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'TWO EXAMINERS MARKED SIX SCRIPTS.' INTERPRETATIONS OF NUMERICALLY QUANTIFIED SENTENCES

0. INTRODUCTION

Numerically quantified sentences such as

(1) Two examiners marked six scripts

admit of several different readings or interpretations. That much, at least, is uncontroversial. A speaker who uttered sentence (1) might be claiming that there were two examiners, each of whom marked six scripts – a claim which might be rendered true by a situation involving two examiners and twelve scripts. Or the speaker might be claiming that there were six scripts – maybe the borderline ones – each of which was double marked, that is, marked by two examiners. This claim might be rendered true by a situation involving twelve examiners and six scripts. Or again, the speaker might be making some claim along the lines that there were two examiners and there were six scripts, and that, as a result of the marking of the two examiners, the six scripts ended up marked. Whatever exactly this claim amounts to, it is clearly different from the other two claims. These three are by no means the only possible interpretations of sentence (1).

Whenever we find ourselves confronted with many possible interpretations of a single sentence, we have a choice of theoretical strategies; and these strategies divide the theoretical labour differently as between the semantic and the pragmatic components of a total account. One strategy is to postulate a *semantic ambiguity*. The several readings of a sentence are distinguished within a systematic semantic theory, and a hearer faces the pragmatic task of disambiguation in context. A second strategy is to postulate a single semantic analysis of the sentence, in such a way that this analysis determines the weakest of the possible interpretations. On this strategy, the other interpretations are treated as the products of *implicatures*, over and above the strict and literal interpretation. A third strategy follows the second in postulating a single semantic analysis, but differs in that it is not required that the semantic analysis determine any possible interpretation at all. Rather, the semantic analysis determines only the outline, or schema, for interpretations; it falls short of specifying any determinate truth conditions. In this sense,

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the semantic analysis is indeterminate, or *non-specific*, in respect of truth conditions. On this third strategy, each possible interpretation is the result of a pragmatic filling in of the semantically determined outline. Sentences containing indexicals or demonstrative pronouns are obvious cases for the third strategy. But to focus on the example of indexicals is potentially misleading; it is liable to obscure the fact that there is, in general, no simple rule for computing an interpretation from the semantic analysis plus facts about who is speaking, what the time is, or what words have recently been uttered.

Several years ago, Ruth Kempson and Annabel Cormack took numerically quantified sentences, like (1), as a case study for more general issues concerning ambiguity, implicature, and non-specificity ('Ambiguity and Quantification'). As they explain ('Quantification and Pragmatics', p. 608), their original hope was to use something like the second strategy; but this hope foundered on the fact that the interaction between the various interpretations and negation is more characteristic of disambiguations than of implicatures. Their question, at the beginning of 'Ambiguity and Quantification', is whether a sentence like (1) should be awarded 'a single semantic representation from which the particular interpretations are derived by applying general rules' (pp. 259–60). An affirmative answer might seem to indicate either the second or the third strategy. But although Kempson and Cormack do, indeed, return an affirmative answer, their proposal is unlike any that we have considered so far. It is, in a way, a hybrid, intermediate between the first and the second strategy. Sentence (1) is awarded a single initial semantic representation; but the general rules that yield the various interpretations are located within the semantic component, rather than the pragmatic component, of the total theory. Consequently, the interpretations themselves live at a level of semantic representation – but a level different from the level of the initial semantic interpretation.

Kempson and Cormack's proposals were greeted with detailed commentaries by Neil Tennant ('Formal Games and Forms for Games') and by Kent Bach ('Semantic Nonspecificity and Mixed Quantifiers'). In particular, Bach urges that a non-specificity account is preferable to the hybrid account offered by Kempson and Cormack. This suggestion is rejected by Kempson and Cormack in 'Quantification and Pragmatics', and one of their reasons for rejection is that 'the one clear-cut criterion of truth-conditional ambiguity of a sentence-string (as opposed to truth-conditional non-specification . . .) is the evidence provided by falsity, as represented in the linguistic system by negation' (p. 608). (See also Gillon, 'The Readings of Plural Noun Phrases in English'.)

However, in more recent work ('Ambiguity and the Semantics-Pragmatics Distinction') Kempson has painted a picture of semantics as massively underdetermining truth conditions, and of negation as operating at the level of truth conditions, which seems compatible in broad outline with the sort of approach that Bach favours.

> The conclusion that there is a lot more truth-conditional ambiguity than is contributed by the language in question itself is unavoidable. Another way of putting the same conclusion is that the truth-functional falsity operator is not restricted to ranging over aspects of sentence meaning: it also ranges over those aspects of propositional content determined by pragmatic principles. In consequence, the negation test of ambiguity does not provide a sufficient condition for some element to be part of the linguistic specification of meaning. ('Ambiguity and the Semantics-Pragmatics Distinction', p. 88)

Consequently, it is reasonable to suppose that a non-specificity account of the interpretations of numerically quantified sentences is at least a candidate, alongside a semantic ambiguity account, and Kempson and Cormack's own hybrid account. I shall not keep in play the idea of an implicature account, partly because of the difficulty of finding a plausible candidate for the strict and literal interpretation, and partly because the attraction of an implicature account as a pragmatic account is shared by a non-specificity account.

A choice between such candidates as remain is bound to be a matter, simultaneously, of high theory and details of the case. The separation between linguistic semantics and contextually determined truth conditions is clearly crucial; and to that extent, a non-specificity strategy is tempting, particularly where there is no syntactic motivation for postulating an ambiguity. But, on the other hand, there are many questions to be raised about a non-specificity strategy.

In the present case, a non-specificity account would need to be quite explicit about just how non-specific the semantic representation is to be. Does it provide a schema just for those interpretations which have an utterer of (1) making a claim about goings on that involve two examiners and six scripts (in which case the total account would have a non-specificity component and an ambiguity component)? Or does it, as Bach suggests (p. 597) provide a fully general schema, abstracting also from interpretations that have the speaker making a claim about up to twelve examiners, or up to twelve scripts? Does a non-specificity account explain why only certain interpretations are available? After all, not every conceivable claim about the marking relation, two examiners, and six scripts is a possible interpretation of (1). And does the fact that there is no independent syntactic reason for distinguishing the interpretations within linguistic theory really motivate a failure to distinguish the interpretations semantically? Must syntax and semantics go so closely in step?

These questions will not be addressed here. Rather, I shall be making some semantic proposals within the familiar framework of truth conditional semantics. If a non-specificity account is to be properly assessed, then it is crucial that we have available for comparison an alternative account in which the various interpretations are distinguished semantically. For present purposes, such an account can leave it open whether the semantic ambiguity is reflected at the level of syntax.

It will be implicit in what follows that the semantic ambiguity account which I propose is preferable to Kempson and Cormack's own hybrid account. But I shall not attempt to adjudicate between an ambiguity account and a non-specificity account. My task here is, rather, to contribute some of the raw materials needed for such an adjudication. Since the major question is, ultimately, over where to draw the boundary between semantics and pragmatics, what we have here is a rather oblique contribution to a particular case study for the refinement of the semantics-pragmatics distinction.

