

Variables

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Abstract

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Doctor of Philosophy in Philosophy

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*Variables* is a project at the intersection of the philosophies of language and logic. Frege, in the *Begriffsschrift*, crystalized the modern notion of formal logic through the first fully successful characterization of the behaviour of quantifiers. In *Variables*, I suggest that the logical tradition we have inherited from Frege is importantly flawed, and that Frege's move from treating quantifiers as noun phrases bearing word-world connection to sentential operators in the guise of second-order predicates leaves us both philosophically and technically wanting.

Technically, the Fregean conception of a quantifier leaves us lacking adequate tools for judging the extent of the notion of quantification, a lack which grows more pressing as recent work in branching, cumulative, polyadic, plural, substitutional, and higher-order quantification stretches the boundaries and conceptual underpinnings of the classical notions. Philosophically, the Fregean conception bars the way to an adequate understanding of the role of quantification in natural language and of the connection between on the one hand referential terms and their propensity to create singular

thoughts and on the other hand quantificational terms and their propensity to create general thoughts.

Rejecting the dominant Fregean conception, I explore a semantic understanding of quantifiers that takes seriously their status as variable binding operators and thus provides a distinct semantic role for the variable. This new understanding reverses the Fregean innovation and restores the connection between quantification and reference; thereby a unified account of natural language noun phrases becomes possible. Noun phrases are argued all to be variables at heart. Languages then impose varying levels of logical structure on those variables, from the minimal case of the (referential) free variable to the intermediate case of the plurally referential 'donkey' pronoun to the full-blown case of the quantificational noun phrase.

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## Preface

### §0.1 Variables

The goal of this work is to examine the semantic behaviour of that most neglected of lexical items, the variable. As will be made clear in chapter 1, most philosophers and logicians who have written on the semantics of natural and formal language have had only the most cursory of comments on the variable. A few elementary misconceptions are generally dispelled, such as the fallacy of conflating variability in the linguistic item with variability in the world,<sup>1</sup> but vague and misleading metaphors are then evoked in place of an explicit semantic analysis. My contention is that the evasion of a serious theory of the variable is not a harmless matter.

While variables have remained an underexplored region of semantic space, the quantifiers which bind variables have not. Quantification is perhaps the most thoroughly discussed topic in the large area of philosophy of language, formal logic, and linguistics. As a result of this thorough examination and superabundance of work, it has become increasingly clear that isolation of a core notion of quantifier, a notion which will unify the various species of quantification and explain the position quantifiers hold in the philosophical firmament, is a highly non-trivial task. A large part of my motivation for pursuing a semantic analysis of the lowly variable is a conviction that other attempts to understand quantification have gone wrong by underplaying

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<sup>1</sup>A 'fallacy' which [Fine 1985] has recently suggested ought to be endorsed as a profitable route to better understanding the nature of natural deduction proof systems.

the *prima facie* central fact about quantifiers: that they are *variable binding operators*.

In place of others' evasion of the semantic analysis of the variable, I develop a substantive theory which takes natural language pronouns and their anaphoric behaviour as the general model for variables. Once the account of variables is in place, a new way of thinking about quantifiers, a way which takes seriously their status as variable binding operators, falls out naturally. The bulk of this work, then, is devoted to exploring the details of this new anaphoric account of quantification, in order to show that it is better suited than accounts currently on the market to explain and unify the range of quantificational devices which both natural and formal language analysis have developed and to enter profitably into a larger project of the analysis of the logical devices underlying the semantic behaviour of natural language.

## **§0.2 Outline of the Project**

Using the importance of quantification as a route to the importance of variables, Chapter 1 opens with a discussion of the desiderata which a philosophically satisfying account of quantification ought to meet. It then examines some of the technical complexities of quantification which make the meeting of these desiderata such a complicated task. Coupling the suggestion that certain failings of what I take to be the dominate neo-Fregean account of quantification motivate a closer look at the semantics of variables with the observation that previous philosophers' comments on variables are far from satisfactory as a *theory* of variables, the chapter proceeds first to determine what kinds of

questions a theory of variables ought to answer and second to sketch my particular theory. That theory, roughly speaking, holds that variables are lexical items with anaphoric propensities, which inherit semantic content from binding operators in the lexical environment.

Chapter 2 is the heart of the work. Here I turn to justifying the claim that the anaphoric account of variables leaves us better positioned to understand the underlying logic of natural language than do competing accounts. After addressing certain preliminary questions about the relation between natural and formal languages and about the distribution of variables in natural language, I suggest a broad taxonomical picture of natural language noun phrases, one which falls naturally out of the picture of variables and variable binding I propose. On this picture, the variable is the heart of every noun phrase, and noun phrases are distinguished in type simply by the degree of binding apparatus brought to bear upon the core variable. I argue that the apparent exceptions to this quantificational paradigm of the noun phrase -- on the one hand quantifier-like phrases which for various technical reasons resist quantificational treatment, such as bare plurals and cross-clausally anaphoric pronouns; and on the other hand apparently non-quantificational referential noun phrases -- can be brought under the umbrella of my view first by establishing and exploiting the fact that the anaphoric account of variables gives rise to a notion of variable binding not limited by syntactic features of scope and second by suggesting a model of natural language understanding on which referential noun phrases correspond to free variables, semantics issues in subpropositional meanings, and pragmatic devices are heavily exploited to obtain propositional speakers' meanings. The overarching

claim of Chapter 2 is that the success of the anaphoric account in unifying diverse natural language phenomena gives us good reason to think that that account hits on central features of the semantics of variables and quantification.

Having thus defended the utility of my account, I proceed in chapter 3 to work out finer details concerning the function of the two central notions of the account -- variable restriction and variable distribution. In the first half of chapter 3 I take up variable restriction, addressing first the reasonably well-understood case of first-order restriction. Noting that such restriction, as I understand it, will require an account of plural reference, I proceed to make some programmatic remarks on the semantics and ontology of plurals. I also draw some consequences of the idea that quantification is essentially restricted quantification, discussing ramifications both for other contemporary accounts of quantification and for issues of ontological investigation. I also take up briefly the nature of higher-order restriction (and hence higher-order quantification) under my account, raising questions about the precise nature of potential higher order restrictors.

In the second half of chapter 3 I take up variable distribution. In this primarily technical discussion, I compare the structural features of distributors as I understand them with the structural features of quantifiers and determiners on standard neo-Fregean accounts, arguing that my account yields a formal landscape more naturally suited to natural language analysis. Finally, I examine in some detail the source of order-dependence among quantifiers, showing

that an order-independent ('branching') notion of quantification is also readily available on my account.

Chapter 3 closes with a discussion of the compositionality of variable restriction, acknowledging that my account has certain (harmless) non-compositional features but arguing both that we should expect such features here and that competing accounts implicitly contain similar non-compositionality.

As I have proceeded with this project, the daunting scope both of reconceiving certain notions which have been at the core of formal logic ever since Frege and of attempting to root out the deep origins of some of the fundamental issues in natural language semantics has become increasingly clear to me. This work obviously represents only a small first step in these directions. On the formal side, much work remains to be done simply in working out technical details of the new account as I see it. Furthermore, there is important comparative work to be done setting out the basic conceptual differences between my system and the Fregean understanding, work which should help in casting off remaining Fregean presuppositions which I still find infecting my work. On the philosophical side, even more remains to be done. I have tried throughout to indicate places where particularly glaring open issues remain.

### **§0.3 Acknowledgements**

My primary debt in this work is to Stephen Neale. His work has obviously served as a starting point for much of what I say here, especially in Chapter 2. Comments from and conversation with him has done much to clarify and improve every aspect of the work. Specific details are too

numerous to mention, but to pick one, it was a technical challenge issued by Stephen several years ago which led me to see that the apparatus of variable binding could (and should) be subdivided the way I have done here. I am also deeply indebted to Charles Chihara, whose comments and conversations have also been invaluable. Early pressure from Charles to give a precise formal statement of the sketchy ideas presented in Chapter 1 lead to numerous clarifications and improvements in my account of quantification. Also, it was as a result of a seminar given by Charles in the fall of 1993 that I first saw the problems with Sher's account of branching quantification, problems which eventually lead to the positive proposal which makes up the bulk of §3.3.

Numerous others have contributed to smaller parts of the total work. The idea that referential terms can be treated as free variables has its roots in a seminar given by François Récanati in the spring of 1994. François also provided valuable commentary on what is now §2.3.3, raising questions the addressing of which lead in part to §2.3.4. An early version of what is now §2.3.4 was read at the Symposium on Scope and Rigidity in the spring of 1996, and there benefitted greatly from comments by Barry Smith, Scott Soames, and David Sosa. Later versions benefitted further from discussions with Herman Cappelen, David Chalmers, Mark Crimmins, Max Deutsch, Kirk Ludwig, John Madsen, Greg Ray, and Robert Stalnaker.

The discussion of compositionality in §3.4 grew out of classes and discussions with Ernie Lepore in the spring and fall of 1995. That discussion was further refined through the comments of two anonymous reviewers at Linguistics and Philosophy and by comments and discussion received during the compositionality symposium at the 1997 European

Summer School in Language, Logic, and Information, especially from Sean Fulop, Herman Hendricks, Theo Janssen, Hans Kamp, and Peter Pagin. Peter Pagin also provided valuable commentary on the material in §3.2.1.3.2, as did Dag Westerstahl. The question addressed in that section originally grew out of issues discussed with Vann McGee, who also pressed issues about ontological commitment and the relation of my system to second-order logic which have shaped much of my discussion of the neo-Fregean account of quantification.

In the views on proper names developed in §2.3.3 I am particularly heavily indebted. Versions of this material were read in the spring of 1997 at the University of California at Berkeley, the University of Michigan, MIT, the University of Texas at Austin, the University of Chicago, and the State University of New York at Albany. Numerous comments and questions received there and elsewhere, especially from Nicholas Asher, Daniel Bonevac, Mark Crimmins, Alan Gibbard, Delia Graff, James McCawley, Ron McClamrock, Greg Ray, and Barry Smith were invaluable in moving the piece toward its final shape.



## Chapter 1

### An Introduction to Variables

#### §1.1 Quantification

The modern era in philosophy might not unreasonably be said to begin with the 1879 publication of Frege's *Begriffsschrift*. Central to the impact of this work was the first fully successful treatment of quantification, a treatment whose novelty lay in taking quantifiers not as subject terms – as their natural language implementation might suggest – but as sentential operators. This innovation, and its concomitant isolation of quantifiers as the subject of special study, sparked a tremendous explosion of productivity in both philosophy and mathematics.

Formally, the importance of quantification in the early part of this century is clear. The logic which developed out of the work of Frege and Russell was a *quantificational* logic, a logic whose two distinctive (structural) features were truth-functional connectives and quantifiers. This logic, moreover, proved a powerful and fertile tool, giving rise to substantial advances in the foundations of mathematics, as well, in large part, to such distinct fields as computer science and formal linguistics.

Philosophically, the importance of quantification is no less clear. It is no accident that the tremendous philosophical activity of the twentieth century was sparked by formal work in quantification. Quantification, it seems, draws together traditional philosophical issues regarding the nature of truth, the structure of thought, the

relation of beliefs and meanings to the world, and the metaphysical furniture of reality. Consider a couple of these issues in more detail. Quantificational issues have long been tied up with the distinction between singular and general thoughts, an issue which itself has implications for how we understand the nature both of mind and of the world. At least since [Russell 1905, 1911], there has been a tendency to correlate an apparent opposition between quantification and reference with another apparent opposition between general and singular thought.

Quantificational issues also have a long history (containing [Quine 1948] as a distinguished ancestor) of involvement in disputes about the ontological commitments of our beliefs and theories. Clearly, any attempt to determine what commitments we incur when we hold a particular belief necessarily involves looking at the appropriate logical analysis of that belief. But finding the appropriate logical analysis itself involves determining what sort of logic is appropriate for performing the analysis. Classical quantified logic has been taken by many to provide that logic, thus forging a link between quantification and ontological commitment. Those who want to resist the use of classical quantified logic as the bellwether of ontological commitment tend to become even more deeply implicated in philosophical questions about quantification, since they find themselves in the position of arguing that some other understanding of logic — and hence, typically, of quantification — is appropriate. Thus, for example, those who want to claim that sentences like

- (1) Pegasus is a winged horse.
- (2) Most philosophers understand logic.
- (3) No number is the sum of all numbers.

(4) Hesperus is necessarily Phosphorus.

involve no unexpected ontological commitments must further argue that free logics, generalized quantifiers, plural quantifiers, or modal logics (respectively) are appropriate venues in which to perform ontological evaluation. Such arguments are, typically, difficult to make without touching of quantificational issues.

The foundations of our philosophical era, then, are steeped in quantification. The philosophical obsession with quantifiers has, if anything, intensified in recent years. Some 23 years ago, [Hintikka 1973] offered the following assessment of the state of the field:

The syntax and semantics of quantifiers is of crucial significance in current linguistic theorizing for more than one reason. The last statement of his grammatical theories by the late Richard Montague (1973) is modestly entitled 'The Proper Treatment of Quantification in Ordinary English'. In the authoritative statement of his 'Generative Semantics', George Lakoff (1971) uses as his first and foremost testing-ground the grammar of certain English quantifiers. In particular, they serve to illustrate, and show need of, his use of global constraints governing the derivation of English sentences. Evidence from the behaviour of quantifiers (including numerical expressions) has likewise played a major role in recent discussion of such vital problems as the alleged meaning preservation of transformations, co-reference, the role of surface structure in semantic interpretation, and so on.

In the intervening decades, Montague grammar and generative semantics have fallen on hard times, and the most recent Chomskyan minimalist program threatens to remove transformations, meaning-preserving or otherwise, from the scene, but one factor remains constant: quantifiers stand at the center of a vast number of ongoing debates in linguistics, philosophy of language, and philosophy of logic.

When it comes to formal work in quantificational logic, Hintikka's observations are particularly poignant because his paper was one of the early trickles in what was soon to become a philosophical flood:

attempts to augment the traditional quantificational devices of first-order logic. A survey of any technically oriented journal in recent years will uncover numerous articles proposing new understandings of, or new extensions to, our current notions of quantification.

The more purely philosophical implications of quantification also continue unabated. Debates over the distinction between singular and general thoughts have, if anything, picked up steam in recent years, spurred on by such works as [Kripke 1980], [Evans 1982], and [McDowell 1994]. The role of logical regimentation in ontological investigation also remains a live topic, especially within Davidson's work — see especially [Davidson 1976] and [Davidson 1990].

My suggestion in this work, then, is that anyone who recognizes the central position of quantification in contemporary philosophy ought also to recognize the need for an account of *what quantification is*. That need is felt all the more strongly by those also familiar with the recent explosion of quantification apparatus in the literature. An adequate account of quantification must tell us what quantification is in such a way as to meet two desiderata:

**Explanation:** First the account must be explanatory, in the sense that it must explain what it is about quantification that has caused it to occupy the prominent position it has. We want an account that will explain why quantification has been and remains a central and recurrent issue in philosophy of language and related areas, and an account that will allow us to trace out how quantification is connected to those philosophical debates which tend to evoke it.

**Unification:** Despite (or perhaps because of) the remarkable body of technical work on the huge variety of technical devices currently

available on the market, we still lack fundamental insights into the nature of quantification. Even in the case of classical logic, we can ask what it is that makes existential and universal quantifiers instances of the same type of thing; this kind of question becomes even more pressing as the number and diversity of quantifier types increases. As we will see in the next section, it is no easy task drawing out what is common to all of what are now claimed to be quantifiers.

The goal of this work, then, is to develop, defend, and deploy an explanatory and unificatory account of the nature of quantification.

## **§1.2 The Diversity of Quantification**

Classical logic, as developed out of the Frege-Russell tradition with its roots in mathematics and the rigorization of analysis, has imparted to us two paradigm cases of quantification: the universal and existential quantifiers. However, subsequent logical and linguistic work has stretched considerably the boundaries of the notion of quantifier — stretched, as we will see, possibly to the breaking point. In this section, I want to provide a brief introduction to several of the major proposed extensions to the historical core notion of the quantifier. None of the following discussions is meant to be exhaustive; the goal is merely to induce enough familiarity to allow the reader to see the scope and formidability of the unificatory and explanatory task we have set for ourselves.

### **§1.2.1 Trivial Extensions of the Classical Paradigm**

Let's begin with a very small addition to the classical ' $\forall$ ' and ' $\exists$ '. We can introduce further quantifiers such as ' $\exists!$ ', ' $\exists^2$ ', or ' $\exists!$ '

(intended to mean 'no', 'at least two', and 'exactly one', respectively)

by adding to our formal language syntactic clauses such as:

(AX1) If  $\phi$  is a well formed formula and  $\chi$  is a variable, then

$(N\chi)\phi$  is a well formed formula.

(AX2) If  $\phi$  is a well formed formula and  $\chi$  is a variable, then

$(\exists 2\chi)\phi$  is a well formed formula.

(AX3) If  $\phi$  is a well formed formula and  $\chi$  is a variable, then

$(\exists !\chi)\phi$  is a well formed formula.

Such new quantifiers, of course, now require semantic interpretation.

Such interpretation is easily provided, however, since these quantifiers

bring no addition to the expressive power of classical languages. Each

can thus be contextually defined using the classical ' $\forall$ ' and ' $\exists$ '

coupled with various Boolean operators. Thus we have:

(Def. 1)  $\Sigma[(N\chi)\phi(\chi)] =_{\text{def}} \Sigma[\neg(\exists\chi)\phi(\chi)]$

(Def. 2)  $\Sigma[(\exists 2\chi)\phi(\chi)] =_{\text{def}} \Sigma[(\exists\chi_1)(\exists\chi_2)(\phi(\chi_1) \wedge \phi(\chi_2) \wedge \chi_1 \neq \chi_2)]^2$

(Def. 3)  $\Sigma[(\exists !\chi)\phi(\chi)] =_{\text{def}} \Sigma[(\exists\chi_1)(\forall\chi_2)(\phi(\chi_2) \leftrightarrow \chi_1 = \chi_2)]$

In the light of this contextual definability, it is reasonably clear that any explanatory theory of quantification which suffices to cover the classical cases will also suffice to cover these minor extensions of those cases.

### **§1.2.2 Less Trivial Extensions of the Classical Paradigm**

That the classical quantifiers could be combined with the Boolean resources of the formal language to create the sorts of extensions discussed in the previous section was more or less immediately obvious to the founders of modern logic, and was famously exploited to

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<sup>2</sup>Where, of course,  $\chi_1$  and  $\chi_2$  are to be chosen so as not to appear elsewhere in  $\Sigma$ .

philosophical gain by [Russell 1905]. That, however, more ambitious extensions of the classical paradigm could be achieved through direct manipulation of the semantic metatheory of a formal language had to await the implementation of an adequate such metatheory by [Tarski 1933]. Even then, it was not until [Mostowski 1957] that the idea of adding genuinely non-classical quantifiers to a formal language was explored.

