusion that whatever it is that renders the sentence a unity is within our referential grasp. At one point Priest asks us to consider self-referential sentences like 'I am thinking about the referent of the copula of this sentence'.[41] The truth of any such sentence depends on its copula's having a reference. But surely (Priest in effect claims) that sentence can be true, given that it is meaningful. We must be careful here. Let us grant that the above sentence is meaningful, and on the plausible (compositionality) principle that the meaning of a meaningful sentence is a function of the meanings of its semantic components, it will follow that the definite description 'the referent of this sentence' is also meaningful. But it obviously does not follow from that that the description is satisfied: the referent (i.e., meaning) of a definite description does not coincide with the object(s), if any, which satisfy it. It is open to us to hold that the above sentence is (meaningful but) false (and indeed necessarily false).

This unsuccessful argument aside, the question remains whether we should regard the copula as having a determinate, but infinitistic, referent, or an indeterminate and deferred reference. The former view is reminiscent of Leibniz's treatment of contingent truths as necessary truths whose analysis is infinitistic: for on the former approach unsaturatedness is treated as a special kind of

[40] *Beyond the Limits of Thought* (Cambridge University Press, 1995). Of course Priest's position is that we both can and cannot talk about the referent of the copula, and that this 'paradox at the limit of thought' is just one of many such paradoxes which we have to live with.

[41] Cf. op. cit. note 40, 212 with n. 22.

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saturatedness. The latter view lies in the tradition, associated with the names of Aristotle, Kant, Zermelo and Russell, which regards certain infinite totalities as illegitimate.[42] What these two approaches have in common—a rejection of the claim that the copula lacks reference *simpliciter*—is surely something we can accept without undertaking further inquiry into their relative merits. And indeed it is rather unclear that such a further inquiry would yield dividends. Of course there is a general issue of coherence between the related rival approaches to infinite totalities, to be settled by an examination of the metaphysics of the infinite. What is not clear is that examination of the semantics of the copula would have anything to contribute to this general issue. Rather, it seems obvious that our choice between 'Aristotelian' and 'Leibnizian' approaches to the semantics of the copula must be determined by the outcome of that wider inquiry. Pending that outcome, we can and should refuse to adjudicate between the two approaches. In fact it remains a clear possibility that the two approaches will turn out to be mere notational variants of one another; and in the remainder of this paper I want to explore an important respect in which the two approaches are indeed in harmony, lending some plausibility to this speculation. It is obvious that on a 'Leibnizian' treatment of the copula the sentence will be, quite straightforwardly, a list of names. But I think that even on the 'Aristotelian' approach there is a good sense in which that will be the right thing to say about the declarative sentence's semantic composition.

7

Frege regularly calls concepts 'unsaturated', but he hardly ever uses the word 'saturated': in most contexts where he wishes to contrast the status of objects with the essentially predicative nature of concepts, the term he applies to objects is 'complete' ('*abgeschlossen*').[43] I introduced the word in my above characterization of the 'Aristotelian' approach to quantification over the deferred referent of the copula in order to help mark the fact that the saturated/unsaturated distinction, as it figures in this context, is quite different from the complete/incomplete distinction employed earlier in the discussion of the hierarchy of levels. In general, an expression of level *n* counts as complete, by the Ramsey test, from the point of

[42] See here Priest, op. cit. note 40, parts 2 and 3.

[43] An exception, however, is to be found in his 'Notes for Ludwig Darmstaedter', where in an extensive passage 'saturated' is applied to objects as a synonym for 'complete' (*Nachgelassene Schriften*, H. Hermes, F. Kambartel and F. Kaulbach (eds) (Hamburg: Meiner, 1969), 274–5).

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view of level $n + 1$, and as saturated (or as having a saturated referent)[44] from the point of view of level $n + 2$, the level from where one quantifies over its referent; from the point of view of level $n - 1$, however, it counts as incomplete by the Ramsey test, and as unsaturated (i.e., as the bearer of the unity of the sentence). It is Frege's neglect of the *relativity* of the saturated/unsaturated distinction in particular which, according to the 'Aristotelian', gives rise in his philosophy to the disastrous paradox of the concept *horse*. (A 'Leibnizian', on the other hand, will resolve the paradox by permitting quantification over the unsaturated entities designated by concept-expressions: concepts will be just infinitistic objects of the specified sort.)

The complex predicate is so defined that it is the carrier of unsaturatedness in a sentence. Now on the 'Aristotelian'—as distinct from the 'Leibnizian'—approach it is not the referent of *that very complex predicate* which we are quantifying over when we replace its occurrence by a variable bound by a higher quantifier: for one cannot treat as saturated what is essentially unsaturated. But in the light of our discussion of the hierarchy of levels in §3 we can see that there is no problem here, just a requirement to handle the terminology with care. A given expression, as it figures in a sentence, may be treated as a complex predicate, and so as unsaturated, or as a name whose referent is available to be replaced by a variable bound by a quantifier, and so as saturated (as having a saturated referent). Both of these approaches are indeed available, according to the 'Aristotelian'—but not from the same standpoint. Of course, as I have stressed, given any unanalysed sentence it is quite arbitrary which of its expressions we assign to higher levels, and which to lower, provided we observe the principles of valency. (An example of failure to observe these principles would be the attempt to construct a sentence out of components of the same level.) But, once we have made a choice, the points of view from which any given expression counts as saturated and as unsaturated are distinct.

This relativization of unsaturatedness is underpinned by the fact that the logical copula—whose presence in the complex predicate is what renders it unsaturated—is, unlike the grammatical copula, not essentially a *word* (or inflection), but a *function* which is available to be *taken over* by words (or inflections), and is so taken over when those words (or inflections) are regarded as themselves supporting the frame of the sentence. That function is just to provide a sentence with what it needs to be enabled to say something, rather than

[44] As I deploy them, the complete/incomplete distinction applies exclusively to linguistic expressions, whereas the saturated/unsaturated distinction applies both to such expressions and to their referents.

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merely list its referents. Hence there can be no question of the *logical copula's* being seen to be unsaturated from one point of view and saturated from another: it is essentially (from all points of view) unsaturated. Only *words* (linguistic components) can be subject to the relativization in question here: from one point of view a word (linguistic component) may be seen to discharge the copulative function; from another point of view not. Words themselves, in advance of entering a particular sentence, have, in the relevant sense, no function: they are not in themselves either unsaturated or saturated. But on entering a sentence, they can assume either of these roles, depending on how they are viewed. There is nothing problematic in this relativization, so long as we do not mistake the kind of thing that is subject to it.

On the 'Aristotelian' approach to the semantics of the copula, whatever bears the unity of the sentence *can* be subject to quantification, but not *as* the bearer of unsaturatedness. The point of view (i.e., the level in the hierarchy) from which any candidate expression for the role of bearer of that unity is so subject—and hence treated as saturated—is not the same as the point of view from which it is regarded as being that bearer, and so at which it is treated as unsaturated. From the former point of view every component of the sentence is displayed as a name; from the latter point of view one component of the sentence—the bearer of its unsaturatedness—fails, strictly speaking, to be a name, the infinitistic nature of its failure being exactly what constitutes the unity of the sentence. The sentence, then, is indeed on this approach, as on the 'Leibnizian' approach, a kind of complex name. It is also capable of saying something. These two characterizations are rendered harmonious by the 'Aristotelian' by relativization to different standpoints—by being delivered from different levels in an indefinitely extensible hierarchy.[45]

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