The overall plan of this paper is as follows. I begin, in Section 1, by summarising Kempson and Cormack's proposals, and raising three questions about them. In Section 2, I turn to methodological issues, and sketch some considerations that guide my own proposals. After a glance, in Section 3, at the *distributive* readings of sentences with just one numerical quantifier, I turn to the *group* or *collective* readings. Here my proposal relies on work by Barry Taylor, on what he calls *articulated predication*. His work is related to that of Adam Morton on *multigrade relations*, and Richard Grandy on *anadic logic*. A sketch of Taylor's system is provided in Section 4, and in Section 5 I consider the conceptual commitments of a representation using articulated predication. In Section 6, I return to sentences, like sentence (1), which contain two numerical quantifiers. Iterated deployment of the semantic resources used in Sections 3 and 4 provides, in principle, for eight readings. But some of the readings turn out to be equivalent; and the pattern of equivalences varies with different choices of binary predicate in place of 'marked'. In Section 7, I consider and reject the idea that the difference between distributive and group readings is constituted simply by a difference in the relative scope of an implicit quantifier over events. Attention is turned, in Section 8, to a comparison between the readings obtained so far and the four interpretations distinguished by Kempson and Cormack; and in Section 9, a branching quantifier representation is proposed for the so-called *complete group* interpretation. I end, in Section 10, with some reflections on the three questions raised at the outset.

1. Kempson and Cormack's proposals

Restricting attention initially to the 'at least' rather than the 'exactly' reading of the quantifiers, K & C distinguish four interpretations of sentence (1). These are formalised, using explicit quantification over sets, as follows. (I retain K & C's numbering.)

(IV) $(\exists X_2)(\forall x_{x\in X_2})(\exists S_6)(\forall s_{s\in S_6})Mxs$
(V) $(\exists S_6)(\forall s_{s\in S_6})(\exists X_2)(\forall x_{x\in X_2})Mxs$
(VI) $(\exists X_2)(\exists S_6)((\forall x_{x\in X_2})(\exists s_{s\in S_6})Mxs \;\&\; (\forall s_{s\in S_6})(\exists x_{x\in X_2})Mxs)$
(VII) $(\exists X_2)(\exists S_6)(\forall x_{x\in X_2})(\forall s_{s\in S_6})Mxs$

Of these, the first two are the scope differentiated *distributive* interpretations. The third is the *incomplete group* interpretation, and the fourth is the *complete group* interpretation. In K & C's scheme, these four forms are derived from a single *initial* form. This latter does not itself correspond to any natural interpretation of the sentence; rather, it is weaker than each of the interpretations. The general rules involved in the derivations of the intepretations from the initial form are these.

Generalising: Replace '$(\exists x_{x\in X_n})$' by '$(\forall x_{x\in X_n})$'
Uniformising: Replace '$(\forall x_{x\in X_n})(\exists Y)$' by '$(\exists Y)(\forall x_{x\in X_n})$'

These proposals prompt a number of questions. The first question is whether the general rules – Generalising and Uniformising – can belong to the semantic component of a total theory, given that they are formulated as syntactic rules. Something like this question might lie behind Tennant's complaint that K & C's procedures 'simply bash one well-formed formula of higher order logic into another' ('Formal Games and Forms for Games', pp. 317–18). The second question is whether the four forms (IV)–(VII) get the truth conditions of the various interpretations right; and, in particular, whether the idea that the incomplete group interpretation (VI) captures one of the readings depends upon an equivocation between 'marked' and 'helped mark'. The third question is whether it is really appropriate that all four interpretations should be represented as equally involving explicit quantification over sets.

These three questions are vague, to be sure. They express an unease concerning K & C's proposals – an unease which is doubtless the product of my own conception of what the semantic theorist's project is. I shall not attempt to refine the three vague questions into direct challenges, although I shall return to them in the final section. In the meantime, I shall briefly spell out some features of my own conception of semantic theorising, and then – guided by that conception – offer some alternative proposals.

2. Methodological considerations

Semantic theorising is obviously not an unconstrained activity. Idealising away from indexicality and other aspects of context dependence, we can say, crudely, that a semantic theory must specify, for each sentence of the language in question, what that sentence means. *Inter alia*, it must yield a specification of how the world has to be in order that a statement made using that sentence should be a true statement. So the most obvious constraint upon a semantic theory is simply that it should get the meanings of complete sentences right. That could be called a condition of *observational adequacy*.

The second kind of constraint that I would impose upon a semantic theory could, in at least one of its forms, be called a condition of *descriptive adequacy*. This constraint, roughly and intuitively, is that a semantic theory should correctly articulate the structure of the language. It is a kind of *compositionality* requirement. While the first constraint concerns only the output of a semantic theory, this second constraint relates to the inner workings of a theory.

In the present paper, I work within the framework of truth conditional semantics. A theory of truth conditions must, evidently, get the truth conditions of sentences right. But we can ask for more than that. We can ask that the way in which the truth conditions for each sentence are specified should faithfully represent the meaning or content of the sentence in question. This is a requirement of *interpretationality*, in the spirit of Davidson, and others such as McDowell and Wiggins who work within that same framework. (See Davidson, *Inquiries into Truth and Interpretation*; McDowell, 'Truth Conditions, Bivalence, and Verificationism', pp. 44–5; Wiggins, 'What Would be a Substantial Theory of Truth?', pp. 199–200.) If the language in question is a language in use, then part of what this requirement comes to is this. The way in which truth conditions are specified should faithfully represent the thoughts that speakers express.

To ask for this is to ask for more than mere correctness of truth conditions, because meaning cuts more finely than truth conditions. It is a familiar point that, given two *necessarily* equivalent specifications of truth conditions, one may represent the meaning of a sentence more faithfully than the other. This is particularly clear with truth condition specifications which are *a posteriori* necessarily equivalent. But we should also be ready to distinguish between *a priori* equivalent specifications of truth conditions.

The condition that Bruce is a man and the condition that Bruce is a *member* of the *set* of all men are *a priori* equivalent conditions. Someone who has mastery of the concepts involved, including the concepts of set and membership, can thereby recognise that neither condition could obtain without the other. But the conditions are still distinct. The first does, while the second does not, involve the concept of a set. For similar reasons, the condition that p is necessarily the case, and the condition that p is the case in every *possible world*, are distinct conditions.

It might be thought that there is no point in being too fussy about the occurrence of concepts from set theory in specifications of truth conditions, since a semantic theory will probably be cast in set theory anyway. But that would be to misconceive the relation between semantic theories and the mental life and conceptual sophistication of speakers. A semantic theory, employing set theoretic and other formal machinery, may be regarded as a theoretical description of a speaker's semantic competence. But, in order to regard a theory in that way, a theorist does not have to attribute to the speaker mastery of all the concepts employed in the theory. It is only at its final output of meaning specifications, or truth condition specifications, that a semantic theory has to connect with the common-or-garden mental life of the speaker.

Thus, working within the framework of truth conditional semantics, the first constraint that I would impose upon a semantic theory is this. In specifying truth conditions for a sentence, we should aim to represent as accurately as possible the content of that sentence; to represent, for example, what a sincere asserter would typically believe, and would intend his audience to believe.