Mostowski's work, and later refinements of it, took as its central idea that new quantifiers could be added to the language not through contextual definition using the syntactic resource of the pre-augmentation language, but through providing additional semantic clauses for the new quantifiers. Assume we have a semantic metatheory which provides clauses such as the following for the existential and universal quantifiers:

(AX 4) A sequence  $\sigma$  satisfies ' $(\forall\chi)\varphi(\chi)$ ' iff every sequence  $\sigma'$  which differs from  $\sigma$  at most in the  $\chi$  position satisfies  $\varphi(\chi)$ .

(AX 5) A sequence  $\sigma$  satisfies ' $(\exists\chi)\varphi(\chi)$ ' iff some sequence  $\sigma'$  which differs from  $\sigma$  at most in the  $\chi$  position satisfies  $\varphi(\chi)$ .

Then we can introduce similar clauses for new quantifiers not contextually definable using the classical pair. Quantifiers such as the following are then possible:

(AX 6) A sequence  $\sigma$  satisfies ' $(\infty\chi)\varphi(\chi)$ ' iff infinitely many sequences  $\sigma'$  which differ from  $\sigma$  at most in the  $\chi$  position satisfy  $\varphi(\chi)$ .

(AX 7) A sequence  $\sigma$  satisfies ' $(M\chi)\varphi(\chi)$ ' iff most sequences  $\sigma'$  which differ from  $\sigma$  at most in the  $\chi$  position satisfy  $\varphi(\chi)$ ).

(AX 8) A sequence  $\sigma$  satisfies ' $(f\chi)\varphi(\chi)$ ' iff few sequences  $\sigma'$  which differ from  $\sigma$  at most in the  $\chi$  position satisfy  $\varphi(\chi)$ ).

Once we begin to introduce new quantifiers in this way, of course, a can of worms has been opened up. The definition of a quantifier has been expanded in an only vaguely defined way. What kind of semantic clause will suffice successfully to introduce a new quantifier?<sup>3</sup>

### §1.2.3 Natural Language Quantifiers

While modern quantificational theory began its life imbedded in formal languages such as that set out in the *Begriffsschrift*, it was from the beginning obvious that natural languages employ similar devices of quantification. Thus, corresponding to the formal:

(1)  $(\forall x)(Fx \rightarrow Gx)$

(2)  $(\exists x)(Fx \wedge Gx)$

we have natural language:

(1') All frogs are green.

(2') Some frogs are green.

---

<sup>3</sup>Note, for example, that all the Boolean sentential connectives can be given semantic clauses which look much like the clauses given for ' $\forall$ ' and ' $\exists$ '; witness:

(AX FN 1) A sequence  $\sigma$  satisfies ' $\neg\varphi(\chi)$ ' iff no sequence  $\sigma'$  which differs from  $\sigma$  nowhere satisfies  $\varphi(\chi)$ .

(AX FN 2) A sequence  $\sigma$  satisfies ' $\varphi(\chi) \wedge \psi(\chi)$ ' iff every sequence  $\sigma'$  which differs from  $\sigma$  nowhere satisfies  $\varphi(\chi)$  and satisfies  $\psi(\chi)$ .

Should we take the availability of such semantic clauses as evidence that all Boolean connectives are species of quantifiers?



An adequate account, of quantification, then, must be also be able to account for the functioning of quantificational phrases in natural language. Such an account faces to immediate hurdles. First, on analogy with the concerns of the previous section, we must account for the remarkable diversity of natural language quantificational phrases, which might well include 'no frogs', 'many frogs', 'every other frog', 'more frogs than toads', and many others.<sup>4</sup> Second, we must account for the shift from the formal quantifier, which is an isolated unit acting as a sentential operator, to the natural language quantifier, which is coupled with a common noun to form a noun phrase.

#### **§1.2.3.1 Definite and Indefinite Descriptions**

One manifestation of the first of these two problems which has plagued philosophers is a persistent debate over the semantics of definite and indefinite descriptions. Naïvely (so goes the common wisdom), definite and indefinite descriptions are most closely allied to referential terms like demonstratives and proper names — terms which are used to pick out particular objects, rather than to make assertions about how many objects possess a given property. [Russell 1905], however, proposed that definite and indefinite descriptions should be assimilated to the case of quantificational noun phrases (following their syntactic similarity to such noun phrases). On the Russellian view, definite and indefinite descriptions are to be contextually defined using the classical quantifiers:

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<sup>4</sup>One difficult question, of course, is determining which natural language formations should be taken as quantificational. This question is taken up briefly and dogmatically later in this section, and rears its head as a more substantial challenge from time to time later throughout this work.

(Def. 4)  $\Sigma[(\text{the } \chi)\varphi(\chi)] =_{\text{def}} \Sigma[(\exists\chi_1)(\forall\chi_2)(\varphi(\chi_2) \leftrightarrow \chi_1=\chi_2)]$

(Def. 5)  $\Sigma[(\text{a } \chi)\varphi(\chi)] =_{\text{def}} \Sigma[(\exists\chi)\varphi(\chi)]$

The Russellian view has met with considerable resistance from (e.g.) [Strawson 1950] and [Donnellan 1966]. It has been defended with equal vigor by (e.g.) [Grice 1968], [Kripke 1977], and [Neale 1990a].

### **§1.2.3.2 Restricted Quantifiers**

Quantifiers in natural languages are restricted, rather than unrestricted. Unlike the quantifiers in formal languages, which are intended to range over all that exists (as realized, formally, by the domain of quantification), natural language quantifiers range over only a particular type of object. To capture this notion of restricted quantification, we take quantifiers not to be the bare cardinality indicators of classical logic (or its Mostowskian extensions), but as complexes of such cardinality indicators along with an open formula indicating what the quantifier is to range over.

Thus, for example, a sentence like:

(7) All men are mortal

is not taken, as in classical logic, to contain under analysis a material conditional. Instead, it is assigned the logical form:

(7') [all  $x$ : man( $x$ )] mortal( $x$ )<sup>5</sup>

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<sup>5</sup>The restricted quantification notation I use here is by some authors (e.g., [Lindstrom 1966], [Evans 1980], and [Sher 1990]) replaced by a notation of binary quantification. Under binary quantification, quantifiers — here unrestricted, as in the classical paradigm — bind two formulae simultaneously. The truth conditions are then written such that, effectively, the first formula acts to provide a quantificational restriction on the second formula. Binary and restricted quantification are, for this reason, mere notational variants of each other. Binary quantification does, however, naturally generalize to a notion of  $n$ -ary quantification, in which a single quantifier simultaneously binds  $n$  formulae. The expressive resources of  $n$ -ary quantification will then outstrip those of binary, and hence also of restricted, quantification.

Here the open formula 'man(x)' tells us that the quantifier is to range over men, and the determiner 'all' (corresponding to the classical quantifier ' $\forall$ ') tells us that *all* men must have the relevant property in order for (7') to be true.

A well-known consequence of the use of restricted quantification (going back at least to [Rescher 1962]) is that it allows us to capture certain claims which can at best quite awkwardly be accommodated in the unrestricted classical format. Thus:

(8) Most men are mortal.

becomes:

(8') [most x: man(x)] mortal(x)

Since no truth-functional connective ' $\oplus$ ' stands to the 'most' quantifier as ' $\rightarrow$ ' does to ' $\forall$ ' or ' $\wedge$ ' does to ' $\exists$ ', we cannot formalize (8) in unrestricted notation as:

(8'') (most x)(man(x)  $\oplus$  mortal(x))

Instead, we must fall back on the cumbersome (and possibly ontologically promiscuous):

(8''')  $(\exists x)(\exists y)((\forall z)(\text{man}(z) \leftrightarrow z \in x) \wedge (\forall z)(z \in y \rightarrow \text{man}(z) \wedge \text{mortal}(z)) \wedge (\exists w)((\forall z)(z \in w \leftrightarrow z \in x \wedge z \notin y) \wedge |y| > |w|))$

#### §1.2.4 Second- and Higher-Order Quantifiers

The quantifiers that we have considered thus far are *first-order*, in the sense that they range over objects and bind variables in term positions.

There is thus a structural analogy between:

(9) Socrates is mortal

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It is unclear whether natural languages exploit or require this greater expressive capacity, although 'more X than Y' constructions, as in:

(FN 1) More philosophers than linguists read Davidson.  
 may be well-suited for analysis using ternary quantifiers. I defer until a later occasion further discussion of n-ary quantification.

and:

(10)  $(\forall x) \text{mortal}(x)$

The quantifier in (10) is associated with the same grammatical position as is occupied by 'Socrates' in (9), and ranges over the same type of thing as 'Socrates' names. But we can also define quantifiers which act on other grammatical positions, and which range over other types of objects. Thus instead of generalizing (9) from a claim about Socrates to a claim about all men, we could have generalized it from a claim about mortality to a claim about all properties. Formally, we could then introduce:

(11)  $(\forall X) X(s)$ <sup>6</sup>

(where 's' names Socrates). (11) is intended to stand to (9) with respect to the predicate position in the same way in which (10) stands to (9) with respect to the subject position. It is not at all clear that natural language makes use of second- and higher-order quantification, so finding a natural interpretation for (11) is difficult. We might do best to rest content with:

(11') Socrates has every property.

and ignore for now the apparent shift here back to first-order quantification.<sup>7</sup>

Other grammatical positions can also have quantifier types associated with them. We can thus introduce sentential quantifiers, which will allow us to generalize the entire claim (9) to create:

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<sup>6</sup>I will follow throughout a convention that upper-case letters are to be second-order variables. For the considerably rarer cases of sentential, adverbial, or third-order variables, I will use Greek, Hebrew, and Arabic letters respectively.

<sup>7</sup>In §3.2.2.2.2.1 below I discuss in greater detail the relation between natural language and higher-order quantification.

(12)  $(\forall \sigma) \sigma^8$

Or we could create adverbial quantifiers, which allow us to generalize from:

(13) Socrates ran quickly.

to obtain:

(14)  $(\forall x) x(R(s))^9,10$

We could even create quantifiers of quantifiers (which, for reasons the next section will make clear, we will call third-order quantifiers), and thus generalize from:

(10)  $(\forall x) \text{mortal}(x)$

to:

(15)  $(\forall y)(y x) \text{mortal}(x)^{11}$

This vast array of higher-order quantifiers gives rise to serious interpretative difficulties, some of which we will return to later. The most pressing, to my mind, is what we are to take these quantifiers as ranging over (or even whether the very notion of a quantifier ranging over things is applicable to these higher-order quantifiers).<sup>12</sup>

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<sup>8</sup>If (11) is difficult to express naturally as a sentence of English, (12) is almost impossible. The best available translation would seem to be

(12') Everything is true.

but (12') suffers from an unwarranted semantic ascent.

<sup>9</sup>I here assume, merely for ease of exposition, that adverbs are some variety of sentential operator. Should the proposals of [Davidson 1967b] prove correct, adverbs would be a variety of predicate, and adverbial quantification would collapse to a species of second-order quantification.

<sup>10</sup>Where (14) is to be understood as something like:

(14') Socrates ran in some manner.

My earlier caveats about the difficulty of providing natural language interpretations of higher-order quantificational claims apply here as well.

<sup>11</sup>Interpreted as, say:

(15') Some number of things are mortal.

<sup>12</sup>Where 'thing' is to be taken quite broadly. Note that if we are forced to give up the idea (or metaphor) of the quantifier as ranging over entities in making sense of higher-order quantification, the question of

### §1.2.5 Branching Quantifiers

All the quantifiers discussed so far have had the feature of linearity. This feature is exhibited in the fact that the order in which quantifiers appear is important. Thus we can distinguish between:

$$(16) (\forall x)(\exists y) Lxy$$

$$(17) (\exists y)(\forall x) Lxy$$

Taking the domain of quantification to be people and interpreting 'L' as 'likes', (17) asserts that there is (at least) one particular person liked by everyone, while (16) makes the weaker claim that, given any person, there is someone liked by that person.

Starting with [Henkin 1959], and later in [Hintikka 1973], it was suggested that there was, or ought to be, an understanding of quantifiers on which they need not be linearly ordered. Instead, the idea was that a block of quantifiers at the head of a formula (a quantifier prefix) would have some partial ordering on it. In place of the classical:

$$(18) (\forall x)(\exists y)(\forall z)(\exists w)(\exists u) Fxyzwu$$

we would have such syntactic structures as:

$$(19) \begin{array}{c} (\forall x) \backslash \\ \quad \quad \backslash \\ \quad \quad \quad (\exists w) \backslash \\ \quad \quad \quad \quad \quad / \quad \quad \backslash \\ (\exists y) / \quad \quad \quad Fxyzwu \\ \quad \quad \quad \quad \quad \quad \quad / \\ (\forall z) \text{ --- } (\exists u) / \end{array}$$

---

what makes higher-order quantification a species of *quantification* will become even more pressing.

The difficulties, of course, lie in (a) understanding what meaning is to be attached to branching structures, and (b) determining what concept of quantification could allow the assignment of such meanings. Despite these difficulties, interest in branching structures has been spurred on by the claim of some authors (primarily [Hintikka 1973], but also [Barwise 1979] and [Sher 1990]) that some sentences in natural language admit or require branched interpretations. Thus each of the following has been held to allow a branched reading:

(20) Some relative of every townsman and some relative of every villager hate each other.

(21) Most of the dots and most of the stars are joined by lines.

(22) The more powerful a country, the richer one of its officials.

(23) Three elephants were chased by a dozen hunters.

Consider (21), and assume we have three dots (D1, D2, and D3), and three stars (S1, S2, and S3). The claim then is that there is a reading of (21) on which neither of the following arrangements of lines suffice to make (21) true:

(LINE 1): <D1, S1>, <D1, S2>, <D2, S2>, <D2, S3>

(LINE 2): <D1, S1>, <D2, S1>, <D2, S2>, <D3, S2>

But such a reading is unavailable using linearly ordered quantifier prefixes, so if it is genuine, it seems to call for a branched understanding of the quantifiers.

#### **§1.2.6 Cumulative Quantifiers**

Bearing some similarities to branching quantifiers, but generally in the literature held distinct from them, are cumulative quantifiers.

Cumulative quantifiers are taken to be necessary in order to capture

certain readings of natural language sentences involving plural noun phrases. Thus consider:

(24) Three professors graded five exams.

The standard quantificational readings of this sentence are distributive, in that they require either (a) three selections of professors, and for each selected professor, five selections of exams graded by that professor, or (b) five selections of exams, and for each selected exam, three selections of professors who graded that exam. Thus the standard readings allow for either 15 exams or 15 professors. There seems to be another reading of (24) on which only three professors and only five exams are involved; the challenge of cumulative quantification lies in accounting for this reading. A number of approaches to cumulative quantification have been proposed; [Scha 1984] and [Davies 1982] provide good overviews of the state of the field.

### **§1.2.7 Plural Quantifiers**

The understandings of quantification we have been considering thus far have all been singular. By this we mean that when we assert sentences like

(25) Some men are mortal.

(26) All men are mortal.

we are making assertions which put restrictions on how things are with single, particular men (that each of them is mortal). [Boolos 1984] has suggested that we need also to allow a plural understanding of quantification. On this plural understanding, (25) would not range over individual (single) men and assert that at least one is mortal, but rather would range over men (plural) and assert that some are mortal.



The important difference between the singular and the plural conceptions of quantification comes out when we consider claims which discuss relations among the plurality quantified over. Boolos gives as an example the Geach-Kaplan sentence:

(27) Some critics only admire each other.

In order to make sense of this sentence, we must take the quantifier as ranging not over individual men, but over men, plural. These plural men then admire each other.

Plural quantification adds considerable power to our languages. Sentences like (27) cannot be expressed using the resources of standard singular quantified logic. Moreover, plural quantification can be used to express certain truths about mathematics which might otherwise be taken to commit us to the existence of and quantification over sets (which existence can be problematic, if the requisite sets are too large). Take for example:

(28) There are some sets that are such that no one of them is a member of itself and also such that every set that is not a member of itself is one of them.

To interpret (28) in singular quantification, of course, requires us to quantify (singularly) over a collection of all and only those sets which are not members of themselves. Famously, to avoid paradox we must then assume that that collection is not itself a set, and thus introduce proper classes into our ontology. Similar considerations force another level of collections above proper classes, and so on. But if the quantification is taken plurally, then we require no collection of the non-self-containing sets — we merely require those sets (plural) themselves.

### §1.2.8 Polyadic Quantifiers

The quantifiers we have considered thus far have in common that they are *monadic* — they all bind exactly one variable. Recent work by [Higginbotham & May 1981] and [Van Bentham 1989], however, suggests that it is possible — and perhaps necessary — to introduce *polyadic* quantifiers which bind multiple variables.<sup>13</sup> Consider a sentence such as:

(29) Every girl read a different book.

The claim here is that a classical analysis, which treats (29) as containing two monadic quantifiers (one ranging over girls, the other over books) is inadequate, because we need to specify that, given any girl, the value picked out by the book quantifier for that girl depends on the value that same quantifier picked out relative to other girls. Such specification is supposedly impossible using two distinct monadic quantifiers, in adherence to the surface form of the sentence.<sup>14</sup> The

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<sup>13</sup>N-adic quantifiers, binding  $n$  variables, should not be confused with  $n$ -ary quantifiers discussed earlier, which bind  $n$  formulae but only on one variable. One can clearly also combine the two innovations to develop  $n$ -adic  $m$ -ary quantifiers, which bind  $m$  formulae across  $n$  variables.

<sup>14</sup>(29) can, of course, be expressed straightforwardly using the traditional resources of monadic quantification, as in

$$(29'') (\forall x)(Gx \rightarrow (\exists y)(By \wedge Rxy) \ \& \ (\forall z)(\forall w)(Gz \wedge Gw \wedge z \neq w \rightarrow \neg(\exists y)(By \wedge Rzy \wedge Rwy))$$

or perhaps

$$(29''') (\forall x)(Gx \rightarrow (\exists y)(By \wedge (\forall z)(Gz \wedge Rxy \leftrightarrow z=x))$$

(it is unclear to me whether the truth conditions of (29) demand the analysis (29'') or (29'''); but both readings can be accommodated in monadic quantification). These analyses, however, clearly depart considerably from the apparent syntactic structure of (29); to the extent that we want a general semantic theory in which semantic form closely mirrors syntactic form this departure would seem to favor the use of polyadic quantification. However, I am rather inclined to read (29) as elliptical, as evidenced by the coherence of the following dialogue fragment:

(FN 2) Voice 1: Every girl read a different book.  
Voice 2: Different from what?