It is not to be thought that the application of this constraint is invariably straightforward. On the contrary, it may sometimes be that the conceptual resources required to understand a sentence go beyond what might be expected given the superficial form of the sentence. Here is an example that is highly relevant to our present concerns, and to which I shall return in Section 9. Concerning sentences with branching *first order* quantification, Jon Barwise says:

> Branching quantification is a way of hiding quantification over various kinds of abstract objects (functions from individuals, sets of individuals, etc.) ('On Branching Quantifiers in English', p. 47)

But, despite complications of this kind, it should be clear that there are some specifications of truth conditions which are so overly sophisticated as not to be easily excused.

For example, systematic mastery of simple subject-predicate sentences, such as 'Bruce is a man', does not seem to require mastery of concepts from set theory. Consequently, we should aim – all else equal – to specify a truth condition for that sentence without reference to, or quantification over, sets. Likewise – I should say – we should aim to specify the truth conditions of the scope differentiated distributive readings of sentence (1), without making use of set theoretic concepts. This conviction is part of what lies behind my third vague question in Section 1. (My unwillingness to make freewheeling use of set theoretic notions distinguishes my proposals in the present paper from those of Scha, 'Distributive, Collective and Cumulative Quantification'.)

A semantic theory should articulate the systematic contribution that a word or phrase makes to the meanings – and to the truth conditions – of complete sentences in which it occurs. This is the basic and familiar idea behind the second kind of constraint to be imposed upon semantic theories.

This idea can be given a more *a prioristic* or a more empirical slant. On the one hand, we can conceive of the dependence of sentence meaning upon word meaning as something that an ideally rational speaker could recognise. This yields a constraint which is *a prioristic* in the sense that it can be applied even when the language in question is not a language in use, and has no actual speakers. It is a constraint which pushes a theorist to uncover maximal semantic structure in the language.

On the other hand, we can give the idea a more empirical slant, if we conceive of the structure to be articulated – the dependence of sentence meaning upon word meaning – as a cognitive psychological structure in a particular speaker. In that case, we are led to the requirement that the derivational structure in a semantic theory should mirror the causal-explanatory structure of the semantic competence of an actual speaker or group of speakers. This is one way of making empirical sense of the idea of a speaker *tacitly knowing* a semantic theory. (See my 'Tacit Knowledge and the Structure of Thought and Language', and 'Tacit Knowledge and Semantic Theory: Can a Five Per Cent Difference Matter?'.)

We have, then, both an *a prioristic* version and an empirical version of a compositionality requirement – or structural constraint – upon semantic theories. Perhaps the first version will appeal more to the semantic theorist who is primarily a philosopher/logician, and the second version to the theorist who is primarily a psychologist/linguist. Distinguishing between the two versions is, in my view, a good way of fending off certain ancient objections to the presumed empirical legitimacy of the notion of tacit knowledge. But, for our present purposes, we can smudge the difference between the two versions.

The application of a requirement of this kind can be a complicated, and indeed an indecisive, matter. This is particularly so in cases where a substantial gap is allowed to open between surface sentences and the corresponding sentences at the level of input to a systematic semantic theory. But, nevertheless, such a structural constraint does have some fairly straightforward consequences for our present case.

It seems uncontroversial that at least some readings of a sentence, such as sentence (1), with two numerical quantifiers could be mastered (by ideally rational speakers) and are in fact mastered (by actual speakers) on the basis of mastery of related sentences containing just one numerical quantifier. Consider, for example, such sentences as

(2) Two examiners marked my script
(3) Tom marked six scripts.

Thus, a minimal consequence of a structural constraint upon semantic theories would be this. Our semantic theorising about two-quantifier sentences should not be carried out in isolation from our theorising about one-quantifier sentences.

This may appear to be a ludicrously minimal consequence; and perhaps it is. But it already illustrates a difference from K & C's approach. The importance of this difference can be seen as follows. Sentence (2) has a non-distributive reading as does – even more clearly – this sentence.

(4) Three men pushed my VW up the hill.

I would expect to reveal at least some of the non-distributive readings of two-quantifier sentences like (1) as the product of iterated deployment of semantic resources already needed for the non-distributive readings of (2) and (4). But K & C focus directly upon two-quantifier sentences; and it is difficult to see, on the basis of their (VI) (the incomplete group interpretation), how they provide for anything other than the distributive readings of sentences like (2) and (4). But sentence (2) does have a distinct group or collective reading – a fact that may be obscured by concentration on the binary predicate 'marked' interpreted as merely equivalent to 'annotated'. So, somewhere in the total account, another resource will have to be employed to generate that collective reading. But then, iterated deployment of that resource would seem to threaten the K & C incomplete group interpretation (VI) with redundancy.

Reflections such as these are part of what lies behind my second vague question in Section 1.

These methodological considerations – and, in particular, the two kinds of constraints upon semantic theories – encourage me to seek to minimise the explicit use of set theory in the specification of truth conditions, and to focus first on sentences with just one numerical quantifier. To such sentences I now turn.

3. Distributive readings

There is not a great deal that needs to be said about the distributive reading of sentences with just one numerical quantifier! Consider, for example

(5) Two examiners attended the meeting

where we ignore the structure within the predicate 'attended the meeting'. The conventional wisdom about such a sentence is that it should be awarded as logical form some first order form like

(5a) $(\exists x)(\exists y)(Ex \;\&\; Ey \;\&\; x \neq y \;\&\; Ax \;\&\; Ay)$.

Since in general I would want to represent natural language quantifiers as binary (i.e. restricted) quantifiers, I shall need a slightly different semantic structure.

First, I take the binary 'at least one' quantifier as a primitive, and I write '$(1x)(\Phi x; \Psi x)$'. The sentence

(6) (At least) one examiner attended the meeting

is then represented as

(6a) $(1x)(Ex; Ax)$.

To say that much is, strictly speaking, not to make a semantic proposal at all, but only a syntactic proposal. But sometimes, as here, it is obvious how the theory of truth conditions will go. For suppose that we specify satisfaction conditions for 'E' and 'A' homophonically, and that we have an axiom for '$(1x)$':

(Ax1) $(\forall x)(s$ sats '$(1v_i)(\Phi v_i; \Psi v_i)$' iff
$(1x)(s(x/i)$ sats 'Φv_i'; $s(x/i)$ sats 'Ψv_i')).

(In the metalanguage (ML) I retain the familiar quantifiers alongside the binary quantifiers; $s(x/i)$ is the sequence which differs from s at most in having x in its ith place.) Then, in the presence of sequence theory, a

ML logic guaranteeing the extensionality of the positions occupied by 'E' and 'A', and the standard definition of truth in terms of satisfaction, we can obviously derive the homophonic biconditional

(Bi1) '$(1x)(Ex; Ax)$' is true iff $(1x)(Ex; Ax)$.

Second, we could have an axiom analogous to (Ax1) for each numerical quantifier '(nx)'. But to do so would be to treat each of the infinitely many such quantifiers as a semantic primitive. Since for familiar reasons, that is not desirable, it might seem natural to introduce the numerical quantifiers other than '$(1x)$' as definitional abbreviations.

However, that proposal is itself highly problematic. For, if the quantifier '(nx)' is merely a definitional abbreviation, then the position of 'n' is not open to quantification. In that case, quantification *over numbers* would have to be regarded as higher order – in the Fregean hierarchy, two levels higher that ordinary quantification. This would apply equally to numerical quantification over numbers, and that conflicts with the apparent univocality of numerical quantification over ordinary objects and over numbers.

The present paper is not the place to grapple with this familiar problem in the philosophy of mathematics. However the problem is resolved, the following schema

(Sch1) $(n+1x)(\Phi x; \Psi x)$ iff
$(1x)(\Phi x; \Psi x \,\&\, (ny)(\Phi y \,\&\, y \neq x; \Psi y))$

will be a truth, whether or not it is a definitional truth. And that is primarily what we need in order to make plain the contrast between distributive readings and group, or collective, readings.

One thing that is clear about the account so far is that it is both explicitly and implicitly first order.

4. Articulated predication

I have already mentioned that the sentence

(2) Two examiners marked my script

has a group or collective reading, clearly distinct from its distributive reading. On the group reading, what (2) says is that two examiners jointly did what one examiner could conceivably have done, namely, mark my script. In a little more detail, what seems to be required for the truth of (2) is that each of two examiners should have made a contribution to the marking of my script, and that, as a result of those contributions, the script should have ended up completely marked. Marking a script, unlike merely annotating a script, is a task that admits of completion.

A similar, but even clearer, case of a distinct group reading is provided by

(4) Three men pushed my VW up the hill.

Here again, we have the intuitive requirements of non-redundancy of the participants and completion of the task.

On the other hand, the sentence

(5) Two examiners attended the meeting

constitutes a much more marginal case for a distinct group reading, and a definitely problematic case is provided by

(7) Three girls kissed Nigel.

Kissing being what it is, it seems that three girls can non-redundantly contribute to Nigel ending up kissed only if they each kiss him, so that Nigel ends up kissed thrice. Consequently, if a group reading is imposed on (7) then its truth conditions will turn out to be the same as those of the distributive reading. This idea of interpretations collapsing into each other in virtue of facts about particular real relations will be important in what follows.

The main suggestion of this paper is that what we need, in order to capture group readings, is what Barry Taylor has called *articulated predication* ('Articulated Predication and Truth Theory'). To be a little more accurate: we need something rather simpler than Taylor's apparatus, for his articulated predication is sensitive to *order* in a way in which our group readings are not. I shall come to the simplification in the next section. First, I shall give a brief exposition of Taylor's work.

The idea of an articulated predicate is a generalisation of the traditional conception of a *Fregean* predicate – a predicate which takes some fixed finite number of terms to make a sentence. And it includes as a special case *multigrade* predicates – predicates that take any finite number of terms to make a sentence.

An articulated predicate, like a Fregean predicate, has a fixed finite *adicity* or *degree*; it has a fixed finite number of argument places. But each argument place can be filled by various finite numbers of terms. In fact, for each argument place a *place limitation* $[n; \alpha]$ is specified, where n is a natural number, α is an ordinal $\leqslant \omega$, and $n \leqslant \alpha$. A place whose place limitation is $[n; \alpha]$ can then be filled with any finite number of terms from n up to α.

The *type* of an m-adic articulated predicate is specified by an m-tuple of place limitations. For example, the predicate 'live together' – sometimes cited as an example of a multigrade predicate – is now seen to be of type $\langle [2; \omega] \rangle$. It is of degree one, and its one argument place can be filled by any finite number of terms from 2 upwards. The predicate 'marked' is of degree two: we register the difference between marker and markee. Consequently, its type is an ordered pair of place limitations. If articulated predicates are to be used in representing group readings of sentences like (2), then the first place limitation for 'marked' should be $[1; \omega]$. And it seems that the second place limitation should be $[1; \omega]$, too; for one, two, three, or more papers can jointly be on the receiving end of some marking. If Tom, Dick, and Harry jointly marked my script, then we can write

$$M(\text{Tom, Dick, Harry; my script})$$

where the semicolon separates the two argument places.

Here, it is important to avoid a misunderstanding about the articulated predicates that I propose to use. It is certainly possible to introduce articulated predicates definitionally, given Fregean predicates. Thus, for example, given a one-place Fregean predicate F, we can introduce F^* of type $\langle [1; \omega] \rangle$ such that

$$F^*(a) \text{ iff } F(a)$$
$$F^*(a, b) \text{ iff } F(a) \,\&\, F(b)$$

and so on. And, given a two-place Fregean predicate R, we can introduce by definition several quite interesting articulated predicates. For example, given R, we could define R^* so that *inter alia*

$$R^*(a, b; c, d) \text{ iff}$$
$$(R(a, c) \vee R(a, d)) \,\&\, (R(b, c) \vee R(b, d)).$$

so, it might be thought that what I am proposing is to make use of such definitionally introduced articulated predicates. The proposal would be, perhaps, to represent

(7) Three girls kissed Nigel

using an articulated predicate defined in terms of the Fregean 'kissed' in much the way that R^* was defined in terms of R. For, after all, I have already said that if three girls are jointly to kiss Nigel then they must each kiss him. In a similar vein, it might be thought that the proposal is to represent

(2) Two examiners marked my script

using some articulated predicate defined in terms of the Fregean 'partly-marked' or 'helped mark'. And quite generally, it might be thought that my strategy is, for each example, to hunt amongst the articulated predicates that can be defined in terms of Fregean predicates corresponding more or less closely to the main verb in the example ('marked', 'pushed', 'attended', 'kissed'), and to use one of these articulated predicates to provide a representation which at least gets the truth conditions right.

Any of this would be a serious misunderstanding of the proposal. I am not proposing a piecemeal approach to the logical form of these sentences; an approach relying on articulated predicates that can be defined in a more or less *ad hoc* way, in terms of Fregean predicates taken as primitive. The suggestion is, rather, to see articulated predicates as themselves primitive, and the more familiar Fregean predicates as the restriction of articulated predicates to the case in which only one term occupies each argument place. To relax this restriction, all that is needed is the intuitive notion of joint agency – and joint 'patiency'.

It would be a mistake, then, to ask how the articulated predicate 'marked' is defined in terms of the Fregean predicate 'marked' or 'partly-marked'. For it is not so defined at all. The articulated predicate is the very same predicate as has hitherto been regarded as Fregean.

Marking being what it is, if Tom, Dick, and Harry marked my script then, probably, Tom marked part of my script, as did Dick, and Harry, and every part of my script was marked by Tom, or by Dick, or by Harry. Pushing being different from marking, if Tom, Dick, and Harry (jointly) pushed my VW up the hill, then not only did Tom not push my VW up the hill, he did not push part of my VW up the hill either. The same goes for Dick and Harry. Kissing is different again. And whatever it takes for Doreen, Maureen, and Noreen (jointly) to kiss Nigel, it takes it in virtue of what kissing is. Such facts about real relations will not be reflected in semantic structure or logical form.

With the potential misunderstanding forestalled, let us return to the exposition of articulated predication.

Thus far, an argument place in an articulated predicate is to be filled by a string of ordinary terms: names and variables. But Taylor follows Morton ('Complex Individuals and Multigrade Relations') by introducing a special kind of 'multigrade', or *place-binding*, variable as primitive notation. Such a variable can stand alone in an argument place, and – on Taylor's account – is to be thought of model-theoretically as ranging over finite sequences of objects.

All the details of the model theory for a language with articulated predicates are provided in Taylor's paper. Here, I shall give the merest sketch.