The original (29) is elliptical, then, for a longer construction such as

proposal, then, is that we read (29) as containing only one polyadic quantifier:

(29') [every-a-different  $x,y$ ]( $Gx \wedge By \rightarrow Rxy$ )

(with the obvious predicate interpretation). The axiom in the truth theory governing the every-a-different quantifier will then have the following form:

(AX9) A sequence  $\sigma$  satisfies [every-a-different  $\chi,\xi$ ]  $\varphi(\chi,\psi)$  iff there is a set  $\Sigma$  of sequences satisfying  $\varphi(\chi,\psi)$  such that:  
(a) for every sequence  $\sigma'$  differing from  $\sigma$  at most in the  $\chi$  position, there is some sequence  $\sigma''$  differing from  $\sigma'$  at most in the  $\xi$  position in  $\Sigma$ , and (b) for any sequences  $\sigma', \tau'$  in  $\Sigma$  differing in the  $\chi$  position, there are sequences  $\sigma''$  and  $\tau''$  in  $\Sigma$  differing from  $\sigma'$  and  $\tau'$  (respectively) at most in the  $\xi$  position such that  $\sigma''$  and  $\tau''$  differ in the  $\xi$  position.

The use of polyadic quantifiers has been proposed to deal with a number of tricky natural language constructions, including Bach-Peters sentences (such as (30)) and multiple wh-questions (such as (31)):

(30) A boy who loved her left the girl who despised him.

(31) Who read which books?

---

(FN 3) Every girl read a book different from the books the other girls read.

the analysis of which does closely mirror (29''). The indeterminacy of the elided material might also help explain the ambivalence of (29) between readings (29'') and (29'''). Note that (29) also supports (albeit awkwardly) a deictic reading, in which we demonstrate, say, a copy of *The Wizard of Earthsea*, and declare "Every girl read a *different* book."

None of these considerations about the express need for polyadic quantification to account (whether gracelessly or elegantly) for natural language phenomena tell, of course, on the larger question of whether such quantification is conceptually coherent in the first place.

### §1.2.9 Dynamic Quantifiers

The diverse accounts of quantification we have so far canvassed all agree in taking the effect of a quantifier — its ability to bind variables — to be limited by syntactic features of scope. The dynamic tradition in logic, however, has (in [Groenendijk & Stokhof 1991]) given rise to an account of quantification in which (some) quantifiers have an open-ended ability to bind variables.

Groenendijk and Stokhof's system of dynamic predicate logic is intended to capture the natural language phenomenon of cross-clausal anaphora. Thus in sentences like

(32) A man walks in the park. He whistles.

we would like to take the pronoun 'he' as bound by the quantifier 'a man'. However, that pronoun lies outside the scope of the quantifier, so traditional accounts of quantification prohibit us from doing so. On the dynamic understanding of the quantifier, however, the existential quantifier serves to place a constraint on what sorts of semantic interpretations are acceptable: it requires that any semantic structure claiming to be compatible with the truth of the sentence contain some object playing the role demanded by the existential quantified formula.<sup>15</sup> Thus in (32), we know from the first sentence that any

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<sup>15</sup>Dynamic logic is thus born out of the tradition of discourse representation theory (see, e.g., [Kamp 1981]). Discourse representation theory associates sentences with discourse representation structures and uses these structures to determine the truth value of the associated sentences. In this theory, existentially quantified claims in the guise of indefinite descriptions introduce discourse referents into the discourse representation structure, and further information about these discourse referents is then accumulated through successive discourse representation structures. (32) above, for example, is associated with the following pair of discourse representation structures:

DR<sub>1</sub>(32):     u           v  
                  ·           ·  
                  A man walks in the park  
                  man(u)

acceptable interpretation must contain some man who walks in the park. The second sentence then places a further constraint on those interpretation, singling out those in which that distinguished man whistles. Further constraints can be dynamically imposed as required by the flow of the conversation.

Dynamic quantification represents a powerful extension to the concept of quantification. Unfortunately, as that logic has to date been developed, it is a somewhat ad hoc extension. Groenendijk and Stokhof claim, without explanation, that the universal quantifier, unlike the existential quantifier, does not permit a dynamic interpretation and thus cannot bind variables outside its syntactic scope.<sup>16</sup> Groenendijk and Stokhof do not discuss generalized and other non-classical quantificational forms, but they certainly provide no general understanding of what enables dynamic interpretation which would allow us to extend their proposals to the broader field of quantifiers in a principled and unified manner.

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DR<sub>2</sub>(32):      park(v)  
                   u walks in v  
                   u        v  
                   ·            ·  
                   man(u)  
                   park(v)  
                   u walks in v  
                   u whistles

See §3.2.1.3.2.2.2 below for further discussion of discourse representation theory and its lessons for a general theory of quantification.

<sup>16</sup>Groenendijk and Stokhof, however, are incorrect in claiming that universal quantifiers do not allow cross-clausal binding. It is certainly true that constructions like

(FN 4) All men walk in the mark. He whistles.

are unacceptable, but this is simply because of syntactic features of number agreement — the plural 'all men' requires a plural pronoun. Other universal constructions, however, are syntactically singular and do support the cross-clausal constructions:

(FN 5) Each man walked in the mark. He whistled as he did.

Even syntactically plural constructions like (FN 4) do allow cross-clausal binding provided agreement constraints are observed:

(FN 4') All men walk in the park. They whistle as they do.

### §1.2.10 Substitutional Quantifiers

The accounts of quantification considered thus far all agree in taking quantifiers to range over entities, broadly speaking. It is presumably this characteristic of quantification which led Quine, in [Quine 1948], to identify quantification as the mark of ontological commitment.

However, one can also give quantifiers a *substitutional* interpretation (an interpretation which, incidentally, they seem plausibly to receive in Frege's *Begriffsschrift*).

Substitutional quantifiers range not over objects, but over bits of language. These bits of language, moreover, serve not as the referent of the quantified variable (-relative-to-a-sequence), but as *replacements* for that variable in understanding the sentence. Thus if we have the substitutionally quantified

$$(33) (\Sigma x) Fx^{17}$$

the resulting claim is not to be understood as saying that there is some object which is F, but as saying that there is some name such that the result of replacing 'x' with that name in 'Fx' is a true claim. What we can substitutionally quantify over, then, is, unlike what we can objectually quantify over, limited by the expressive resources of the object language.

Substitutional quantification thus differs from objectual quantification in two important aspects. First, since substitutional quantification makes no appeal to objects in its truth conditions, it may appear to carry with it an ontological neutrality not found in

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<sup>17</sup>I here follow [Kripke 1975] in using ' $\Sigma$ ' for the substitutionally interpreted existential quantifier and ' $\Pi$ ' for the substitutionally interpreted universal quantifier.

objectual quantification.<sup>18</sup> Second, since substitutional quantification allows direct replacement of linguistic material, it is easy to make sense of a wide range of non-first-order substitutional quantifiers. Thus we can take the substitution class to be sentences, and immediately make sense of:

(34)  $(\Pi x) x$

Or, to adapt an example from [Kripke 1976], we can take the substitution class to be punctuation marks, and thus make sense of:

(35)  $(\Sigma x) (\text{John is tall } x)$ <sup>19</sup>

Several authors have expressed doubts as to whether substitutional quantification is a coherent form of quantification. Troubled by its easy dismissal of ontological consequences, along with its ability to create apparently trivial theories of linguistic understanding, [Wallace

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<sup>18</sup>The supposed ontological neutrality of substitutional quantification is a difficult matter. Although the semantics for the quantifiers themselves make no reference to objects in the world, one might easily suppose that the appeal to substitution instances formed using names, *combined with reference clauses for those names*, would give rise to ontological commitment. However, given a finite vocabulary for the object language, one can simply correlate truth values with all atomic (non-quantificational) sentences, and use these truth values to determine the truth values of substitution instances of quantified claims without passing through reference axioms. In this way, truth could be extended to the entire (quantified) language without appeal to world-word relations. On the other hand, no language with a finite vocabulary will be able to provide the correct truth conditions for all quantified claims (on the assumption that there are actually infinitely many objects) since we will be unable to cover substitutionally all of the objects which need to be covered.

<sup>19</sup>Expressing in natural language the claims made by (34) and (35) — especially (35) — is no easy task. Perhaps the best we can do is to retreat to a metalinguistic formation and employ:

(34') Every sentence is true.

(35') Some punctuation mark is such that the result of concatenating "John is tall" with that punctuation mark is true.

However, this semantic ascent is not meant to be mirrored in the meanings of the object language substitutionally quantified claims. The difficulty of determining what is said by the likes of (34) and (35) provides the heart of [Van Inwagen 1981]'s objection to substitutional quantification.

1969], [Tharp 1970], and [Van Inwagen 1981] all inveigh against substitutional quantification. The coherence of such quantification has been defended with at least equal vigor by [Kripke 1976]. Clearly there is room here for a general account of what quantification is to adjudicate this dispute.

#### **§1.2.11 Adverbs of Quantification**

[Lewis 1975] suggest that certain adverbs in English and other natural languages should be taken as forms of quantifiers. Thus, for example, in sentences like

(36) Tigers usually eat animals.

(37) I occasionally read magazines.

the adverbs 'usually' and 'occasionally' are to be understood as inducing a quantification over instances — instances of tiger-eatings in (36), and instances of readings in (37). The adverbial quantifiers then place constraints on the cardinality of such instances required for the sentence to come out true.

Interpreting adverbs as quantifiers in this way open up new difficulties in determining what in natural language is to be taken as a quantifier and what is not. No longer do we need the explicit determiner-restrictor formation we observed earlier in natural language quantified noun phrases. Nor do we need any observable variables bound by such quantifiers.

#### **§1.2.12 Intensional Operators as Quantifiers**

The remarkable success of the possible worlds semantics given by [Kripke 1958, 1963] in aiding our comprehension of modal and other intensional logics has led some — notably [Lewis 1968] — to take seriously the



metalinguistic quantification over worlds employed in the semantic analysis of ' $\Box$ ', ' $\Diamond$ ', and other intensional operators. Lewis's position is that the modal operators really are just a type of quantifier — here quantifying over some particularly unusual variety of object, entire worlds.

Treating intensional operators quantificationally does help account for certain inferential similarities of such terms to quantificational inference patterns. For example, the invalidity of the move from

$$(38) \Diamond p \wedge \Diamond q$$

to

$$(39) \Diamond(p \wedge q)$$

can be explained, or at least illuminated via analogy, by consideration of the similarly invalid quantificational move from

$$(40) (\exists x)Fx \wedge (\exists x)Gx$$

to

$$(41) (\exists x)(Fx \wedge Gx)$$

Similarly, our ability to take instances of modal claims, as in the move from

$$(42) \text{Necessarily, } 2+2=4.$$

to

$$(43) \text{Had the universe contained one fewer electron, } 2+2=4.$$

bears a clear resemblance to our ability to take instances of quantificational claims.

Nevertheless, like the proposal that certain adverbs are really quantifiers, the proposal that certain intensional sentential operators are really quantifiers clearly stretches our concept of quantification.

Again, at least in the surface appearance of things, there are no variables being bound by these quantifiers, so their relation to other things we want to call quantifiers is attenuated at best.

### **§1.2.13 Evidence Base for a Theory of Quantification**

The above examples of the ways in which the notion of a quantifier has been stretched in recent formal and linguistic work suffice, I hope, to show that none of the properties we standardly associate with classical quantification persevere robustly or universally enough to ground the type of unificatory and explanatory account we are seeking. These examples, moreover, point to a serious problem with even *seeking* such an account. I take it that it is not at all obvious that every formal construction discussed above ought to count as a quantifier. Even if all of the above cases seem unproblematically quantificational, it is surely just a matter of time before ambitious and technically gifted linguistics construct some new 'quantificational' language which will strike even the most jaded of sensibilities as unpalatable qua quantifiers. But how are we to try to extract a core of features essential to quantification if we cannot even agree on what counts as a quantifier?

In order for the project we have set for ourselves to succeed, the answers to three separate questions must evolve simultaneously. The first of these questions is what types of phenomena count as quantificational. Second, we will attempt to determine what the nature of quantification is. Third, we will ask why the notion of quantification has occupied such a prominent place over the last century in the philosophy of language and related fields. The hope, then, is

that a bootstrapping process will develop in which we start with a rough-and-ready idea of what counts as quantification, attempt to extract core features from that idea, allow those features to be further modified in order to account for the role we want quantification to play in our larger projects, return to see if our refined notion of quantification allows us to settle more questionable cases of putative quantifiers, and so on.

The first point to settle, then, is what we will take our rough-and-ready starting point to be. The Fregean paradigm, born out of the nineteenth century rigorization of analysis, took the universal quantifier to be the paradigm case of quantification. Subsequent developments in *classical* quantificational logic have retained the idea that the universal quantifier, along with its Boolean soulmate the existential quantifier, exhaust the concept of quantification. I take it, however, that in light of recent formal work, such a narrow paradigm is no longer acceptable.

Instead, I would like, initially, to attempt to construe quantification as broadly as possible. The general heuristic we will follow is to take the phenomena of natural language as paradigms of the logical phenomena we want to study. In natural language, there is a class of expressions called noun phrases. Members of this class are used to talk about objects in the world. As we will come to see throughout our discussion, noun phrases are quite a complex class, and much philosophical ink has been spilled in attempting to understand how various types of noun phrases function. We will be interested at various points in almost every type of noun phrase, but for now I want to advance the suggestion that the best starting point for a theory of

quantification is the field of quantified noun phrases. Furthermore, we will initially assume that quantified noun phrases are just those which (in contrast to proper names, demonstratives, and others) have a certain distinctive syntactic structure — a concatenation of a determiner and a common noun.<sup>20</sup> This is not an uncontentious assumption — as noted above, the cases of definite and indefinite descriptions, which I rule by my syntactic test to be quantificational, are taken by many to be referential. But we must start somewhere, and this strikes me as a starting point rich enough to produce significant results while conservative enough to have some plausibility. With luck, the internal coherence of the resulting account will itself lend backward-looking plausibility to the starting point.

### **§1.3 The Neo-Fregean Account of Quantification**

I have argued in the previous section that the logico-philosophical developments of the last century have presented us with a trifold puzzle regarding quantification. The need for some account of quantification has not, of course, escaped others working within these areas, and in recent years some degree of orthodoxy has developed on the nature of

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<sup>20</sup>Problematically, there are constructions in natural languages which *prima facie* are of the determiner-common noun syntactic form but which are not obviously indicators of quantity. Thus consider noun phrases such as:

- (FN 6) John's book
- (FN 7) other philosophers
- (FN 8) even ethicists

I will make only a few passing remarks on the place of such noun phrases in a complete account of quantification.

- Complex demonstratives, as in
- (FN 9) that man in the corner drinking wine

are an apparent counterexample to my methodological principle, since I take them, like all demonstratives, to be nonquantificational. I hold here that surface appearances are deceiving and that complex demonstratives are not of the determiner-common noun syntactic form; see §2.3.3.1 for detailed discussion of complex demonstratives.

quantification. In this section, then, I want to sketch what I will call the neo-Fregean approach to quantification, as exemplified in (e.g.) [Barwise & Cooper 1981], [Higginbotham & May 1981], [May 1989], [Van Bentham 1989], [Scha 1984], and [Sher 1991]. This neo-Fregean approach will make repeated appearance throughout this work, as I attempt to show that there are compelling reasons for preferring my own (soon-to-be-unveiled) account of quantification to it. In this section I want merely to lay out the neo-Fregean view and give some broad hints at its failings.

The neo-Fregean view is, in essence, a formal implementation of the Fregean idea that quantifiers are predicates of predicates. Frege's thought was that, in a sentence like

(26) All men are mortal.

one could see the universal quantifier as attributing a certain property to the (complex) property of *being mortal if a man*. The property to be attributed was that of universal instantiation — that property which a predicate has if it is true of everything. Similarly, the existentially quantified

(25) Some men are mortal.

would attribute the property of *being instantiated* to the property of *being mortal and a man*. The Fregean conception of the quantifier thus assimilates quantificational sentences to the reference predication paradigm, elevated one level in abstraction.

The neo-Fregean orthodoxy takes this assimilation and backs it with a formal implementation in set-theoretic terms. Assuming for convenience's sake that we are working in an extensional language (this assumption is dispensable), we take the semantic value of a predicate to

be an extension — that is, a set of objects of which the predicate is true. A quantifier is then also taken to have an extension, but in this case the elements of that extension are taken to be predicate extensions. The extension of a quantifier, then, is a subset of the power set of the universe of discourse.

Take, for example, the universal quantifier. The extension of the universal quantifier will contain exactly one element — the set which contains all members of the universe of discourse. The universal quantifier will then be true of (the extension of) a predicate just in case the extension of that predicate is an element of the extension of the quantifier — that is, just in case the extension of that predicate is identical to the universe of discourse.<sup>21</sup> The existential quantifier, on the other hand, is assigned as extension all those subsets of the universe of discourse which are non-empty:

$$\text{(Def. 6) Ext('}\exists\text{')} = \wp(\text{UD}) - \emptyset$$

The existential quantifier will thus be true of any predicate which has at least one object in its extension.

Once we formulate the semantics for the universal and existential quantifier in this way, it quickly becomes clear that we have a semantic framework ripe of generalization. Any subset of the power set of the universe of discourse can serve as an extension for a quantifier, not just the two subsets singled out above. Thus we can easily define additional quantifiers such as:

$$\text{(Def. 7) ALL-DOGS: Ext('ALL-DOGS')} = \{\{x \in \text{UD} \mid x \text{ is a dog}\}\}$$

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<sup>21</sup>Strictly speaking, quantifiers, under the neo-Fregean analysis, are predicates of extensions of predicates (in extensional languages). However, for simplicity I will occasionally refer to a quantifier being true of a predicate itself, rather than the extension of that predicate.

(Def. 8)  $\exists_3$ :  $\text{Ext}(' \exists_3 ') = \{X \subseteq UD \mid |X| \geq 3\}$

(Def. 9) MOST:  $\text{Ext}(' \text{MOST} ') = \{X \subseteq UD \mid |X| > |UD - X|\}$

(Def. 10) KRIPKE:  $\text{Ext}(' \text{KRIPKE} ') = \{X \subseteq UD \mid \text{Kripke} \in X\}$

Extensive projects setting out taxonomies of this vast new array of quantifiers also become possible (see (e.g.) [Sher 1991], [Van Bentham 1989], and [Keenan & Stavi 1989]). We can isolate various subgroups of these quantifiers which strike us as being of particular interest, such as those which are permutation invariant, in the following sense: Given any permutation  $p$  of the universe of discourse  $UD$  and any quantifier  $Q$ ,

(Def. 11)  $Q$  is permutation invariant iff  $\text{EXT}(Q) = p(\text{EXT}(Q))$

Permutation invariance has been taken by many to be characteristic of the logicity of a quantifier. It strikes me as a difficult and obscure issue what standards might be used to determine what would make a quantifier logical or non-logical, and the issue of logicity will play relatively little role in my subsequent discussion. The system I will set out in subsequent chapters, however, is entirely compatible with the idea that permutation invariance is the hallmark of logicity.

The neo-Fregean approach thus amounts to a reinterpretation of first-order logic into second-order logic in the guise of set theory. This move to a second-order analysis of apparently first-order quantification carries with it a powerful flexibility, allowing the neo-Fregean approach to gather a number of disparate quantificational phenomena under its theoretical roof. As an example of the unificatory power of the neo-Fregean approach, consider the semantics that [Barwise 1979] provides for branching quantification. Barwise takes a branching structure of the form:

$$(44) \quad \frac{(Q_1 x) \backslash \varphi(x, y)}{(Q_2 y) /}$$

to be true if and only if

$$(45) \quad (\exists X)(\exists Y)[(Q_1 x)(x \in X) \wedge (Q_2 y)(y \in Y) \wedge (\forall x)(\forall y)(x \in X \wedge y \in Y \rightarrow \varphi(x, y)]^{22}$$

Barwise thus employs the neo-Fregean move to a broadly set-theoretic understanding of quantifiers to allow himself to pick out (independently) two sets of the appropriate cardinality and check if they bear the right  $\varphi$  relation to one another. There is, of course, no guarantee that Barwise's definition gives the right truth conditions — [Hintikka 1973] and [Sher 1990] both disagree with some of Barwise's evaluations of branching structures. But whatever results we want to impose on branching structures, the neo-Fregean framework gives us the flexibility to do so. Broadly speaking, since the open formulae we want to quantify merely serve to specify (in an extensional context) certain sets, a semantic framework which allows us to construe quantifiers as arbitrary operations on sets gives us the maximum possible freedom for developing and implementing quantifiers.<sup>23</sup>

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<sup>22</sup>This truth clause for branching structures applies only when the quantifiers  $Q_1$  and  $Q_2$  are monotone increasing; Barwise has a separate truth definition for branching structures involving two monotone decreasing quantifiers and rejects outright structures which either mix monotone increasing and monotone decreasing quantifiers or which use nonmonotonic quantifiers. See §§3.3.1.2, 3.3.1.3.1, 3.3.2.2.3.2.2 below for definitions and discussion of quantifier monotonicity and for the connection between quantifier monotonicity and branching quantification.

<sup>23</sup>Despite this flexibility, the neo-Fregean framework may be ill-suited for interpreting plural quantifiers, for the reasons hinted at in §1.2.7.



### §1.3.1 Weaknesses of the Neo-Fregean Account

Despite its power and versatility, the neo-Fregean account has certain weaknesses as a general account of the nature of quantification. First, its very flexibility may come back to haunt it. Just about anything even putatively quantificational can be accommodated within the neo-Fregean framework. As a result, it begins to look unlikely that the category of the quantifier can serve the kinds of philosophical purposes I indicated earlier we want it to serve. For example, names are easily construed as quantifiers in the neo-Fregean framework. Given any name  $\alpha$ , we can define a quantifier  $Q_\alpha$  as follows:

$$\text{(Def. 12) } \text{EXT}(Q_\alpha) = \{X \subseteq \text{UD} \mid \text{ref}(\alpha) \in X\}$$

Thus any hope that an understanding of quantification will shed light on the distinction between reference and denotation or the distinction between singular and general thoughts and propositions is shot down by the neo-Fregean account.

Similarly, the ontological purposes to which philosophers have put quantification are endangered by neo-Fregeanism. By shifting all cases of quantification into set theory, the neo-Fregean theorist makes it impossible to determine which structures are genuinely first-order and thus independent of set-theoretic assumptions and ontology, and which rely on or incorporate such assumptions and ontology.

Take as an example questions about the ontological involvement of branching structures. [Barwise 1979], considering such questions, observes:

One of Hintikka's aims, in the paper Hintikka (1974), was to show that there are simple sentences of English which contain essential uses of branching quantification. If he is correct, it is a discovery with significant implications for linguistics, for the philosophy of language, and perhaps

even for mathematical logic. Philosophically, it would influence our views of the ontological commitment inherent in specific natural language constructions, since branching quantification is a way of hiding quantification over various kinds of abstract abstract [sic] objects (functions from individuals to individuals, sets of individuals, etc.).  
(47)

*Prima facie*, this move is too quick. Even if we isolate constructions in English which 'make essential use of branching quantification', we will not know what ontological commitments to associate with these constructions until we know whether there is a genuinely first-order notion of branching quantification with which to analyze them (in which case they commit us to no more than, e.g., villagers and their relatives), or whether we must construe branching structures as implicitly higher-order (in which case we commit ourselves also to, e.g., functions from villagers to relatives of villagers). The neo-Fregean account blurs this distinction, leaving us with no clear standards as to when a quantificational structure has the ontological innocence we expect of simple first-order objectual quantification. The consequent void is then filled by seemingly endless debates such as that of [Quine 1972], [Patton 1991], and [Hand 1993] over whether branching structures are genuinely first-order — debates without clearly defined success criteria.

In place of the neo-Fregean account, I will suggest, we want an account which is general and unificatory without being simply promiscuous. We want to draw together all the important cases of quantification, but we also want to be able to show that and *why* these are the important cases, and consequentially also show that certain formal systems simply are not acceptable as forms of quantification.

The other major weakness of the neo-Fregean account to which I want to draw attention is the difficulties it faces in meeting the explanatory goals we set earlier. On the neo-Fregean account, quantification is merely a species of predication. A sentence of the form

(26) All men are mortal.

that is, has the same basic form as a sentence of the form:

(46) Socrates is a man.

Both are reference-predication pairs — in the one hand manhood is being predicated of Socrates, and in the other case universality is being predicated of *if manhood then mortality*. Admittedly, we have ascended a step up the Russellian type-hierarchy in moving from (46) to (26), but the core logical notions remain the same. Given that structural isomorphism, it seems extremely difficult to see *why* quantifiers should be of such great interest to philosophers and philosophical logicians. If quantifiers are just a type of predicate, what is to explain the vastly greater interest of these predicates than that of more plebeian predicates like 'is a chair' or 'is mortal'? All that is distinctively quantificational in quantification is lost under the neo-Fregean analysis.

I want to focus on one particular kind of such loss. It will perhaps not be immediately obvious that the loss I single out is of crucial importance, but the thesis I want to pursue for the rest of this work is that this deficiency of the neo-Fregean account is central to its failure as an account of quantification, and that by correcting that failure, we put ourselves on the road to an adequate account.

This crucial loss, then, is the loss of the variable. Prima facie, one distinctive characteristic of quantifiers is that they are variable binding operators. But on the neo-Fregean account, the variable plays no significant role. The relationship of quantification is a relationship which holds between a quantifier and a predicate. The only role of the variable is to help determine which predicate the quantifier is predicating something of. That is, variables help us distinguish

$$(47) (\forall x)Fxy$$

in which the property of bearing  $F$  to  $y$  is said to have the property of being universal, from

$$(48) (\forall x)Fxx$$

in which the property of being a self- $F$ -bearer is said to have the property of universality. My proposal, then, is that we seek, contra the neo-Fregean account, a substantive understanding of the role variables play in the process of quantification.

#### **§1.4 Three Inadequate Accounts of Quantification**

Philosophers and logicians rarely take much care in their remarks on the role of variables in quantified logic. I want to canvass some of the rather casual remarks which are made in order to extract sketches of three views on variables. All three of these views, we will see, are severely deficient given the goals we set for ourselves earlier.

##### **§1.4.1 Variables as Slot Machines**

I want to begin with a view which readily falls out of our informal talk about variables. It is also a view which has deep historical roots in discussions of variables — consider Cauchy's explanation of the role of the variable:

One calls a quantity which one considers as having to successively assume many values different from one another a variable. [Cauchy 1821]

Cauchy's idea here seems to be that the variable is much like a name, but a name whose referent changes, so as to take on many different values (presumably all those values in the domain of quantification). The variable is a semantic slot machine, continually spinning through values.

The slot machine view of variables infects the way we tend to talk about variables. It is common, especially when teaching students about quantified logic, to talk about quantifiers and variables ranging over objects, and to discuss what happens when a particular variable refers to a particular object. Moreover, the view is encoded in the very name 'variable', which clearly implies some sort of variation associated with the variable.

The slot machine view may sit well with colloquial usage, but it's hard to make it into a satisfying philosophical account. Clearly the rhetoric of explicit change in the variable can only be cashed out if we can explain change as change over something — over time, over distance, etc. The language of the slot machine view, as seen for example in Cauchy's comments, most readily invoke the idea of change over time. But obviously such a view, whatever its metaphorical and rhetorical utility, is philosophically bankrupt. The variable is not so literally a slot machine, rapidly spinning through its possible referents. Even if such a semantic device were possible, we would be left with unanswerable questions both about the details of its function (just how frequently would the variable change referents, and in what order would it progress through the domain of quantification?) and about the utility of this

variability in securing the role of the variable in quantification (how will change over time in the variable help secure the timeless truth or falsity of a universally quantified claim?).

I think the best that can be done in the way of making literal this metaphor of change is to follow the lead of [Tarski 1933] and appeal to variation with respect to sequences. We would then say, for example, that the variable 'x1' referred to Paris with respect to the sequence:

<Paris, London, Berlin, Paris, ...>

and to Moscow with respect to the sequence:

<Moscow, Moscow, London, San Francisco, Albany, ...>

Variation with respect to sequence is, I suppose, a perfectly respectable notion when we are considering formal languages (although I suspect that the concomitant questions about what counts as an appropriate sequence will continue to leave us adrift when we pursue our unificatory purposes). But when we come to the quantificational devices of natural language, talk of variation with respect to a sequence is much less satisfying. There is no clear sense in which any stage of language production or comprehension requires or accommodates consideration or exploitation of multiple sequences by any agent involved in the process. Sequences just don't seem to be among the things in the air when we use natural language, and I find it hard to see how talk about variables varying with respect to sequences could help us understand how those variables function in natural language.<sup>24</sup>

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<sup>24</sup>There is, as we shall see in §2.3.3.1, some formal resemblance between the notions of a sequence and of a context. I will argue in that section that there is no useful notion of the conversational context which allows specification of values to variables.

#### §1.4.2 Variables as Blurry Names

Perhaps feeling the inadequacy of the explicit variation of the slot machine view, many philosophers try to construe variables as stable but somehow blurry terms. Russell, for example, clearly rejects the slot machine view and promotes this alternative:

A variable is a symbol which is to have one of a certain set of values, without it being decided which one. It does not have first one value of a set and then another; it has at all times some value of the set, where, so long as we do not replace the variable by a constant, the 'some' remains unspecified [Russell 1908]

Both the slot machine and the blurry name views take names to be the starting point in understanding variables.<sup>25</sup> The slot machine tries to add something to the semantics of names to get variables — adding a peculiar element of change. The blurry name view, on the other hand, subtracts from names to get variables. On this name, one takes names, and then somehow removes the element of *definiteness of reference*, and thus obtains variables.

Russell is joined by other logicians of not inconsiderable technical and philosophical perspicacity in endorsing the blurry name view. Thus Carnap, in this vein, says:

We divide all the signs of our symbolic language into two classes, the constants and variables. Every constant has a fixed specific meaning. Variables, on the other hand, serve to refer to unspecified objects, properties, etc. [Carnap 1954, 16]

Quine, in one of his moods, also says that "the 'x', 'y', etc., of algebra behave as names of unspecified numbers" [Quine 1945].<sup>26</sup>

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<sup>25</sup>I will argue later (see §2.3.2) that this assumption exactly reverses the appropriate order of explanation.

<sup>26</sup>For Quine's other moods, see §§1.4.3, 1.5.1 below.

The blurry name view, however, is if anything even less satisfying than the slot machine view. The most pressing difficulty, of course, lies in saying what it means for a term to refer without referring to anything definite. Those who endorse the blurry name view tend to do so rather casually, and certainly none give any approximation of an explanation of indefinite reference.<sup>27</sup> We also need to understand the indefiniteness of reference in some way which is compatible with the requirement that in, say,

$$(49) Fx \wedge Gx$$

the two occurrences of 'x' refer, however vaguely, to one and the same object. Finally, and perhaps most seriously, we need to be able to understand how this indefiniteness of reference results, when combined with the binding of a variable by a quantifier, in quite definite truth conditions. Pending answers to these questions, the blurry name view is not viable.

### §1.4.3 Variables as Marks of Generality

Another tradition in the understanding of variables grows out of the mathematical practice of using variable-containing assertions as ways of making claims about the behaviour of all of a class of entities.

Consider in this vein Frege's example:

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<sup>27</sup>One might turn to supervaluation theory (see, e.g., [Van Fraassen 1966]) to account for the apparent contradiction between the following two theses of the blurry name view:

(TH1) Given a variable  $\chi$ , there is something that  $\chi$  refers to.

(TH2) Given any object  $\alpha$ ,  $\chi$  does not refer to  $\alpha$ .

I am, however, skeptical that any theory thus developed could account for the quantificational behaviour of variables (or make philosophical sense of the notion of indefinite reference). Certainly no advocate of the blurry name view has tried to develop the view along these lines. One might also, following the lead of [Fine 1985], try to develop the notion of an indefinite, or arbitrary, object, and have variables definitely refer to indefinite objects.



$$(5) (a + b)c = ab + bc$$

(50) is intended to employ variables to indicate that all numbers ( of a certain sort) obey the distributive law. In the *Begriffsschrift*, Frege formalizes this mathematical practice. Thus he says there of variables:

The signs customarily employed in the general theory of magnitude are of two kinds. The first consists of letters, of which each represents either a number left indeterminate or a function left indeterminate. This indeterminacy makes it possible to use letters to express the universal validity of propositions, as in  $(a + b)c = ab + bc$ . [Frege 1879, 10]

In his formal apparatus, Frege uses variables in conjunction with a correlated sentential operator to indicate that the assertion is to be read universally in the relevant argument position. Quine at some points acknowledges, if not necessarily endorses, this view of variables:

Indeed, logicians and mathematicians nowadays use the word 'variable' without regard to its etymological metaphor; they apply the word merely to the essentially pronominal letters 'x', 'y', etc., such as are used in making general statements and existence statements about numbers. [Quine 1960, 227]

The 'mark of generality' view of variables is less philosophically troubled than the two views previously considered. Were quantification restricted, as it is for Frege, to the universal quantifier (or even the universal and existential quantifiers), it might prove a perfectly adequate explanation of the nature of variables. However, once we take into account the many and varied extensions of quantification discussed in §1.2 above, it becomes clear that the 'mark of generality' view yields only the most ad hoc and superficial of explanations of the role of variables. To account for generalized quantifiers, for example, we will have to add to our marks of generality further marks of manyness, marks of fewness, marks of infiniteness, and so on. Restricted quantifiers will require marks of general humanity, marks of

instantiated dogness, and so on. Plural quantifiers will require an obscure distinction between singular and plural (or perhaps distributive and collective) marks of generality. And so on.

There is, I think, an instructive failure in the mark of generality view. The underlying idea is that in a formal language which has the universal quantifier as its only quantifier, we can treat variable and quantifier as two sides of the same coin.<sup>28</sup> They are essentially notational variants of each other, each being used to indicate that the proposition in which they appear is to be taken as a general one (true of everything). Once we expand our notion of quantification, however, well beyond the universal quantifier, I think an interesting conclusion is forced on us — whatever variables are, if we are to have a substantive theory of them, they need to be defined and understood prior to the advent of *any particular quantifier*. We need, that is, an understanding of variables which puts them there in their full form for quantifiers to act on, rather than seeing them as obtaining their existence and form through the particular quantifier. This idea appears, for example, in [Barwise & Etchemendy 1992]'s introduction to variables:

Their semantic function is not to refer to objects. Rather, they are placeholders that indicate *relationships* between quantifiers and the argument positions of various predicates. [115, emphasis added]

By focusing on variables as mediating a relationship between quantifiers and quantified formulae, Barwise and Etchemendy allow for the

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<sup>28</sup>To this extent, the mark of generality view is closely related to the Quinean eliminativist view discussed in §1.5.1 below. [Quine 1960] does not seem to me always to distinguish carefully between these two views.

independence of variables from particular quantifiers.<sup>29</sup> That independence, I will suggest below, is the key to the correct account of variables. I will argue, in fact, that pervasive in the modern logical tradition is a counterproductive bias in favor of thinking about variables exclusively in terms of their relationship to quantifiers, and that only once we free ourselves of that bias can we arrive at the correct view.

### **§1.5 Two Dismissive Answers**

I have suggested in the previous section that none of the remarks available in the literature on the role of variables is at all satisfying. In large part, the reason for this lack of satisfaction is that people have not intended their remarks to be taken very seriously. This, in turn, is due to the fact that people have believed that no *serious* theory of variables was necessary in the first place. Before moving on to present my own views on the role of variables and the light thus shed on the structure of quantification, I want to discuss two reasons people have had for this belief — two dismissive strategies toward the purported need for a substantive theory of variables.

#### **§1.5.1 Quinean Eliminitivism**

[Quine 1960] suggests that we have simply been misguided in assuming that variables are even a necessary or genuine part of quantificational

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<sup>29</sup>Barwise and Etchemendy, however, probably do not intend this independence to be in the service of a substantive theory of variables. Instead, they likely intend to deny the antecedent of the condition — to deny that we need or want such a substantive theory. Given the close affiliation of the neo-Fregean approach to quantification with Quinean eliminativism about variables, and given Barwise's endorsement elsewhere of the neo-Fregean account, it is likely that what we see here is a nascent manifestation of that Quinean eliminativism.

logic. Instead, Quine suggests that variables are purely a notational convenience:

Basically the variable is best seen as an abstractive pronoun: a device for marking positions in a sentence, with a view to abstracting the rest of the sentence as predicate. Thus consider the existence statements 'Some number  $x$  is such that  $x$  is prime' and 'Some number  $x$  is such that  $x^3 = 3x$ '. The variable is conveniently dropped from the first: we may better say simply 'Some number is prime', because in ' $x$  is prime' the predicate 'is prime' is already nicely segregated for separate use. The variable can be eliminated also from the second example, but less conveniently: we could say 'Some number gives the same result when cubed as when trebled', thus torturing the desired complex predicate out of ' $x^3 = 3x$ ' with a modicum of verbal ingenuity. In more complex examples, finally, use of ' $x$ ' is the only easy way of abstracting the jagged sort of predicate which we are trying to say that some number fulfills. Where the variable pays off is as a device for segregating or abstracting a desired predicate by exhibiting the predicate sentencewise with the variable for blanks. [Quine 1960, 228]

Quine's idea here has a natural affinity with the neo-Fregean account of quantification. The underlying suggestion is that a construction such as

$$(51) (\exists x)Fx$$

can be understood simply as an ascription of instantiation (or, as Quine calls it, dereletivization) to the predicate 'F'. We could thus introduce an equivalent variable-free notation

$$(52) \mathbf{Der} F$$

using a dereletivization operator **Der** with the following semantics:

$$(AX10) \text{ A sequence } \sigma \text{ satisfies } \mathbf{Der} Fx_1 \dots x_n \text{ iff some sequence } \sigma' \text{ differing from } \sigma \text{ at most in the } n+1 \text{ position satisfies } Fx_1 \dots x_n x_{n+1}.$$

To introduce the **Der** operator is, of course, just to make syntactically explicit the neo-Fregean rejection of the substantive role of the variable.