The model theory retains the familiar idea of satisfaction of a formula by a sequence of objects in the domain. A sequence provides an assignment to variables. In the case of ordinary variables, a member of the sequence is assigned in the usual way. In the case of place-binding variables, a sub-sequence is assigned. To this end, the assignment of values to variables, determined by a sequence, is defined relative to a *boundary function* which, in effect, carves sub-sequences out of that part of the sequence not already used for assigning objects to the ordinary variables in the formula in question. The basis of the recursive definition of satisfaction appeals, in the usual way, to an interpretation of the atomic predicates. An interpretation assigns to each articulated predicate of degree n a set of n-tuples of *finite sequences of objects* in the domain, in such a way that the length of each sequence lies within the corresponding place limitation.

A first shot at a logical form for

(2) Two examiners marked my script

on its group or collective reading would be

(2a) $(\exists x)(\exists y)(Ex \,\&\, Ey \,\&\, x \neq y \,\&\, M(x, y;$ my script$))$.

Making the usual adjustment for binary quantification, and employing an arrowed variable $x^{\rightarrow}$ as a notational convenience, we can write down

(2b) $(2x)(Ex; M(x^{\rightarrow};$ my script$))$.

The arrowed variable '$x^{\rightarrow}$' is not, of course, one of Taylor's place-binding variables. If it were, then it would not be bound by the quantifier, for it would simply be a primitive variable distinct from 'x'. If we were to use a place-binding variable, say '$z_{\rightarrow}$', then we would have to make further adjustments and write

(2c) $(2z_{\rightarrow})(E^{*}z_{\rightarrow}; M(z_{\rightarrow};$ my script$))$

where 'E^{*}' is an articulated predicate of type $\langle[1; \omega]\rangle$ such that, for example

$$E^{*}(a, b, c) \text{ iff } E(a) \,\&\, E(b) \,\&\, E(c).$$

The main points that I want to make in the rest of the paper could be made in terms of regimentations like (2c) or like (2b).

Since I have opted for (2b) it is important to note that this is absolutely *not* to be taken as equivalent to

(2d)* $(1x)(Ex;\ M(x;\ \text{my script})\ \&\ (1y)(Ey\ \&\ y \neq x;\ M(y;\ \text{my script})))$.

What (2b) *is* equivalent to is

(2e) $(1x)(Ex;\ (1y)(Ey\ \&\ y \neq x;\ M(x,\ y;\ \text{my script})))$.

And, in general, in *contrast* to the schema (Sch1) we have

(Sch2) $(n+1x)(\Phi x;\ \Psi x^{\rightarrow})$ iff
$(1x)(\Phi x;\ (ny)(\Phi y\ \&\ y \neq x;\ \Psi x,\ y^{\rightarrow}))$.

Once one has seen the *model theory* that Taylor provides for a language with articulated predication, it is not so difficult to give a Tarski-style *truth theory* related to it just as a familiar truth theory for a standard first order language is related to first order model theory. As might be expected, the resulting truth theory is not homophonic in respect of the object language's articulated predicates. Where there is an articulated predicate in the object language, the metalanguage has a Fregean predicate of sequences, and where the OL has one of Taylor's *place-binding* variables, the ML has a variable ranging over sequences. Consequently, where the OL has the articulated predicate 'M' of type $\langle[1;\omega],[1;\omega]\rangle$, the ML has a binary Fregean predicate '$M\#$' such that, for example

(Eq#) $M(\text{Tom, Dick, Harry; my script})$ iff
$M\#(\langle\text{Tom, Dick, Harry}\rangle,\ \langle\text{my script}\rangle)$.

The same deviation between OL and ML will be present when we make the alterations needed to take into account that the OL has binary quantifiers, and will remain whatever exactly is our policy on the 'arrowed' variables – whether, for example, we opt for the (2b) or the (2c) style of regimentation.

In the light of the first kind of constraint on semantic theories that was discussed in Section 2, this feature of the truth theory just gestured at must raise a worry. That constraint was along the lines that we should aim to represent as accurately as possible the content of sentences. Yet it really does not seem very plausible that in order to master the group or collective reading of a sentence like (2) or (4) a speaker needs to employ the concept of a sequence, and predication of sequences.

5. Conceptual commitments

Does the proposal that the group or collective reading of (2) or (4) should be represented using articulated predication have the consequence that mastery of the group reading involves employment of the concept of a sequence? If so, then doubt is cast on that proposal, for the consequence would be implausible.

For two reasons, I do not think that the proposal does have that consequence. First, articulated predication as set out by Taylor enables us to mark distinctions which are not needed in the formalisation of group or collective readings. In particular, in, for example,

(8) Tom, Dick, and Harry marked by script,

the order of the three names is irrelevant. Of course, there are other possible applications of the machinery of articulated predication in which the order of the terms filling the argument places is crucial as, for example,

(9) Tom, Dick and Harry kissed Maureen, Doreen and Noreen respectively.

But if we are interested in borrowing Taylor's idea *only* as a way of representing group or collective readings then we can, in the model theory, treat all sequences with the same members as equivalent. If we factor out by such an equivalence, then we can replace sequences by (unordered) sets; and if we then construct a truth theory we shall state the truth conditions of OL sentences containing articulated predicates by using Fregean predicates of *sets* rather than sequences in the ML. So, if conceptual commitments are to be read off from the ML's statements of truth conditions, then the proposal has, at most, the consequence that mastery of the group reading involves employment of the concept of a *set*.

But further, somewhat impressionistic, considerations suggest that even the concept of a set – a kind of abstract object – is not required. In stating non-homophonic truth conditions using predication of sets we never need to consider sets of sets, but only sets of individuals. What is all of a piece with that, we have no use for the set theoretic distinction between $\{\{a, b\}, \{c, d\}\}$ and $\{a, b, c, d\}$, or between $\{a, \{a, b\}\}$ and $\{a, b\}$. Further, we have no real use for the set theoretic distinction between a singleton set and the individual which is its unique member. What this suggests is that if we are going to use Fregean predicates in the ML then they could be Fregean predicates of *aggregates* rather than of sets: of physical objects rather than of abstract objects.

So, *if* the conceptual involvements of articulated predication as used to represent group readings are to be read off from a *Fregean* ML in which a truth theory is cast, then – I suggest – what is involved is some concept tantamount to that of an aggregate, as in Tyler Burge's 'A Theory of Aggregates'.

However, there is a second reason why I do not think that the proposal to use articulated predication has the implausible consequence that mastery of group readings involves employment of the concept of a sequence. If this second reason is correct, then we shall not even have to accept the less implausible consequence that mastery of group readings requires the concept of an aggregate.

As Taylor himself points out, it is not essential that a systematic truth theory for a language with articulated predication should be cast in a Fregean ML. Rather than have such a discrepancy between the OL and the ML, we can cast a truth theory in a ML which itself contains articulated predicates.

The issues here are quite delicate, since it is certainly possible to obscure the conceptual commitments of speakers of a language. It is, for example, a substantive question whether speakers of a modal language are committed to some notion of possible world, possible situation or possible state of affairs; and the question becomes pressing when the modal OL contains not just the simple modal operators but, say, indexed necessity and actuality operators which mimic the complexity of quantifiers and variables. This question is surely not settled merely by pointing out that we can give a systematic truth theory for such an OL without explicit quantification over worlds or possibilities if we cast the truth theory in a ML which itself contains the indexed operators. (On this issue see Forbes, *The Metaphysics of Modality*, Chapter 4, especially pp. 93–4.)