The meaning of Quine's claim that the variable acts as an abstractive pronoun comes out when we consider more complex cases.

Consider the differences among the following:

(53)  $(\exists x)(\exists y)(\exists z) Rxyz$

(54)  $(\exists x)(\exists y) Rxy$

(55)  $(\exists x)(\exists y) Rxyx$

(56)  $(\exists x) Rxxx$

we can't understand (53) through (56) simply by considering the applicability of various quantificational properties to the predicate R. We need instead to distinguish the various guises in which R manifests itself here. (53) is, so to speak, the pure occurrence of the predicate R — that occurrence which might be captured in English through an expression like

(53')  $it_1 R'd it_2 to it_3$ .<sup>30</sup>

where none of the three tokens of *it* is anaphorically linked with any other token of *it*. (53), then, can be understood as a trifold attribution of instantiation to the predicate (53'):

(53'') There is some (first) object and there is some (second) object and there is some (third) object such that  $it_1 R'd it_2 to it_3$ .

(54), on the other hand, is a partially reflexivized manifestation of R:

(54')  $it_1 R'd itself_1 to it_2$ .

(55) is also partially reflexivized

(55')  $it_1 R'd it_2 to itself_1$ .

and (56) is fully reflexivized:

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<sup>30</sup>I assume here for the sake of convenience that the argument places of the predicate R are associated with various thematic roles.

(56')  $it_1 R'd\ itself_1$  to  $itself_1$ .

Quine's claim, then, is that the sole role of the variable is to indicate which manifestation of a particular predicate is being ascribed a quantificational property — to indicate, for example, that (54) is partially reflexive in the subject and indirect object positions, while (55) is partially reflexive in the subject and direct object positions.

Quine's further claim is that variables are not the only way to make the necessary distinctions among predicates. Once one has adopted the neo-Fregean perspective and thus endorsed this Quinean conception of the role of the variable, of course, this last step follows quite quickly. One could, for example, replace the old 'R' with new predicates 'R(unreflexivized)', 'R(1,2 reflexivized)', 'R(1,3 reflexivized)', and 'R(fully reflexivized)' (and then introducing some derivational rules operating on these canonical names to capture inferential relations among the R manifestations). Quine opts for introducing reflexivization and inversion operators into his new formalism, thus giving rise to a variable-free language. Variables have been eliminated — and, Quine claims, profitably so:

The interest in carrying out the elimination is that the device of the variable thereby receives, in a sense, its full and explicit analysis. [Quine 1960, 229]

Quine thus thinks that variables have been 'explained away'. If this is right, the project of this work is shown to be pointless from the start. We are attempting to *explain* that which has already been explained away. Quine's conclusion, however, strikes me as somewhat hasty. As a formal result, of course, the eliminability of variables is impeccable. The eliminability result can serve to provide further insight into the structural properties of the predicate calculus

([Bernays 1959], [Schönfinkel 1924]), or as a guidepost to new combinatorial ways of thinking about the semantics of a quantified language ([Tarski 1948]). But the philosophical conclusions to be drawn from the formal results are suspect.

#### **§1.5.1.1 Eliminativism and Languages With and Without Variables**

As a simple-minded response to Quine, we might simply observe that while he has allowed us to construct quantified languages which do not make use of variables, he has done nothing to show us how to understand quantified languages which *do* use variables.<sup>31</sup> If Quine's syntactic alternative is to impel us to do away with variables, it must be accompanied by a reason why his variable-free style of language ought to be adopted over, or at least understood as underlying, variable-containing languages. Now perhaps there is such a reason — the neo-Fregean might take the (putative) success of his account of quantification, along with the affinity of that account for Quinean eliminativism, as supplying such a reason. But the important point here is that one cannot cut off an investigation into the nature of variables at the source by appealing to Quine's eliminative. One must argue additionally for the reduction of variable-containing languages to the eliminative paradigm, and that argument structure allows for the prior

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<sup>31</sup>Since Quine can provide a translation mapping between standard variable-containing languages and his variable-free language, he can provide the minimal understanding of such languages yielded through knowing the truth conditions of sentences in the language. But of course that level of understanding was never in doubt. The claim here is that there is more to understand about what sort of things variables are. This further level of understanding Quine does not provide, and it (I will go on to claim) is unclear why the felt need for such understanding would disappear just because another route through the formalism is made available.

construction of a substantive theory of variables to compare to the hypothetical Quinean pro-eliminative rationale.

Moreover, it's not immediately clear to me that the Quinean eliminativist project really is as eliminativist as Quine seems to hope. Let's grant that the general strategies of [Quine 1960] can be extended in a non-ad-hoc way to the full variety of cases discussed in §1.2. Nonetheless, a worry remains. The immediate manifestation of that worry is that Quine himself uses variables in the metalanguage when giving the semantics for his variable-free language. Clearly no elimination of variables has been effected if they simply reemerge in the metalanguage.

Quine's position, of course, can be stated more carefully (entirely in natural language) so that it avoids *explicit* use of variables. But — and here we reach the crucial point — the worry may not so easily disappear. One might still wonder if an adequate account of the semantics of natural language will itself require an appeal to variables. Certainly most standard approaches to syntax and semantics in linguistics make extensive use of variable-like phenomena (see, e.g., [Chomsky 1982]). Consider for example Quine's putatively variable-free natural language reworking of:

(57) Some  $x$  is such that  $x$  is a number and  $x$  is prime.

into:

(58) Some number is prime.

On versions of the Chomskyian picture, (58) itself is merely the morphological realization of a complex entity which is the entire sentence. The entire sentence might, for example, consist of a pair of logical form and surface form of the following structure:

(58-LF) [<sub>S</sub> [<sub>NP</sub> some number]<sub>1</sub> [<sub>S</sub>  $x_1$  is prime]]



(58-SF) [<sub>S</sub> [<sub>NP</sub> some number] [<sub>VP</sub> is prime]]

where the logical form (58-LF) is derived from (58-SF) through an application of a movement rule which (in order to obey the Projection Principle) gives rise to the trace 'x<sub>1</sub>' in the logical form.<sup>32</sup> These traces which appear in logical form bear a suspicious resemblance to the variables which would appear in a formal regimentation of (58) (especially one employing restricted quantification):

(58'') [some x: number x](x is prime)

When we add to the syntactic analysis of natural language a semantic theory, we will find not only that the traces continue to play a variable-like role, but also that other variable-containing language readily emerges. Thus, on the assumption that it is on the level of logical form that a sentence receives semantic analysis, we will find ourselves augmenting our syntactic analysis with semantic clauses such as:

(58-T) A sequence  $\sigma$  satisfies '[<sub>S</sub> [<sub>NP</sub> some number]<sub>1</sub> [<sub>S</sub> x<sub>1</sub> is prime]]' iff some sequence  $\sigma'$  differing from  $\sigma$  in the first position and satisfying '[<sub>N</sub> number]<sub>1</sub>' satisfies '[<sub>S</sub> x<sub>1</sub> is prime]'.

The practice of linguists and philosophers of language, then, gives us reason to consider natural languages to be variable-containing languages, and if natural languages are variable-containing language,

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<sup>32</sup>The particular version of the Chomskyan project which I sketch here is not one which I endorse, but is closer to the syntactic mainstream than my preferred construal of syntax. §2.1.3.1.2 below sketches some of my departures from the Chomskyan mainstream in the process of tracing some connections between variables, traces, and pronouns. The worries that I raise here for the Quinean eliminativist project persist at least as robustly when the syntactic theory is developed in the way I suggest later.

then a natural language explanation of the semantics of Quine's variable-free formal system threatens to reimplicate him in the task of explaining the nature of variables.

Quine's claim that variables are to be analogized to pronouns is particularly interesting in this light. I will argue below (especially in §2.1.3.1) that pronouns are a type of variable, and that by examining the behaviour of pronouns we can gain useful insights into the semantics of variables and of quantification. To the extent, then, that Quine agrees that there are substantive issues in understanding the function of pronouns in natural language, he and I are in complete agreement and his eliminativist aims vanish into the murky seas of language. Quine's reference to the variables as 'abstractive' pronouns, however, hints that he has a less substantive understanding of pronouns in mind.

Obviously, these considerations leave the matter far from settled. We will need to — and will, throughout Chapter 2 — consider in much greater detail the demands placed on us by theories of syntax and semantics for natural language, and the relations between entities posited by such theories and variables before we can fully determine whether Quinean eliminativism can be carried out successfully. For now, however, it suffices to observe that the eliminativist project does not immediately carry the day, and that the call for a substantive theory of variables can still be heeded.

### **§1.5.2 Tarskian Silentism**

The second dismissive response to the call I issued above derives from what I take to be a natural curiosity about why we need a *theory* of variables at all. All of us, the dismissive interlocutor might respond,

know perfectly well half a dozen methods for writing down semantic theories for any of a variety of formal languages which employ variables. Why not rest content with this knowledge and hold that there is no more to variables than is written in our Tarskian truth definitions? Call this response the move of *Tarskian silentism*.

#### **§1.5.2.1 Silentism, Unification, Logical Platonism, and Linguistic**

##### **Psychologism**

Tarskian silentism deliberately rejects the need, declared earlier, for a unificatory account of variables. If the silentist is right, we may well fail to obtain such an account: looking at the semantics for a first-order language will tell us all there is to know about variables in that language, and looking at the semantics for a second-order language will tell us all there is to know about variables in that language, and if these two examinations tell us nothing about *why* the variables in the first-order language are the *same sort of thing* as the variables in the second-order language, then so much the worse for us. The silentist will not be disturbed by any resulting lack of unification, since he is both likely and free to say that he cares nothing for the category 'variable'. He cares only about understanding the formal systems he engages with, which he can do quite adequately with his array of truth definitions. If it bothers you to call both the x's and y's in all my languages variables, the silentist can respond, call them something else. It will have no impact on the formal results drawn from the system. As long as the semantics works, why care whether it employs something with a legitimate right to be called a variable?

The silentist, of course, may have the usual aesthetic considerations in favor of a unified account — the elegance of a theory which draws together previously disparate phenomena under a single roof, the formal advances which frequently accompany the drafting of such a theory — but such considerations are relatively unlikely to suffice to motivate the kind of wholesale revision in our understanding, and occasionally practice, of logic that I am inclined to endorse as consequences of my account. The silentist, for example, may well be pleased to adopt the neo-Fregean view as a method for treating classical first-order logic as a special case of second order logic, but will be unwilling to reject plural quantification just because it fails to conform to the neo-Fregean mold. Instead, he will simply treat plural quantification as yet another formal system, but one not exhibiting the structural features characteristic of those systems which can be seen as instances of neo-Fregeanism.

I want tentatively to suggest two non-aesthetic reasons why, pace the silentist, we ought to seek a unified account of variables (and thus ought to reject mere silentism). A defense of either of the reason-giving positions I am about to sketch goes well beyond the scope of this work, so for these purposes I won't rely on their plausibility — in the next section I will set out reasons to reject silentism independent of issues of unification. Nevertheless, I think there is enough truth and interest in these two positions to make a brief examination of them worthwhile.

The first position giving us reason to seek unification and reject silentism I will call logical platonism. The silentist claims that there need be no interesting category of the variable, that one can simply

treat the variables (and quantifiers) within any given formal language as constructs whose entire nature is given by the behaviour of that language and which need not be of a type with so-called 'variables' ('quantifiers') in other formal languages. We might, however, hold that there are facts about logic independent of our construction of particular formal languages. In particular, there might be independent categories of logical operations and devices which we implement in various ways through our formal languages. Were this the case, we could reject the silentist's silence not as wrong, but as incomplete. There would be a fact about whether two things were both variables (or, weaker, whether two things had a logical and semantic behavior that fell within a certain category of logical operations) which a complete theory would explain.

The second position I will call linguistic psychologism. Broadly speaking, this is the thesis that there are facts about the mental lives of speakers which are partially or fully determinative of the syntactic and semantic properties of their utterances. Defined so broadly, of course, no one could object to linguistic psychologism. For our purposes, we will need a version of the position which is strong enough to ground a *prima facie* preference for simpler and more unified accounts of the syntax and semantics of natural language. The rough idea here is that, if the syntax and semantics of natural languages are tied closely enough to what actually goes on in the heads of speakers, then well-known considerations concerning language learnability and speakers' competence (see, e.g., [Davidson 1965], [Chomsky 1985]) will lead us to prefer an account of those syntactic and semantic features which gives them the simplest and most unified treatment. The hope, then, would be

that a substantive theory of variables leading to a unificatory account of quantification would have an ability to explain the competence of speakers which eluded the heterodoxy of the silentist.

Motivating my project in its fullest form will require the backing of both logical platonism and linguistic psychologism — logical platonism to justify unification within formal languages where no learnability or competence issues apply, and linguistic psychologism to justify unification within natural languages.

#### **§1.5.2.2 Mathematical Versus Philosophical Silentism**

I am inclined to think that there is enough truth to the rough positions of logical platonism and linguistic psychologism sketched in the previous section to justify using a need for unification as a motive for seeking a substantive theory of variables. However, I don't want here to rely on such underdeveloped and underdefended positions, so I want now to suggest that, questions of the desirability of a unified account of variables aside, we as philosophers still need a substantive account not provided by the silentist response.

[Kripke 1976], speaking in quite a different context, endorses a variety of Tarskian silentism about substitutional quantifiers. After giving a truth definition for a language  $L$  containing such quantifiers, he remarks that "any working mathematical logician would regard [the truth definition] as settling the question whether the semantics of  $L$  has been given" (333). There is certainly *something* correct in this silentist line. To shift slightly the emphasis of the Kripke quotation, any *working mathematical logician* can gladly make this response. But what about the rest of us (in particular, what about philosophers)? I'm

not so convinced that the rest of us can be so sanguine. If your interests extend only to the structural, mathematical properties of certain types of systems, of course you can just set up the system in whatever way you find comprehensible and then start exploring those structural features.<sup>33</sup>

As philosophers, however, we are often interested in a great deal more about formal systems than just their abstract shape. Once one starts putting variables to philosophical work, which many have wanted to do, a theory about them is already implicit, and an *explicit* theory is then required for good philosophy. To see that one's philosophical needs may not be compatible with just any formal device for handling variables, consider [Quine 1948]. Here Quine endorses the doctrine of ontological commitment that "to be is to be the value of a bound variable" (15). Clearly Quine here puts variables to a philosophical use, and a use that not just any semantics for variables will support. In particular, his own eliminativist tendencies elsewhere are not clearly compatible with his philosophical methodology here.<sup>34,35</sup>

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<sup>33</sup>Of course even pure mathematics is not so free of ideology as this remark might suggest. If you want to know whether certain structures (say the ZFC universe) have certain structural properties (say obeying the GCH), it's no good to characterize these structures using tools (such as second-order logic) which themselves presuppose certain answers to these questions. But it is *perhaps* easier to isolate these kinds of concerns in mathematics than in philosophy.

<sup>34</sup>One suspects, of course, that the real doctrine of [Quine 1948] is intended to be: "to be is to be in the domain of quantification of an existential quantifier". Nevertheless, the point remains that the doctrine as stated cannot (easily) be combined with an eliminativist take on variables.

<sup>35</sup>For another example of a philosophical application of variables which seems to require more than silentism as a theory of variables, consider [Kaplan 1977]'s assertion that "free variables under an assignment of values are paradigms of what I have been calling directly referential terms" (484). That this parallel between variables and directly referential terms such as demonstratives, indexicals, and proper names depends on holding substantive views on the nature of variables should be clear — consider the oddity of holding simultaneously Kaplan's views

My claim, then, is that Tarskian silentism, whatever its virtues for the pure logician, falls short when we turn to the philosophical problems raised by and associated with variables and quantification. Of course, the argument still needs to be made case by case that the application to which a particular formal system is being put requires substantive views on variables. I hope a convincingly large number of such cases will come out over the course of this work, but we will, as a promissory note on that larger project, turn in the next section to alluding to some philosophical questions in which variables are implicated.

#### **§1.6 Some Questions About Variable Binding**

In this section, I want merely to list a number of questions about variables, quantifiers, and the process of variable binding. The questions range from the purely technical to the purely philosophical, but all, I think, point toward potential deficiencies in our current understanding of these topics. It is not my intention in this work to answer all of these questions, or even to provide the necessary groundwork for answering all of them. Some will be explicitly addressed, and more will have clear connections to topics discussed below. Others, however, must serve as directions for further research, or as potential test cases for the general approach I endorse here.

- What is the arity of the relationship of variable binding? Is the relationship binary, as conventional wisdom holds? If it is not binary, what are the other relata? Is the arity fixed, or can there be different
- 
- on the semantic parallel and a view which takes variables to be any of semantic slot machines, blurry names, marks of generality, or in-principle eliminable markers of predicate abstraction. My views on the relation between variables and so-called directly referential terms are set out in considerable detail in §2.3 below.



types of binding relations imposed on a core notion of variables? Can the variable binding relation hold, for example, between a single quantifier and multiple variables?

- What are the domain restrictions on variable binding? Must variable binders be linguistic objects? If non-linguistic objects can bind variables, do these objects need to be contentful, or can they be wholly natural?

- Are quantifiers best understood, as in the modern tradition, as sentential operators? If so, how is it that the binding of a variable takes an object which is nonpropositional — which does not have a truth value and does not express a complete thought — and converts it into something which is propositional? What notion of completeness of proposition underlies this potentiality of variable binding?

- Are there variables in natural language? If so, what are they, and how many of them are there? Do they all have visible manifestations in surface syntax? More generally, what are the individuation criteria for variables? In virtue of what in any language, formal or natural, are two things instances of the same or different variables?

- Are the semantic effects of quantifiers limited by syntactic features of scope? If so, why are there such limitations at all, and why are there the particular limitations that there are? What is the connection between the scopal properties of quantifiers and the order-dependence of quantifier combinations? Why does the order of quantifiers matter? Is there a way to understand variable binding which does not create linear, or any, ordering relations?