But, although the issues are delicate, we are entitled to claim that there is no purely semantic argument to show that articulated predication is *really* just ordinary Fregean predication of extraordinary objects. That is, it is not clear that the concept of a set, or an aggregate, or of any kind of object other than ordinary individuals is essentially involved in mastery of the group or collective readings of sentences like (2), (4), and (8). (In 'To Be Is To Be a Value of a Variable (Or To Be Some Values of Some Variables)', George Boolos argues that 'neither the use of plurals nor the employment of second-order logic commits us to the existence of extra items beyond those to which we are are already committed' (p. 449). See also his 'Nominalist Platonism'. I shall return to this point in Section 9.)

6. Two-quantifier sentences

Iterated deployment of the semantic resources involved in the distributive and collective readings of one-quantifier sentences evidently provides, in principle, for *eight* readings of

(1) Two examiners marked six scripts.

If the 'two' quantifier has wider scope then we have

(i) $(2x)(Ex; (6y)(Sy; M(x; y)))$
(ii) $(2x)(Ex; (6y)(Sy; M(x^{\rightarrow}; y)))$
(iii) $(2x)(Ex; (6y)(Sy; M(x; y^{\rightarrow})))$
(iv) $(2x)(Ex; (6y)(Sy; M(x^{\rightarrow}; y^{\rightarrow})))$

and similarly there are four readings in which the 'six' quantifier has wider scope. However, a moment's reflection is enough to see that the two scope differentiated pure collective readings (iv) and

(viii) $(6y)(Sy; (2x)(Ex; M(x^{\rightarrow}; y^{\rightarrow})))$

are truth conditionally equivalent. So that leaves seven *prima facie* non-equivalent readings; and these are indeed non-equivalent in the sense that for each pair there is some binary predicate which can take the place of 'marked' to yield clearly non-equivalent readings. On the other hand, it does seem that for most such binary predicates some of the seven readings do turn out to be equivalent. In each case the pattern of equivalences and non-equivalences is determined by the nature of the relation expressed by the binary predicate.

It might be useful to have a complete taxonomy of binary relations. But I shall simply focus on a few examples. Let us begin with sentence (1), which is in relevant respects analogous to

(10) Two men pushed six VWs up the hill.

Clearly the two scope differentiated pure distributive readings are non-equivalent, and reading (i) is not equivalent to reading (ii). On the other hand, readings (i) and (iii) are equivalent, for the only way for an examiner to mark six scripts taken together as a group is for him to mark each of the six scripts; and similarly for men pushing VW's. That is to say that

(3) Tom marked six scripts

does not have a distinct group reading. It does not follow from this that readings (ii) and (iv) are equivalent. While (ii) requires that each examiner made a contribution to the marking of *each* script, (iv) requires only that each examiner made a contribution to the task of marking the six scripts *taken together*. Since there are no equivalences amongst the four readings in which the 'six' quantifier has wider scope, we have the following table for sentences (1) and (10). (I use *italic* numerals to indicate a *distributive* reading, and **bold** numerals to indicate a **collective** reading.)

Table I

(i)	*(2)(6)*	(v)	*(6)(2)*
(ii)	**(2)** *(6)*	(vi)	**(6)** *(2)*
(iii)	*(2)* **(6)**	(vii)	*(6)* **(2)**
(iv)	**(2)(6)**	(viii)	**(6)(2)**

I said that, for sentence (1), readings (i) and (iii) are equivalent, and that this is essentially because sentence (3) does not have a distinct group reading. However, some may want to distinguish a group reading as follows. If Tom marked six scripts simultaneously, or immediately consecutively, then both the distributive and the group reading are true, whereas if Tom marked six scripts separately and unrelatedly, then the distributive reading is true, but the group reading is false. If that distinction is made then the group reading entails the distributive reading, but not *vice versa*. Clearly, we could discuss readings (i) and (iii) further. But more important than whether there is or is not a distinction is something that the putative distinction very naturally suggests. This is that the distinction between group and distributive readings is a matter of whether the truth of a sentence on the reading requires the occurrence of a single large event, or merely several smaller events. I shall come back to this suggestion in the next section.

A pattern of equivalences and non-equivalences different from that exhibited in Table I is provided by the sentence

(11) Two punks fought six skinheads.

Indeed, we would intuitively expect more equivalences, since if two punks jointly fought someone then each of them fought him, whereas if two examiners jointly marked my script then they did not each mark it. Such is the difference between skinheads having been fought and scripts having been marked. Thus, to the same extent as before, readings (i) and (iii) are equivalent; but now, to that same extent, readings (v) and (vii) are also equivalent. What is more, readings (ii) and (vi) are equivalent: each requires that each of two punks fought each of (the same) six skinheads. So we have the following table, revealing just four inequivalent readings.

Table II

(i)	*(2)(6)*		(v)	*(6)(2)*
(ii)	**(2)***(6)*	⟷	(vi)	**(6)***(2)*
(iii)	*(2)***(6)**		(vii)	*(6)***(2)**
(iv)	**(2)(6)**	⟷	(viii)	**(6)(2)**

The same table would serve for

(12) Two students annotated six papers
(13) Two girls kissed six boys.

For sentences (1) and (10), readings (i) and (iii) are equivalent in virtue of the nature of being marked and being pushed. In particular, (iii) entails (i) because the only way for six scripts jointly to be marked is for each to be marked, and the only way for six VW's jointly to be pushed up the hill is for each to be pushed up the hill. For sentences (11), (12) and (13), readings (v) and (vii) are equivalent in virtue of the nature of fighting, annotating and kissing. In particular, (vii) entails (v) because the only way for two punks jointly to fight a skinhead is for each to fight him, and similarly for annotating and kissing. Let us then look at a sentence for which neither of these entailments holds.

(14) Two sheets of paper exactly covered six cards.

There is, for example, a world of difference between each of six cards being exactly covered by two sheets of paper *individually*, and each of six cards being exactly covered by two sheets of paper *jointly*. So readings (v) and (vii) are non-equivalent; and, in particular, (vii) does not entail (v). Similarly, readings (i) and (iii) are non-equivalent. And it is even easier to see that readings (ii) and (vi) are non-equivalent.

7. Event quantification

In the discussion following Table I, I mentioned the suggestion that the difference between the group and the distributive readings of

(2) Two examiners marked my script

is a matter of whether the truth of the sentence on the reading requires the occurrence of a single script marking event, or the occurrence of two separate script marking events. This suggestion could also be applied to

(3) Tom marked six scripts.

If Tom marked six scripts simultaneously, or immediately consecutively, then the individual script marking events might be allowed to add up to a single larger script marking event, thus rendering true a distinct group reading. If, on the other hand, Tom marked six scripts separately and unrelatedly, then those individual script marking events would not jointly constitute a single event, and so only the distributive reading would be true.

The thought behind this suggestion is that, quite generally, the distinction between group and distributive readings is a matter of the relative scope of an implicit quantifier over events. If Davidson's familiar proposal ('The Logical Form of Action Sentences') is to be applied to sentence (2), then we have to decide whether the existential quantifier over events has wider or narrower scope than the numerical quantifier over examiners. If the event quantifier has wider scope, then what (2) says is that there is a single event e, and there are two examiners x and y, such that e is an event of marking my script, and e is by x and also by y – that is, x and y are both agents in e. If, on the other hand, the event quantifier has narrower scope, then what (2) says is that there are two examiners x and y, for each of whom there is a marking event; that is, there are events e_1 and e_2 such that e_1 is a marking of my script and is by x, and e_2 is a marking of my script and is by y.