- Is there any interesting distinction between variables and schema letters? Does one carry ontological commitment and the other not? If so,

why? Can we make sense of variables which are incapable of entering into binding relations?

- What sorts of semantic values do variables assume, and what determines the available range of such values? Do we need to understand the meaning of a variable relative to some other factor?

- What, if anything, is the connection between variables and proper names? Do proper names provide a useful paradigm for understanding variables? Do variables provide a useful paradigm for understanding the putatively directly referential quality of proper names? Is there any difficulty in having each serve in this way as a paradigm for the other?

- Does the distinction between singular and general thoughts or expressions line up with the distinction between non-quantificational and quantificational sentences? If so, why?

- Is there any interesting connection between the notion of a variable binder and the notion of logicality? Must all variable binders be logical? If so, why? If not, is there an independent notion of logicality which will shed light on which variable binders are logical?

And is there any clear demarcation of the range of nonlogical variable binders?<sup>36</sup>

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<sup>36</sup>On the arity of the variable binding relation and the nature of the relata, see especially §3.2.1. See also §3.2.1.2 on the variability of the arity and the possibility of one-many quantifier-variable binding relations, and §3.3.3 on polyadic determiners. The present approach assumes throughout that all variable binders are sentential operators; for implicit support for this position and at least a beginning of an explanation for the ability of quantifiers to map the subpropositional into the propositional, see throughout chapter 2 the discussion of the relation between natural language noun phrases and the quantificational structure I posit. See also §2.3.4.1 for some discussion of the notion of a complete proposition. On variables in natural language, see §2.1.3, and on the individuation criteria for variables see §2.3.2.3.2. On the relation between the semantic effect of quantifiers and syntactic scope properties, see §2.2.1, and especially §2.2.1.2.2 for discussion of the need for scope-like restrictions on the semantic effect of determiners. See also §3.3.2 for extended discussion of the order dependence of

## **§1.7 The Anaphoric Account of Variable Binding**

I want to close this chapter by sketching the theory of variables, and the subsequent theory of quantification, that I intend to defend in this work. In this section I will do little more than gesture toward the outlines of that theory; much of the rest of this work will be devoted to filling in the details of the sketch given here, and the final chapter will provide a rigorous investigation of the ideas set out here.

### **§1.7.1 Natural Language Preliminaries**

I indicated earlier (§1.2.13) that I wanted to look extensively toward natural language for my evidence basis when constructing a theory of quantification. In that spirit, I want to preface the details of my account of variables and quantification with a brief discussion of two logical phenomena from natural language which help motivate the form of my account.

#### **§1.7.1.1 Anaphora**

First, consider the phenomenon of anaphora. In natural languages, there are certain terms which have no independent meaning of their own, but

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quantifiers, especially §3.3.2.2.3.3 for a formal picture relating scope-like restrictions on determiners to order-dependent semantic interactions in quantifier blocks, and §3.3.2.2.3.1 for an order-independent notion of quantification. On the distinction between variables and schema letters, and the ontological commitments of higher-order quantification, see §3.2.2.1; see especially §3.2.2.2.1 on the possibility of in-principle unbindable variables. On the semantic values carried by variables, see §3.2 and especially the discussion of plural reference in §3.2.1; see also §3.2.2 for semantic values of higher-ordered variables and §2.2.2.2.2 for intensional semantic content of variables. On relativized understanding of variable content, see §3.2.1.2. On the connection between proper names, see extensive discussion in §2.3, especially in §2.3.2. On the distinction between singular and general thoughts and the relation of this distinction to the distinction between non-quantificational and quantificational sentences, see especially §2.3.2.4.2 and §2.3.2.4.3. The question of quantifier logicity is only touched on briefly in this work, but see §3.2.1.3.2 for some discussion and also §3.3.3 for more detailed discussion of the particular case of the logicity of polyadic quantifiers.

which are used to inherit meaning from other items in the lexical context. The paradigm example of anaphora — that inheritance of meaning — is pronominal reference. Consider an example such as:

(59) **Hitchcock** called **his** actors cattle.

Here there is an anaphoric relation between the proper name 'Hitchcock' and the pronoun 'his'. That pronoun, considered independent of any context of use, does not serve to refer to anything. However, once placed in a context of use like (59), the pronoun inherits a referent from the name 'Hitchcock', and comes to refer to Hitchcock. Anaphora is apparently not limited to the pronominal cases, however: consider cases of so-called verb phrase (VP) deletion, as in:

(60) John watched *Vertigo*, and Mary did too.

Here the verb 'did', considered independent of any context of use, does not pick out any action. However, once placed in a context of use like (60), 'did' inherits content from the earlier VP 'watched *Vertigo*', and comes to ascribe to Mary the act of watching *Vertigo*.

My claim will be that natural language anaphora is a species of variable binding, and that by looking to the structure of anaphora we can uncover much about the structure of quantification. I thus call my account an anaphoric account of variable binding.

#### **§1.7.1.2 Restricted Quantification, Again**

The second natural language phenomenon to which I wish to draw attention has already been alluded to earlier (§1.2.3.2). Quantification in natural language is exemplified by the quantified noun phrase, which makes use of restricted quantification. Thus the typical quantified noun phrase breaks down into two components: the common noun (or N'), which

serves to restrict the domain of quantification, and the determiner, which implements quantification on that restricted domain. I intend to take seriously this appearance of dyadicity. Thus I will suggest that there are two distinct components, capable of and requiring distinct analysis, to quantification, mirroring the two components of the quantified noun phrase. More controversially, I will suggest that it is just one of these components — the restriction by the common noun — that effects the crucial process of variable binding.

### **§1.7.2 The Anaphoric Account of Variable Binding**

Classical logic tells us that the variable binding operators are the existential and universal quantifiers, and subsequent logical work has taken these two operators as paradigmatic and attempted to expand them into a broader class meeting various constraints. This entire tradition, however, rests on a mistake. Neither the existential nor the universal quantifier is a variable binding operator, nor is either even the right kind of thing to bind a variable. Classical logic instead smuggles in (and abuses) a prior notion of variable binding and then applies to the consequent bound variable a separate class of operators.

I think some intimation that classical logic has looked in the wrong place for variable binders can be gleaned from considering other uses of classical and other quantifiers. These quantifiers, in natural language, can be attached not only to variables, but also to any referring or even Russellian denoting expression:

(61) **Both** Mark and Albert enjoyed the play.

(62) **Some** of them will join us at the theatre.

(63) **All** the men enjoyed the portrayal of Iphigenia.

These constructions at least suggest that natural language determiners (the counterparts of formal quantifiers) are not exclusively variable binding operators, but rather are general semantic operators on plural reference.

To draw together my rejection of classical quantifiers as variable binding operators with the undeniably important position of such quantifiers in quantificational theory, I appeal to the dyadicity of quantification observed in the previous section. My suggestion is that we consider *two distinct components* of the quantificational process. The first of these two components is the *anaphoric binding of a variable*. A variable is an item in the lexicon which has the ability to inherit semantic content (anaphorically) from other parts of the lexical context. The variability of a variable, then, is over lexical contexts. The semantic content, moreover, will paradigmatically be inherited in a transformed condition. In the first-order case, semantic content is passed on from a predicate. Predicates possess some semantic content in virtue of which they are able to distinguish among objects. Thus, for example, there is some fact about or associated with the predicate 'is red' in virtue of which that predicate serves to divide the world into two classes — the class of things satisfying the predicate (a stop sign) and the class of things not satisfying it (a lime). This semantic capacity of the predicate is then passed on to the bound variable, where that content causes the variable to refer to those objects distinguished by the predicate (the stop sign) but not to those not so distinguished (the lime). More generally — to move beyond the first-order case — if there are semantic categories  $C$ ,  $C^*$  such that elements of category  $C^*$  carry semantic content which can be used, via some transformation, to

distinguish semantic contents of the type associated with category C, then a variable (in a syntactic position associated with semantic category C) can be restricted by the C\* term to become a C term. This process of restriction is the first of the two components of quantification, and the one which embodies the true process of variable binding.

#### **§1.7.2.1 Restriction, Enhancement, and the Word-World Connection**

The terminology of restriction used in the previous paragraph is rather infelicitous. While the awkwardness of alternatives, combined with the established reference in the literature to restricted quantification, persuade me to adopt the infelicitous terminology, any associated suggestion that there is a *preexisting value* of the variable being restricted by the restrictor should be firmly rejected. Variables are not restricted by restrictors in the sense of having their semantic options limited. The restrictors are rather enablers of the variables, providing whatever semantic value these variables have. This distinction between the restricting and the enabling nature of variable binding is not, I think, an unimportant one. Classical logic in essence opts for the restricting view. By pushing the anaphoric binding of the variable entirely out of the picture, it implicitly assumes that the variable, prior to the advent of the quantifier, already possesses some semantic relation to the entire universe of discourse, which relation allows it to range over that universe.

By covertly opting for the restricting view of variable semantics, classical logic risks violation of an important limitation on our

semantic and cognitive abilities. Borrowing from [Evans 1982], I want to identify what I will call the General Russellian Principle (GRP):

(GRP) In order for an agent to talk or think about an object, that agent must have some route of access to that object.

I leave deliberately vague the range of methods of 'accessing' an object — they might include having an appropriate description, being related through a causal chain to someone who had an appropriate description, being acquainted with, being able to identify or recognize, or merely being causally connected with. The underlying intuition is that I cannot make reference to some far distant nondescript object which neither I nor my linguistic community have ever encountered. While the GRP does require that there be some connection between what goes on in speaker's heads and what semantic properties their linguistic utterances and psychological states enjoy, I believe that it is so a weak connection condition as to be wholly unobjectionable. Evans' so-called 'Russell's Principle' (see [Evans 1982, 43]), which pursues the a much more robust version of the same underlying intuition I attempt to encode, will, due to its requirement that speakers know what object they are speaking about, be rejected by certain advocates of direct reference theory — those who advocate anything like what Evans calls the 'Photograph Model.' (pp.76-79) The GRP on the other hand, should be wholly acceptable to both the ardent internalist and the hardened externalist — although the two will, of course, disagree about what counts as an appropriate route of access.

My claim is that, despite its apparent innocuousness, the GRP is violated by the semantics of classical logic. Just as the GRP prohibits access to distant nondescript objects, it also prohibits reference to



absolutely everything (including those distant nondescript objects) without the presence of some mechanism through which we gain access to all of those referents. But when classical logic assumes — as I have suggested it implicitly does — that variables arrive on the scene fully semantically stocked, referring to everything (in the universe of discourse), it violates the GRP by failing to provide any mechanism to account for that universality of reference.<sup>37</sup> My account, on the other hand, is through the mechanism of anaphoric binding intended to respect the GRP. On my account, it is the conceptual content of the restrictor, under which the referents of the variable fall, which provides the route of access required by the GRP.

#### **§1.7.2.2 The Anaphoric Account of Variable Binding**

In sketching the structure of anaphoric binding above, I concentrated primarily on the first-order case, in which a (referential) variable is bound by a predicate-like construction. I indicated, however, that the account could be generalized beyond the first-order case by considering any two semantic categories  $C$  and  $C^*$  such that elements of category  $C^*$

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<sup>37</sup>This failing of classical logic can be corrected, of course, by providing some explanation of how we gain access to the referents of the variables. In a formal language, we can take the method of specification of the universe of discourse to provide the route of access (although we will also, I suggest, have to suppose that that specification of the universe of discourse is accessible to any (hypothetical) speakers of the formal language). Were classical logic to provide the appropriate analysis of natural language, we would have to assume that, since we want our quantifiers to range over everything, we (as speakers of the natural language) have some route of access to everything. Such an access route *may* not be problematic — an existence predicate or a tautological predicate like 'red or not red' are obvious candidates. But (a) such routes are not entirely free of worries, and (b) even if a worry-free route is available, its *necessity* shows that the primary form of quantification is restricted, rather than unrestricted, and that classical logic merely plays off our ability to provide trivial restrictors. The relation of unrestricted to restricted quantification and the implications of that relation for our understanding of logic and natural language is taken up in more detail in §3.2.1.3.

carry semantic content which can be used, via some transformation, to distinguish semantic contents of the type associated with category C. The necessary relation between categories C\* and C was left deliberately vague earlier. We can now see that that vagueness is meant to mirror the corresponding vagueness of the concept of a route of access appealed to in the GRP. Anaphoric binding must give rise to linguistic behaviour in a way which respects the GRP, so whenever there is binding between C\*-terms and C-terms, the C\* semantic content must contain routes of access to the C contents. Think here of the difference between, on the one hand, the predicate 'whale' giving rise to reference to all whales and, on the other hand, the referring expression 'Kripke' giving rise to all predicates satisfied by Kripke. Intuitively, the semantic content of 'whale' is adequate to provide an access route to the individual whales, while the semantic content of 'Kripke' is inadequate to provide access to the properties of Kripke.

The first component of quantification, then, is anaphoric variable binding. This component is suppressed in classical logic (emerging implicitly through the specification of a universe of discourse), and in that forum everything is focused on the second component: the *distribution of the bound variable*. Once a variable has received semantic content through anaphoric binding, that semantic content (like any semantic content) can be operated on in various ways. These ways include, but may not be limited to, the determiners of modern logic, such as the classical ' $\forall$ ' and ' $\exists$ '. In the first-order case, these operators are distributors of reference — they take the plurally referring variable and divide it into a (possibly infinite) group of expressions referring to the appropriate number of objects. The

determiner ' $\exists$ ', for example, will distribute the plural reference of the bound variable into references each of which contains at least one of the original plurality.<sup>38</sup> The determiner ' $\forall$ ' will perform a trivial distribution, leaving intact the original plurality. The natural language determiner 'most' will distribute the reference of the variable into references each of which contains most of the original plurality. And so on. The precise details of distribution will be taken up later (primarily in chapter 4), but for now the important points are (a) distribution is a species of semantic operation, and is defined in such a way that it need not be restricted to variables, and (b) when the distribution process is applied to a variable, it can be made sense of only in light of the prior anaphoric binding of that variable (else there is nothing available to distribute).

#### **§1.7.2.2.1 A Simple Example of Anaphoric Binding**

The above sketch of the anaphoric account was given at a high level of generality, in order to set out as global a version of the account as possible. While we will in subsequent chapters be considering numerous examples of implementations of the account, in order to trace out its expressive resources and formal and philosophical implications, I want here to give one simple example of its use, just to make concrete the framework given above.

Consider the following sentence of English:

(64) Most philosophers know logic

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<sup>38</sup>One could also define the ' $\exists$ ' determiner by having it distribute the anaphorically provided reference into groups of exactly one object. See the discussion, in §3.3.1.1.1 below, of the distinction between strong and weak readings of distributors.

My claim is that this sentence exhibits quantification, and quantification best understood via the anaphoric account. To make explicit the quantifier-variable substructure of (64), let's look at more detailed syntactic structures associated with it, such as:

(64') [most x: philosopher(x)] knows-logic(x)

(64'') [<sub>S</sub> [<sub>NP</sub> [<sub>D</sub> most] [<sub>N</sub> philosophers]]]<sub>1</sub> [<sub>S</sub> [<sub>NP</sub> x<sub>1</sub>] [<sub>VP</sub> knows logic]]]

(Questions about the appropriate syntactic representation of quantificational structures will be taken up later.)

The quantification in (64) involves, according to my view, two stages. First, an anaphoric binding of a variable occurs. Here the anaphoric binder is the noun 'philosophers'. That noun passes on to the variable its semantic capacity to discriminate between philosophers and non-philosophers, and in virtue of that new ability the variable becomes a plurally referential term referring to all philosophers. At this intermediate stage, then, we might think of the sentence as having a form such as:

(64''') [<sub>S</sub> [<sub>NP</sub> [<sub>D</sub> Most] [<sub>NP</sub> them]] [<sub>VP</sub> know logic]]

where 'them' is understood as referring to all philosophers. Once the anaphoric binding has occurred, the determiner 'most' can act on the newly plurally referential variable. The effect of the determiner is to distribute that plural reference into a number of new plural references, with the distribution pattern determined by the semantics of the particular determiner. In the case of 'most', the distribution is done in such a way that each referent yielded by the distribution contains most of the original philosophers.

Thus, for example, if there were exactly three philosophers,  $P_1$ ,  $P_2$ , and  $P_3$ , then 'them' (i.e., the anaphorically bound variable) would, prior to distribution, refer to all of  $P_1$ ,  $P_2$ , and  $P_3$ . The application of the determiner 'most' would then distribute that plural reference into four new plural references:

(R1)  $P_1, P_2$

(R2)  $P_1, P_3$

(R3)  $P_2, P_3$

(R4)  $P_1, P_2, P_3$

each of which obeys the distribution rule for 'most'. (64) itself will then be true if and only if some one of the distributed referents satisfies the original VP 'knows logic' — that is, if it is true of at least one of (R1) through (R4) that they know logic.

#### **§1.7.2.2.2 Dyadic Quantification and a Brief Taxonomy of Variables**

Drawing together the components of quantification set out above, we arrive at a dyadic view of quantification. We have a variable sitting in a syntactic position associated with some semantic category  $C$ . Elsewhere in the linguistic environment there is another term of semantic category  $C^*$ . Through anaphoric binding, the initially semantically null variable comes to be associated with the  $C^*$  term, and via the route of access provided by the semantics of that  $C^*$  term, the variable becomes a legitimate  $C$  term. The  $C$ -type semantic value of that variable can then, in the second half of the dyad, be acted upon by certain types of semantic operators — paradigmatically, for our purposes, distributing operators, which will take the new-found semantic value of the variable

and decompose it in certain ways to impose new truth conditions on the utterance.

Because quantification is now seen to be dyadic, rather than monadic, the old dichotomy of free and bound variables is replaced by a more complex four-fold categorization. First, a variable may be wholly free — neither anaphorically bound nor subsequently distributed. In this case, it remains merely a semantically empty syntactic placeholder. Second, a variable may be anaphorically bound but not subsequently distributed. In this case, the variable retains the full semantic potency it acquired from its anaphoric binder. Third, a variable may lack anaphoric binding yet still undergo distribution. This category we will reject as incoherent, since absent anaphoric binding there is no semantic content available for distributing operators to operate on. Thus we see here that anaphoric binding is conceptually prior to distribution, since the latter cannot be made sense of without the former. Finally, a variable can be both anaphorically bound and subsequently distributed. This final case will closely model the behaviour of the traditional quantified noun phrase.

The above sketch of the anaphoric account of variable binding is only a sketch. In subsequent chapters, we will work toward expositing and defending that account, filling in some of the lacunae left here and illustrating how the distinctive features of the account make it uniquely well suited to our formal and philosophical goals. In the end, I hope to show that in the anaphoric account we have a powerful new theory which is technically descriptive — not requiring that we change our formal practice — but philosophically prescriptive — demanding

that we do change how we *understand* that formal practice, and the philosophical purposes to which we put that practice.<sup>39</sup>

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<sup>39</sup>I want my account to prove technically descriptive (rather than prescriptive) both so that we can retain the well-established practice of classical logic and so that the account can have some hope of serving the unificatory ends I set earlier (§1.1) for an adequate account of quantification. However, inevitably as we approach the fringes of the notion of quantification my description will occasionally turn to prescription, and we will find at times that the anaphoric account simply forces us to reject that what some people have taken as quantificational can indeed be made sense of as quantificational. I find this drift from the descriptive to the prescriptive not only inevitable but desirable. One difficulty with recent work in formal semantics is that authors have shown a regrettable tendency simply to cobble together a formalism to account for whatever range of data confronts them, without pausing to think about how coherent the underlying conceptual foundations of that formalism are. To the extent that my account provides a tool for clearing away some of the burgeoning formal underbrush and allowing room for philosophical growth, I think it can only prove an asset to the semanticist.

## Chapter 2

### Variables and Natural Language

#### §2.1 An Introduction to Noun Phrases

The most cursory examination of natural language will uncover a subject-predicate structure to sentences: we use parts of sentences -- subjects -- to pick out things in the world, and other parts of the same sentences -- predicates -- to ascribe qualities to those things picked out by the subject. Call those expressions which can appear in a subject position noun phrases (NPs). Clearly it will be of no small interest to philosophers to understand how noun phrases work, since it is here that we find the linguistic nexus for world-word connection and ontological commitment.

##### §2.1.1 The Diversity of Noun Phrases

Once we begin a careful examination of noun phrases, however, it rapidly becomes clear that gaining thorough understanding of their function is a formidable undertaking. The most immediate, though hardly the only, problem is that noun phrases turn out, on inspection, to be quite a diverse category. All of the following are expressions which can serve as subjects of sentences:

(56) Aristotle

(57) This

(58) Those philosophers

(59) Several tall men

(60) The first postmaster general

(61) More philosophers than linguists



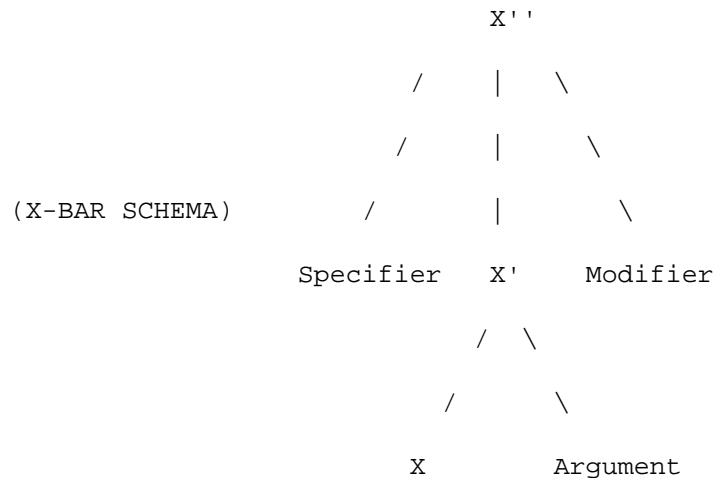
- (62) I
- (63) He
- (64) Justice
- (65) Gold
- (66) Whales
- (67) That Gödel proved the Completeness Theorem
- (68) My favorite paper by Kripke
- (69) Studying logic
- (70) Now
- (71) It
- (72) Whoever
- (73) Even a philosopher
- (74) To understand natural language
- (75) Who
- (76) Which admirer of Landow
- (77) Both Albert and Frederick
- (78) Either Elizabeth or Louise

#### **§2.1.1.1 Syntactic Difficulties in Unifying the NP Category**

This array of noun phrases exhibits two troubling types of diversity. First, it would seem that there is no clear syntactic form associated with the category of the noun phrase. Indeed, it must be taken as a tribute to the strength of the naive intuition that language has the subject/predicate structure sketched at the beginning of this section that the category NP survives at all in contemporary linguistics. Absent that intuition, the driving theoretical principles of Chomskyian syntax

give us reason to reject the claim that (56)-(78) above share a syntactic category.

In Chomskyian syntax (as developed in, say, [Chomsky 1982]), the starting point for generating well-formed syntactic structures is X-bar theory.<sup>40</sup> According to X-bar theory, sentences in natural language arise through the repeated instantiation of a basic phrasal structure. Any phrase type -- noun phrase, verb phrase, prepositional phrase, adjectival phrase, or (as we will see) complete sentence -- has the same underlying structure. In each case, one begins with a phrase head -- a lexical item drawn from a given lexical category. That head is then projected repeatedly via a certain structure to create an entire phrase. There is some disagreement about what the appropriate X-bar structural schema is, but for our purposes we can adopt as paradigmatic the following structure (taken from [Sells 1985]):

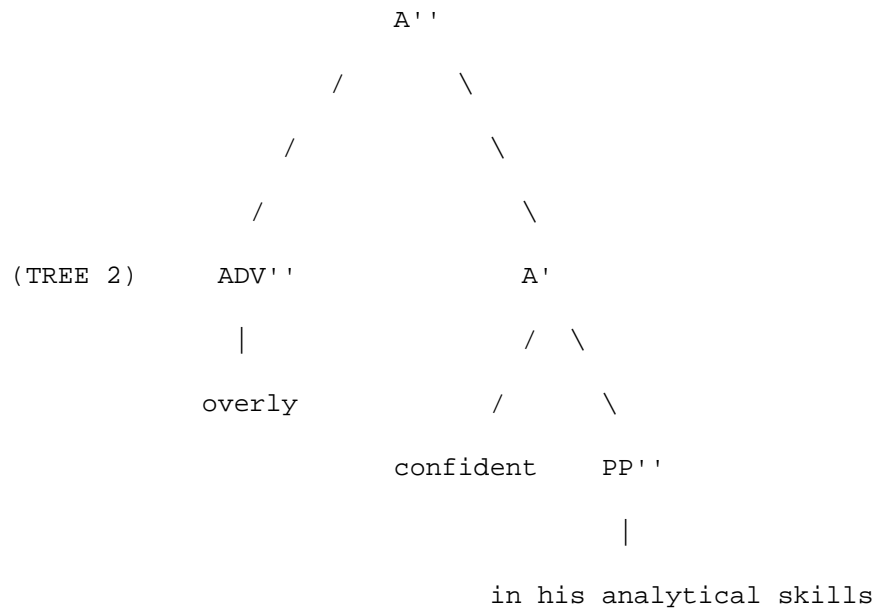



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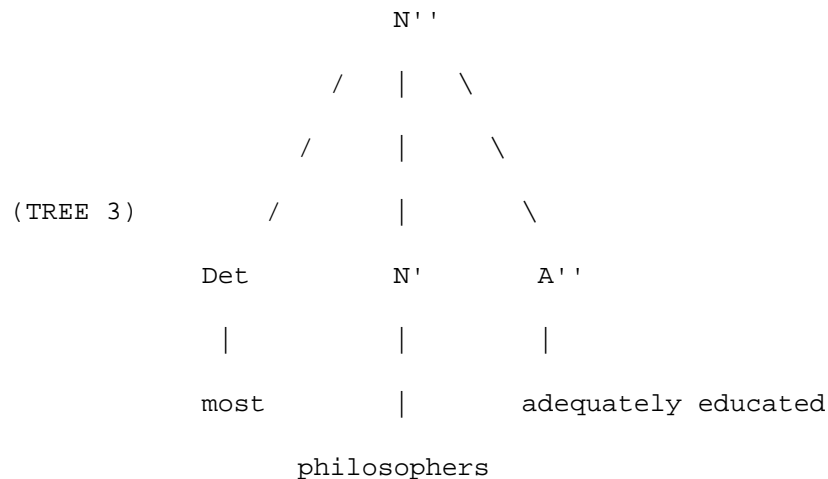
<sup>40</sup>The following discussion of X-bar theory is highly schematic, and omits discussion of numerous complexities which would have to be settled in order for a fully adequate X-bar theory to be used as the basis for a syntactic theory. The goal here is just to give enough of a sample of X-bar type reasoning to see how such a theory might ground a syntactic theory and then to show how certain types of noun phrase cause problems for the assumption that X-bar theory lies at the heart of our syntactic knowledge.



corresponding to the English verb phrase 'admires the famous philosopher for his logical work'. Similarly, we can generate an adjectival phrase by drawing from the lexicon an adjective such as 'confident'. We can then project this adjectival head upward through the addition of arguments and specifiers to create:



corresponding to the English 'overly confident in his analytical skills'. Moving back in the direction of our original topic, we can also construct noun phrases using X-bar theory. Thus we can take a noun like 'philosopher' and add to it a determiner and a modifier to create:



corresponding to 'most adequately educated philosophers'.

The elegance of X-bar theory derives from two sources. First, X-bar theory holds that all phrase types have the same underlying generative procedure. Second, X-bar theory shows how X-bar structures can recursively call on the X-bar schema to create elaborate phrases. Thus in our verb phrase 'admires the famous philosopher for his logical work', we embed within the X-bar structure for the verb phrase two additional instances of the X-bar schema -- one generating the noun phrase 'the famous philosopher' and one generating the prepositional phrase 'for his logical work'. If we add to X-bar theory the assumption that sentences, as well as phrases, are maximal projections via the X-bar schema of some lexical head (such as the predicate inflexion; the details of what lexical entry projects to the sentential level need not concern us here), we see that the basic framework for sentences can be generated through repeated calls to the X-bar schema. Of course, considerable work remains to be done to transform the outputs of the X-bar process into the full range of grammatical sentences, but an impressive and impressively economical core has been established.

X-bar theory, then, is a vital component of Chomskyian syntax, and that theory says that all noun phrases are maximal projections of nouns via the X-bar schema. But even a cursory examination of the list (56)-(78) of noun phrases above will show that not all noun phrases have the structure X-bar theory says they should have. None of (56), (57), (62), (63), (70), (71), (72), (75), (77), or (78) contains a noun at all, and thus none of these can be a maximal projection of a noun.<sup>41</sup> (64), (65),

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<sup>41</sup>[Burge 1973] claims that proper names should in fact be understood as a type of common noun. Part of the evidence for this view is that proper names do appear at times to be combinable with determiners, as in:

and (66) all contain nouns, but each fails to project its noun at all, let alone according to the X-bar schema. (67), (69), and (74) do contain the syntactic articulation which allows them to be construed as projected structures, but don't appear to be projections of the noun. (69) is at least *prima facie* some sort of projection of the verb 'study', while (67) and (74) appear to be projections either of the complementizers 'to' and 'that' or of the inflection. (61) is plausibly a projection of the noun 'philosopher', but clearly not a projection obtained via our X-bar schema. Of the 21 noun phrases listed above, only

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(FN 10) There are several Sophias in my daughter's class.

or:

(FN 11) The next Hitler will come from Bosnia.

However, I am unconvinced that such examples genuinely support treatment of proper names as common nouns. Some proper name/determiner combinations, such as that in (FN 11), are clearly metaphorical uses, and should perhaps be understood as something like:

(FN 11') The next person like Hitler will come from Bosnia.

Note here the difficulty in making sense of a construction of the form of (FN 11) when the included proper name is not the name of a person with some well-known salient characteristic:

(FN 12) The next John Smith will come from Bosnia.

or even:

(FN 13) The next Gerald Ford will come from Bosnia.

Proper name/determiner combinations of the type exemplified by (FN 10) are not, I think, to be taken as metaphoric. Instead, such constructions should be understood as playing off natural language looseness with use/mention issues, and thus should be understood as:

(FN 10') There are several people named 'Sophia' in my daughter's class.

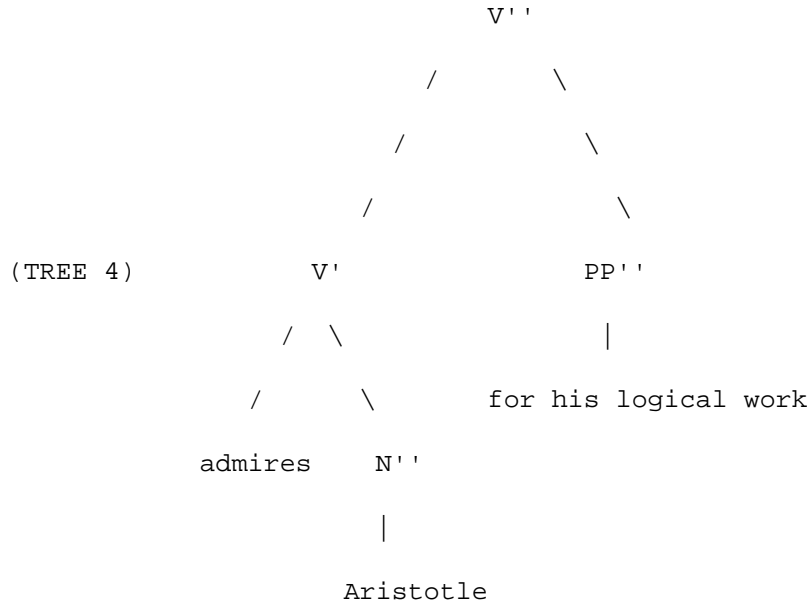
This metalinguistic analysis, among other virtues, helps explain the slight awkwardness in the placement of the final 's' in 'Sophias' in (FN 10) and also the mild infelicity created by introducing adjectival modification of the proper name, as in:

(FN 14) There are several tall Sophias in my daughter's class.

Setting aside issues about how well the proper name/determiner combinations do or do not motivate the Burgeian proposal of treating proper names as common nouns, semantic considerations raised by [Kripke 1980] confirm that proper names ought not be assimilated to the common noun category. Common nouns function semantically by providing some characteristic in virtue of which particular objects are or are not of the type singled out by the noun. Kripke's considerations, however, show that even if there were for every individual some characteristic singling out that individual, that characteristic would be no part of the semantics of the proper name -- since no knowledge of the nature of the named individual is required in order to be a fully competent user of a proper name.

(58), (59), (60), (68), (73), and (76) are even plausibly obtained via X-bar theory.

The Chomskyian, of course, continues to acknowledge all of (56)-(76) as noun phrases.<sup>42</sup> It is standard practice in formal syntax to represent structures such as:



which assume that 'Aristotle' can legitimately be treated as an N'' construction, despite not plausibly being the maximal projection of a noun. But what the Chomskyian seems forced to admit is that we don't know what the syntactic form of noun phrases really is -- and to admit that -- especially given the assumption that syntactic form determines

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<sup>42</sup>In fact, the Chomskyian has little choice, since he needs to reconcile the theoretical assumption that, say, verbs take N'' as arguments with the empirical observation that verb phrases such as 'admires Aristotle' are countenanced in natural language. More generally, the fact that the deviant (from the point of view of X-bar theory) constructions enjoy the same distribution as orthodox noun phrases makes it difficult, given the other theoretical commitments of the Chomskyian, to deny that the deviants are also noun phrases. Some maneuvering room, however, may be obtained by analyzing such apparent NPs as (67) and (74) as maximal projections of the inflection INFL and then assuming that inflections can take INFL'' as well as NP as an argument.

semantic form -- is, to paraphrase Davidson, to admit that we don't know anything.

I have focused in the above discussion of the difficulties faced by Chomskyian syntax in accommodating the full range of noun phrases. The Chomskyian, of course, is no worse off than the rest of us when it comes to the syntax of NPs. Anyone who wants a satisfactory understanding of how noun phrases function needs an account of what syntactic features delimit the class of the noun phrase, and anyone who wants an adequate syntactic theory lying behind such an account needs further a story about how this range of syntactic features forms a natural category fitting into an elegant and psychologically plausible overall account of how syntactic structures appropriate for conveying our thoughts are generated.

Ideally, an adequate syntactic theory would meet two requirements. First, it would generate all noun phrases from within the confines of a single conception of what characterizes the noun phrase. We don't want a theory which simply accounts for NPs by enumerating a grocery list of syntactic types; we want syntactic recognition of the semantic intuition that there is a core of similar function in all noun phrases. It is such a unified account of the syntactic structure of noun phrases that X-bar theory unsuccessfully attempts to give us. Second, we want that underlying syntactic conception of the noun phrase to yield a ready explanation of the diversity of form in the NP category we have been examining. We must be able to see how a single underlying mechanism can give rise to such variation in final output.



### §2.1.1.2 Semantic Difficulties in Unifying the NP Category

Matching the syntactic diversity of the NP category is an impressive semantic diversity.<sup>43</sup> While there is some vague sense that all noun phrases have in common the semantic role of serving to make the world-word connection, there appears to be no simple story to tell about how each NP goes about performing that role. In this section, I want to give some indication of the range of semantic functions performed by noun phrases, first by setting out in some detail what I take to be the major semantic divides in the NP category, and second by sketching briefly several other potential trouble spots.

- Some noun phrases give rise to object-dependent, or singular, propositions, while others give rise to object-independent, or general, propositions. Consider an arbitrary sentence using the name 'Aristotle', such as:

(79) Aristotle was the last great philosopher of antiquity.

The thought expressed by (79) -- like the thought expressed by any sentence containing the name 'Aristotle' -- is one which could not have been expressed had Aristotle not existed. 'Aristotle', then, gives rise to object dependent propositions. A noun phrase like 'the tallest man in the room', on the other hand, gives rise to object independent propositions (in the absence of other triggers for object dependence).

Thus a sentence like:

(80) The tallest man in the room is wearing a hat.

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<sup>43</sup>Determining whether the semantic diversity is a *corresponding* diversity -- a diversity, that is, which makes distinctions along the same axes singled out by the syntax of noun phrases -- will have to await the development of an adequate syntactic account which will make clear what those axes are.

expresses a thought which could still be expressed regardless of which particular individuals existed. Had the actual tallest man in the room not existed, or even had there been no men at all in the room, (80) would still express the same thought.

- Some noun phrases are referential, while others are quantificational. Roughly speaking, referential noun phrases serve merely to pick out an individual in the world, while quantificational noun phrases connect with the world by way of some property or properties. Thus 'Aristotle' is a referential noun phrase, since it does not single out the individual Aristotle by means of any of his characteristics, but directly refers to him. 'the greatest philosopher of antiquity', on the other hand, is quantificational, since it connects with the world via the satisfaction properties of the predicate 'greatest philosopher of antiquity'.

Since quantificational noun phrases connect to the world in this mediated way, it is possible for them not to pick out any determinate individual. Thus in:

(81) **Some senator** will support the bill.

there need not be any *particular* senator picked out by the NP 'some senator'. Furthermore, quantificational noun phrases can even fail to hook up successfully with the world, either accidentally, as in:

(82) **The largest prime number** is greater than 5.

or deliberately, as in:

(83) **No admirer of Bresson** dislikes *Au Hazard, Balthazar*.