It is initially quite plausible that giving the event quantifier wide scope yields the group reading of (2) while giving the event quantifier narrow scope yields the distributive reading. That is, it is initially plausible that the distinction between the group and the distributive reading is simply constituted by a scope distinction. However, this cannot be quite right as it stands. For the wide scope version of (2) actually entails the narrow scope version (e_1 and e_2 are not necessarily distinct in the narrow scope version); but the group reading of (2) certainly does not entail the distributive reading. This failing in the proposal is not so evident if we concentrate on sentence (3). For, if a group reading is forced upon that sentence, then it does indeed entail the distributive reading.

This problem can be overcome if we replace the notion of an event being by an agent or agents with the notion of an event being by an agent or agents and by no one else. Clearly, given that change, the wide scope version of (2) no longer entails the narrow scope version.

However, even after this modification, we have to reject the suggestion that group readings are simply the result of a wide scope quantifier over events. The scope difference is indeed important. But it does not correspond perfectly with the difference between group and distributive readings.

To see why the scope difference is important, we can borrow an example from Taylor (*Modes of Occurrence*, p. 17; 'Events and Adverbs', p. 103).

(15) Henry gracefully ate all the crisps.

This sentence is ambiguous. On one reading, its truth requires that each individual crisp eating should have been graceful. On the other reading, all that is required is that the total performance of eating all the crisps should have been a graceful one. The two readings can be distinguished by a scope difference.

Roughly, for the first reading of (15) the universal quantifier over crisps has wider scope than the existential quantifier over events, while for the second reading the scopes are reversed. If the event quantifier has wider scope, then (15) says that there is an event e which is an eating by Henry, such that for each crisp x, e is of x – that is, each crisp is a patient in e – and e is graceful. If the event quantifier has narrower scope, then (15) says that for each crisp x there is an event e_x which is an eating by Henry, such that e_x is of x, and e_x is graceful.

This is only rough, since the wide scope version of (15) actually entails the narrow scope version, whereas the whole point of the example is that a crisp eating performance that is graceful overall may yet involve certain individual crisps being eaten gracelessly. To overcome this problem, we need to replace the notion of an event being of a patient or patients by the notion of an event being of a patient or patients and of nothing else. (See again Taylor, *Modes of Occurrence*, p. 18.)

Sentence (15) shows why the scope difference is important. But giving the event quantifier wide scope does not force a group reading upon such a sentence. This fact is not immediately clear from the example, because there is no clear sense to be attached to joint 'patiency' on the part of the crisps. But consider now the sentence

(16) Twelve football players pushed the pram up the hill.

This sentence certainly has distinct group and distributive readings. But if we fix upon the distributive reading of (16), we *still* need to make a distinction analogous to that in (15) when we turn to the adverbially modified sentence

(17) Twelve football players pushed the pram up the hill gracefully.

There are two distributive readings of sentence (17). On one reading, its truth requires that each individual pushing of the pram should have been graceful. On the other reading, all that is required is that the overall performance should have been a graceful one, even if one or two of the football players went in for some graceless pushing.

If we follow Taylor's treatment of the ambiguity in sentence (15) then, in order to capture the second of these readings of sentence (17), we shall give the event quantifier wide scope. This is enough to show that an event quantifier having wide scope is not all that is distinctive about group readings. None of this is to suggest that it is obvious how my own proposals are to be extended to handle the group and the distributive readings of sentences like (17). The point is that, although we do need event quantification and the scope distinctions that it brings, we also need the notion of joint agency; and it is that latter notion that I am proposing to represent using articulated predication. (I would also reject the suggestion that the difference between group and distributive readings has to do with the relative scope of a temporal quantifier. See Hintikka, 'Temporal Discourse and Semantical Games', especially p. 6.)

8. Comparison with Kempson and Cormack

The time has come to compare the various readings of two-quantifier sentences obtained so far with the four interpretations distinguished by K & C. What is not at all controversial is that K & C's scope differentiated distributive interpretations (IV) and (V) are second order equivalents of my readings (i) and (v). Let us now focus on the examples (11), (12), and (13) to which Table II applies. K & C's incomplete group interpretation (VI) is a second order equivalent of my readings (iv) and (viii) in these cases, because in virtue of what fighting is, for example, if two punks (jointly) are to fight six skinheads (jointly) then there has to be a set X of two punks and a set Y of six skinheads such that each member of X fights at least one member of Y and each member of Y is fought by at least one member of X. Similarly in these cases, K & C's complete group interpretation (VII) is a second order equivalent of my readings (ii) and (vi).

But these equivalences between K & C's interpretations and my readings hold only in virtue of the meanings of the binary predicates in the particular examples. If we turn now to examples (1) and (10) to which Table I applies, we find a sharp discrepancy between the two sets of proposals. What we expect, and what we find, is that none of the K & C interpretations is equivalent to any of my readings in which the subject noun phrase is read collectively rather than distributively (or more accurately, in which the subject position of the transitive verb is treated collectively rather than distributively). Thus, my readings (ii), (iv) (i.e., (viii)), and (vii) go uncaptured by the K & C proposals. The K & C complete group interpretation (VII) is in these cases, as in the cases to which Table II applies, a second order equivalent of my reading (vi). But the K & C incomplete group interpretation (VI) is not a second order equivalent of any of my readings.

Does this fact show that my semantic proposals are inadequate as applied to examples like (1) and (10)? Not unless, in such cases, the K & C incomplete group interpretation (VI) can be heard as a distinct interpretation. My intuitions are that, when we have a case in which there is a possibility of genuine irreducibly joint agency, a sentence like (1) or (10) does not have a distinct reading equivalent to the incomplete group interpretation (VI). The situation described in (VI) is just one way in which the sentence read as my (iv) (or (viii)) could be true. *One way* in which two examiners can jointly mark six scripts taken together as a group is for one of the examiners to mark (individually and completely) two of the scripts and the other examiner to mark the remaining four scripts. But there are plenty of other ways, in many of which no examiner completely marks any script.

For the examples to which Tables I and II apply the K & C complete group interpretation (VII) is a second order equivalent to my reading (vi). But this is not generally the case, as example (14) shows. Are we to say that the *complete* group interpretation, like the *incomplete* group interpretation, is not a genuinely distinct reading?

9. Branching Quantifiers

There are significant disanalogies between the case of the complete group interpretation and the case of the incomplete group interpretation. I have claimed that the incomplete group interpretation (VI) is not in general a distinct reading. An initial intuition that the incomplete group interpretation is a distinct reading can be accounted for as follows. That interpretation is, in many cases, a second order equivalent of the pure collective reading (iv) (or (vii)); and when it is not an equivalent it usually at least describes a situation in which reading (iv) is true.

But the complete group interpretation is not so easily regarded as an approximation to one of the readings so far distinguished. Certainly it is, in some cases, a second order equivalent of reading (vi), but when it is not an equivalent it typically describes a situation in which reading (vi) is false. Further evidence that the complete group interpretation is only fortuitiously related to reading (vi) is provided by the following fact. One can encourage reading (vi) of, for example, sentence (1) by shifting to the passive and adorning the result with 'together' and 'each' thus.

(1a) Six scripts *together* were marked by *each* of two examiners.

But quite generally, the complete group interpretation can be encouraged in a different way, namely by doubly adorning the sentence with 'each' thus.

(1b) *Each* of two examiners marked *each* of six scripts.

And note that shifting to the passive seems to make no relevant difference.

Doubtless, one should not rest a semantic proposal on such impressionistic remarks as these. Nevertheless, what is suggested is this. Since 'each' clearly requires a distributive reading, the complete group interpretation is not in general to be seen as an approximation to (vi) or to any purely or partly collective reading. Rather, it is a purely *distributive* reading not accounted for by my proposals so far. Further, since 'each' seems to indicate wide scope, the complete group interpretation is a purely distributive reading in which neither numerical quantifier has scope wider than the other. In short, the impressionistic remarks suggest that the complete group interpretation is the branching quantifier reading, which might be represented thus.

(1c) $$\left.\begin{array}{l}(2x)(Ex;\\ \\ (6y)(Sy;\end{array}\right\rangle M(x;y))$$

And indeed the truth conditions of the branching quantifier reading are just those of the complete group interpretation.

Branching quantification is a new semantic resource, going beyond mere iteration of the resources involved in our semantic theorising about one-quantifier sentences. Indeed, the introduction of branching quantification has serious consequences for systematic (that is, compositional) semantic theorising. In 'On Branching Quantifiers in English' (to which I referred in Section 2), Jon Barwise put the point this way.

> It is not possible to explain the meaning of an essential use of branching quantification ...

inductively by treating one quantifier at a time in a first-order fashion. Some use of higher type abstract objects is essential. (pp. 74–5)

An essential use of branching quantification is one that is not equivalent to any standard – that is, linear – first order formula. In such a case, any equivalent linear formula will involve second order quantification. Second order quantification is typically interpreted as quantification over higher type abstract objects – sets and functions; hence Barwise's point.

However, we face a complication in our exposition here. For we have already noted (at the end of Section 5) that Boolos rejects this interpretation of second order logic. What we need to say, if we are to follow Boolos, is that branching first order quantification hides second order semantic resources. And while those second order resources – 'there are some . . .', 'each of them', 'some of them' – may not really involve the concepts of set and function, they are new resources even so. Consequently, systematic mastery of the branching quantifier reading of two-quantifier sentences – such as (1), (10), (11), (12), (13), and (14) – requires a level of conceptual sophistication going beyond what is required for the other readings.

Although branching quantification is a new semantic resource, it need not be a mystery how the branching quantifier reading – with the truth conditions of the complete group interpretation – becomes available to a speaker with the requisite conceptual sophistication. (See Barwise, 'On Branching Quantifiers in English', p. 63.) And it would be misleading to say, simply, that the branching quantifier reading presents more difficulties than any of the other readings. For there is also a feature of the branching quantifier reading which makes for less difficulty, namely that the hearer does not need to keep track of the relative scopes of the two quantifiers.

10. Three questions revisited

In Section 1, I raised three vague questions about K & C's proposals. I end with some reflections loosely organised around those questions.

(i) The first question was whether the general rules of Generalising and Uniformising can belong to the semantic component of a theory, given that, on the face of it, they are just rules for syntactically transforming one formula into another. In the subsequent exchange between K & C (Cormack and Kempson, 'On "Formal Games and Forms for Games"') and Tennant ('Formal Games and Forms for Games'), there are suggestions that, within the framework of game theoretic semantics, these rules might be replaced by operations on games. But I shall not pursue that suggestion here.

The general rules are needed to derive the various interpretations from a single, initial semantic representation, itself weaker than (in the sense of being entailed by) the various interpretations. But I am not myself much attracted to the idea of a single weakest semantic representation, both because, as Tennant notes, the partial ordering by entailment may have 'local minima but no overall minimum' (p. 316), and because the idea would lead to different semantic treatments of intuitively analogous phenomena. The proposals I have sketched would, I think, be compatible with the assignment, at some level of description, of a single *syntactic* form to a sentence such as (1). But this would not be thought of as determining a semantic representation. It would, rather, correspond to several different forms to which distinct truth conditions would be assigned by the semantic component of a total theory.

(ii) The second question was whether K & C's interpretations (IV)–(VII) get the truth conditions of the various readings right. As we have seen, the main problem is over their incomplete group interpretation (VI) which, I assume, is intended to capture what I have called the pure collective reading. For a range of examples, the K & C incomplete group interpretation is a second order equivalent of my pure collective reading, and indeed, the verb 'marked' can perhaps be so construed that K & C's own example – the sentence (1) – falls within that range. But this equivalence, when it does obtain, is fortuitous. It depends upon the properties of the particular real relation in question. There are other examples, intuitively of the same semantic structure, for which the incomplete group interpretation is not a second order equivalent of the pure collective reading.

(iii) The third question was whether it is desirable that all four of K & C's interpretations should be represented as involving quantification over sets. The first kind of constraint on semantic theories that I mentioned in Section (2) was intended to respect the intuition behind that question. Intuitively, a person who uses sentence (1) on any of its readings other than the complete group interpretation (the branching quantifier reading) does not thereby express a belief about sets, and does not even require the concept of a set. My proposals accord with that intuition. Second order resources come into the picture only when we intuitively expect them to do so. Otherwise, the level of conceptual sophistication attributed to speakers is very modest.

11. CONCLUSION

Numerically quantified sentences admit of several different readings or interpretations. Consequently, they constitute a case study for the distinction between semantics and pragmatics. Are the various readings generated within the systematic semantic component of a linguistic theory? Or does semantic theory yield but a single form for such sentences, leaving pragmatic factors to generate the various readings in context? If the readings are generated pragmatically, are they the products of implicatures added on to some literal interpretation? Or is the contribution of semantic theory merely an outline or schema for determinate interpretations?

Kempson and Cormack's account has two main aspects. First, they propose determinate truth conditions for four different readings of two-quantifier sentences. Second, they propose that the different readings are derived from a single semantic representation, but by rules which themselves belong within semantic theory.

I have challenged their claim to have provided the correct truth conditions for the readings of two-quantifier sentences; and I have distinguished rather more readings than they do. Guided by the methodological considerations of Section 2, I have connected the readings of two-quantifier sentences with the distinct group and distributive readings of one-quantifier sentences. And I have been rather fussy about the concepts involved in the specifications of truth conditions. I have not distinguished any single initial semantic form, underlying the various readings.

It would be possible for someone to accept that I have correctly specified the truth conditions for the available readings of these sentences, but to maintain that those readings are, nevertheless, generated – and not just selected amongst – pragmatically. In particular, it is possible for someone to offer a non-specificity account of the interpretations of numerically quantified sentences. Such a theorist owes us an answer to the questions raised in the Introduction; but there is nothing in this paper that shows such a theorist to be wrong. Where to draw the line between semantics and pragmatics remains an open question.[1]

NOTE

[1] A much earlier version of this paper was read at the Conference on The Psychological Content of Logic, held at Tilburg University in the Netherlands in October 1982. Most of the work towards the present version was completed while I was visiting the Center for the

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