- Some noun phrases pick out objects in the world by way of the semantic properties of their component parts, while other noun phrases are indifferent in their semantic behaviour to the semantic properties of

their parts. Thus a noun phrase like 'the first Postmaster General of the United States' picks out Benjamin Franklin via appeal to the referent of 'the United States', the meaning of the title 'Postmaster General', and the semantics of 'first'. On the other hand, noun phrases like 'the Holy Roman Empire' and 'Dartmouth' are notoriously indifferent to the meanings of their component parts -- the Holy Roman Empire need be neither holy, Roman, nor an empire, and Dartmouth need not be at the mouth of the Dart.<sup>44</sup>

- Some noun phrases are rigid, in that they pick out the same object regardless of the intensional context in which they are imbedded, while others pick out different objects relative to different intensional contexts. Thus, to focus on the case of modal rigidity, consider the contrast between the following the schemata:

(84) Had it been the case that  $p$ , Aristotle would have been tall.

(85) Had it been the case that  $p$ , the greatest philosopher of antiquity would have been tall.

In (84), the noun phrase 'Aristotle' picks out the same individual -- Aristotle -- regardless of the content of  $p$ . In (85), however, the noun phrase 'the greatest philosopher of antiquity' can change who it picks out as we change choice of  $p$ , as the content of  $p$  affects who, in that counterfactual circumstance, would be the greatest philosopher of antiquity. Thus 'Aristotle' is modally rigid while 'the greatest philosopher of antiquity' is not.

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<sup>44</sup>I assume here that 'holy', 'Roman', 'empire', 'Dart', and 'mouth' are in fact parts of these noun phrases, and not 'accidental' occurrences in Quine's sense, on the same level as the occurrence of 'cat' in 'catastrophe'. In fact, I suspect this assumption should not be made.

Given any type of intensional operator, we can distinguish between noun phrases rigid with respect to that intensional type and noun phrases not so intensional. Thus, for example, we can have temporally rigid and non-rigid phrases. Contrast the following:

(86) In *t*, Lyndon Johnson was a Democrat.

(87) In *t*, the president was a Democrat.

Regardless of what year we substitute for *t*, 'Lyndon Johnson' picks out Lyndon Johnson. 'the president', however, picks out different individuals with respect to different choices of *t* (at least on one reading of (87)). More generally, given any intensional type *I*, we can distinguish *I*-rigid from *I*-non-rigid NPs. Prima facie, all types of rigidity need not come as a package. Thus the NP 'the actual president of the United States' is modally rigid, picking out the same individual with respect to any possible world, but is not temporally rigid, since it picks out different individuals with respect to different times.<sup>45</sup>

- Some noun phrases pick out the same objects in the world regardless of the context in which they are used. Thus, for example, the proper name 'Aristotle' always refers to Aristotle, regardless of who is using it

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<sup>45</sup>I will suggest later, however, that the appropriate test of rigidity is not uniformity of semantic output regardless of intensional context of evaluation, but (stronger) semantic indifference or inertness with respect to intensional context of evaluation. 'the positive square root of 4', for example, does not meet this second test for rigidity, since, while it denotes the same individual regardless of world of evaluation, it does appeal to that world as world-indexed satisfaction clauses are employed -- it just turns out that the positive square root of 4 is the same in every possible world. (The distinction being drawn here is similar to that which [Kripke 1980] draws between *de jure* and *de facto* rigid expressions, although Kripke might count 'the actual president of the United States' as *de jure* rigid, even though this NP does not pass my more stringent test for rigidity.) This stronger version of rigidity makes it possible to endorse the thesis, endorsed below, that in fact any noun phrase is either rigid with respect to all intensional types or rigid with respect to none.

and under what conditions they are using it.<sup>46</sup> The indexical 'I', on the other hand, refers differently depending on who is using it. The indexical 'here' refers differently depending on where it is used, the indexical 'now' refers differently depending on when it is used, and the demonstrative 'that' refers differently depending on what is demonstrated by the speaker.

'I', 'here', 'now', and 'that' are all speaker-centered in their context-sensitivity, in that the facts which determine what object is picked out by the noun phrase are facts about the speaker employing that noun phrase. Other noun phrases exhibit what we might call a world-centered context sensitivity. Thus, for example, the phrase 'the tallest man in the room' picks out different individuals depending not on the condition of the speaker, but on the condition of the world -- specifically, on who is the tallest man in the room. Note that lack of world-centered context sensitivity is not the same thing as rigidity. The phrase 'the actual president of the United States' is modally rigid, since it picks out the same individual (Bill Clinton) with respect to all possible worlds. However, that phrase does display world-centered context sensitivity, since it picks out who it does only because of facts about the way the world is. Had Bob Dole been the president at the time of utterance, 'the actual president of the United States' would have picked him out. Interestingly, there seem to be no noun phrases which display world-centered context sensitivity but which are rigid, even though, *prima facie*, such phrases are a theoretical possibility.

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<sup>46</sup>Assuming, of course, that the speaker is using 'Aristotle' as a word of English with its normal meaning. I ignore from here on this trivial sort of context-sensitivity.

• Some noun phrases are sensitive not to the context of the speaker or the utterance, but on the linguistic context in which they are embedded. Thus, for example, in a context like:

(88) Godard directed *Weekend* after **he** directed *Alphaville*.

the fact that the noun phrase 'he' refers to Godard is determined by the lexical context -- in particular, by the earlier appearance of the proper name 'Godard' in the sentence. Thus the same pronoun 'he' in different lexical contexts can take on different referents:

(89) Michael Snow directed *Wavelength* after **he** directed <-->.

(90) Every boy read some book **he** liked.

Sensitivity to lexical context is wholly distinct from contextual sensitivity in the more usual sense -- thus 'he' in (88) refers to Godard regardless of the context of the speaker or the state of the world, while 'I' refers to the speaker of the utterance, regardless of the linguistic context in which it occurs.

In addition to these variations in the semantic behaviour of noun phrases, there are many other difficulties in delivering a unified semantic account of the category which are more minor, more contentious, or more heavily theory-laden. Thus, for example, we might distinguish between those names, like 'Aristotle', which carry no descriptive content, and those, like Evans' 'Julius', which do. We might distinguish between noun phrases like proper names and definite descriptions which allow the kind of backward-looking anaphora exhibited in:

(91) If **he** arrives in time, **Mark** can come with us.

and those like generic quantificational noun phrases which do not allow such anaphora:

(92) \*If **he** arrives in time, **some tall man** can come with us.

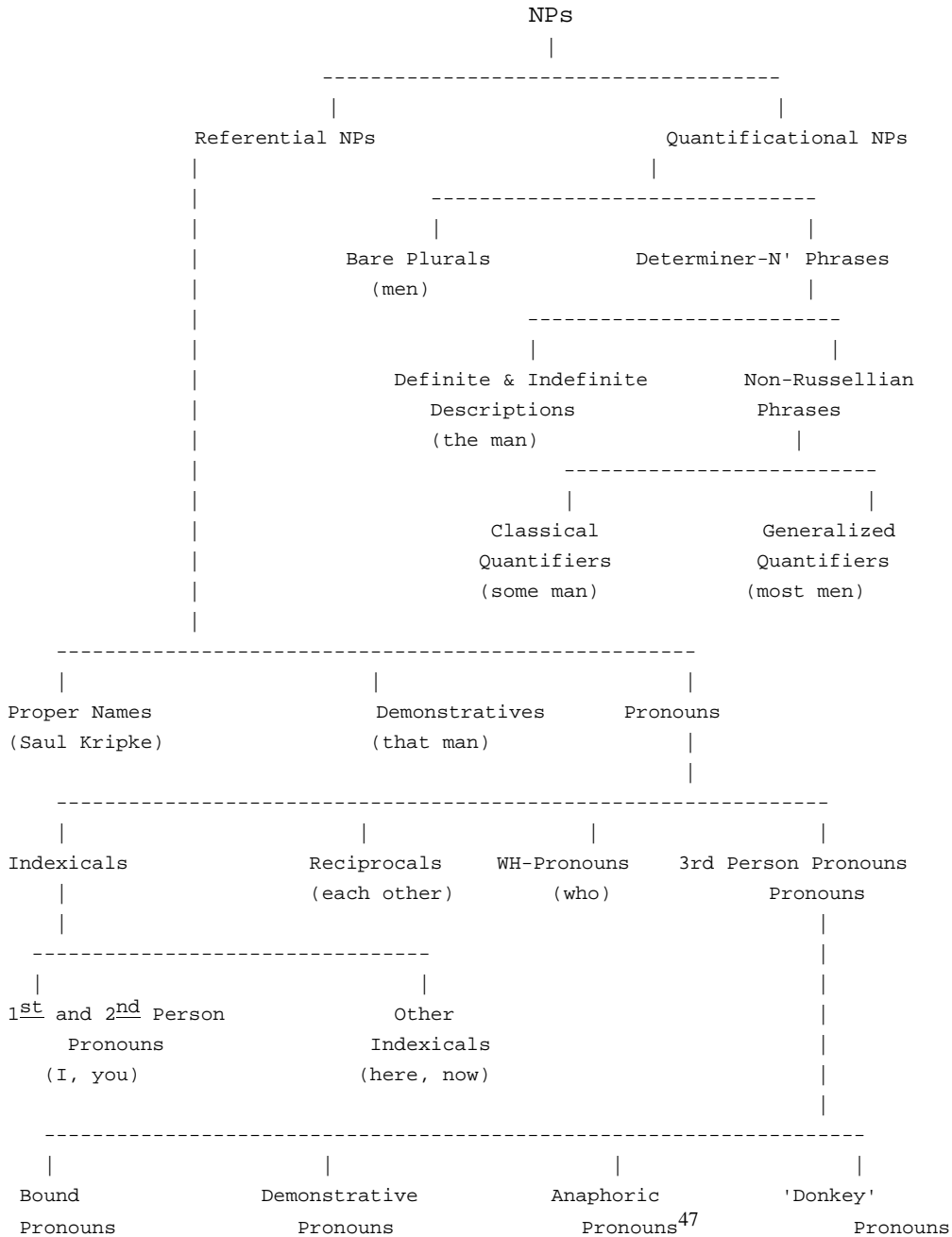
We might distinguish among noun phrases which refer to particular objects, noun phrases which refer to abstract or intensional entities, and noun phrases which refer plurally to several objects. We might distinguish, following [Hornstein 1984], between quantificational noun phrases formed using 'a certain' and 'any', which always take wide scope, and those formed using other determiners, which can take wide or narrow scope. We might distinguish between noun phrases like indefinite descriptions which purportedly are not bound by certain island constraints, and other quantificational noun phrases which are so bound. And so on.

As do the syntactic divisions in the NP category, the semantic divisions present the theorist with two difficulties. First, we must develop some account of the semantic function of noun phrases which explains how all these diverse semantic phenomena can be the result of a single coherent semantic type. Second, we must be able to show how that single underlying story gives rise in a non-ad-hoc way to the full range of phenomena exhibited by natural languages.

### **§2.1.2 Preliminary Remarks on a Taxonomy of Noun Phrases**

The syntactic and semantic difficulties set out in the previous two sections have, of course, not escaped the attention of those thinking about natural languages. Much work has been devoted to determining how different types of noun phrases relate to one another and to what extent different types of noun phrases can be seen as natural manifestations of core underlying semantic features. As a result of this work, some degree of consensus on a rough taxonomy of noun phrases has emerged. While there is of course no universal agreement

about the details of such taxonomy, one can with some confidence endorse a hierarchical structure such as:



<sup>47</sup>Pronouns, that is, which are anaphoric on earlier referential NPs, as in:

(F15) Hitchcock wasn't a nice man. **He** called his actors cattle.

(F16) This is my favorite scene. **It** unveils the birds so effectively.



This taxonomy is certainly not exhaustive<sup>48</sup>, and it can of course be made either more or less fine-grained<sup>49</sup>, but it is adequate to provide an interesting launching point for conversations on the NP.

There are two aspects of this taxonomy to which I wish to draw attention. The first is its usefulness in coming to grips with certain properties of noun phrases. Take, for example, the property of rigidity. As noted above, some NPs are rigid -- their semantic value (their reference or denotation) is unaffected by the possible world (time, place, etc.) with respect to which they are evaluated. Looking at our taxonomy, we discover that the rigid NPs are all and only those on the 'referential' side of the primary dichotomy.<sup>50</sup> Similarly, the NPs which are context-sensitive in their extension -- the indexicals, demonstratives, and demonstrative pronouns -- are neatly shepherded into their own taxonomic slots.

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<sup>48</sup>The taxonomy given here omits entirely (a) gerundive phrases like 'reading *Naming and Necessity*', (b) infinitive phrase such as 'to have loved and lost', and (c) 'that' clauses such as 'that Frege wrote the *Begriffsschrift*', all of which can occupy the syntactic positions characteristic of noun phrases. Discussion of all these three types of noun phrases lie outside the scope of this work. There are other smaller categories which are or may be omitted from the above list. Thus, for example, apparently determiner-N' structures such as 'only tall men' and 'even linguists' may not fit into the category of quantificational noun phrases where other determiner-N' structures reside, and pleonastic pronouns don't fit into any of the pronominal categories given. 'Only' and 'even' are discussed briefly in §3.3.1.3.2 below.

<sup>49</sup>For finer grain, we might, for example, distinguish complex from simple demonstratives, complete from incomplete definite descriptions, or monotone increasing from monotone decreasing quantifiers. For coarser, see below.

<sup>50</sup>I am adopting here the stricter definition of rigidity set out in footnote 44 above, which rules out certain quantificational NPs, such as 'both square roots of 4' or 'the actual president of the United States' which would be ruled rigid under the weaker definition. I also overlook (a) difficulties in making sense of the notion of rigidity as applied to bound pronouns and (b) apparent non-rigid behaviour of 'donkey' pronouns, as exhibited in the true reading of:

(FN 17) The president is a Democrat, but in 1987 he was a Republican.

Interactions between donkey pronouns and intensional operators are discussed in §2.2.2.2.2 below.

Ideally, of course, the taxonomy will reveal not just *that* semantic properties are distributed among noun phrases in a certain pattern, but also *why* those semantic properties are so distributed. Thus, in addition to knowing that the referential, but not the quantificational, NPs are rigid, it would be nice to know that some feature of referentiality gives rise to rigidity. Here we might appeal to the kind of story told by fans of direct reference, on which since the sole semantic property of the referential term is its referent, there is no semantic ability to interact with intensional operators in a way which could yield varying results. The explanatory project would then be pushed back a stage, and we could ask why referential and quantificational features are distributed among noun phrases in the way that they are. In the end, an appropriate taxonomy for noun phrases should lead to an accompanying semantic theory which distributes semantic features along the paths of the taxonomic hierarchy, and explains why the nodes of the hierarchy have the semantic relevance that they do.

The second point about the taxonomy is that we can, in many cases, now collapse some of the distinct categories into a more general scheme. Thus, for example, the distinction between definite descriptions and (classical) non-Russellian quantificational NPs is, in the light of Russell's theory of descriptions, no longer necessary, since both can be subsumed under a more general theory of restricted quantification. Similarly, the distinction between classically quantified NPs and generalized quantifier NPs can be eliminated in favor of a Mostowski-Barwise-Cooper style theory of generalized quantifiers which unifies the two categories.

The possibility of such categorical collapses, of course, does not show that the original distinctions were not real distinctions. There are important differences between definite descriptions and other quantificational noun phrases (although, appropriately, they may be more differences of degree than of kind) -- definite descriptions, for example, give rise to singular propositions on the level of speaker's meaning much more readily than other quantificational noun phrases. Similarly, there are legitimate distinctions between classical and more broadly Mostowskian quantificational noun phrases. Classically quantified noun phrases admit a complete deductive system and allow for easy expressability in unrestricted notation, while generalized quantified noun phrases do neither. Such differences between the newly collapsed categories must be both respected and explained by the reductive theory. Thus a Russellian account of definite descriptions must go hand-in-hand with, say, an account of pragmatics which can appeal to certain formal features of the 'the' determiner (perhaps its existence and uniqueness implications) to explain the greater tendency toward object-dependent discourse associated with definite descriptions. A general Mostowskian account of quantificational noun phrases must similarly be able to identify those features of the classical quantifiers which make them axiomatizable or expressible in unrestricted notation.<sup>51</sup>

There are, however, many distinctions in the taxonomy which do not currently subsume into any broader semantic theory. The distinction between referential and quantificational noun phrases, for example, is

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<sup>51</sup>See §3.2.1.3.2.1 below for a response to the second of these two challenges to the fan of generalized quantifiers.

fundamental on current semantic theory -- there is no larger context which shows these two types to be special cases of a higher genus. Similarly, although we can point to some strands of semantic connection between the various species of referential NPs -- the proper names, demonstratives, and indexicals -- we have no unified account of what semantic features serve to make all of these into referring expressions.

Ultimately, a taxonomy of noun phrases ought to lead to a theory of noun phrases in the following way: we should have a single syntactic mechanism which generates all noun phrases in natural language. There will be various parameters of implementation in that syntactic mechanism, choice of which will allow production of the full variety of noun phrases. This syntactic mechanism will yield naturally a taxonomy of noun phrases. We should then have a single semantic mechanism sensitive to the differences in syntactic structure caused by variation in choice of syntactic parameter. That single semantic mechanism will allow for interpretation of any noun phrase in our taxonomy, but will (a) show that certain semantic properties are grouped in certain regions of the taxonomy and (b) show why these particular regions give rise to the particular semantic features they possess. The production of a fully adequate such taxonomy, of course, is an enormous task, and not one which will be carried out here. The rest of this chapter, however, will be devoted to attempting to show that adoption of the anaphoric account of variable binding set out in the previous chapter enables us to make substantial forward progress toward that goal.

### **§2.1.3 Noun Phrases, Quantification, and Variables**

At the end of the previous chapter, I indicated that the traditional distinction between free and bound variables would, under my system of anaphoric binding, have to be replaced by a four-fold categorization of (i) anaphorically bound and distributed variables, (ii) anaphorically bound but undistributed variables, (iii) distributed but not anaphorically bound variables, and (iv) neither anaphorically bound nor distributed variables. Of these four categories, I suggested that the third, due to the nature of the system, was incoherent or useless. My goal in this chapter will be to show that the three remaining categories yield the right starting point for a proper taxonomy of noun phrases, and thus that moving from classical quantification to quantification understood by way of anaphoric variable binding enables us to obtain a fully general theory of noun phrases in natural language.

The first of my three (non-empty) categories of variable configurations is that of the anaphorically bound and distributed variable. This configuration corresponds closely to the traditional restricted quantifier, and here I will rely heavily on earlier work (stemming largely from [Barwise & Cooper 1981]) which shows that restricted quantification is a useful syntactic and semantic framework for understanding quantified noun phrases of the determiner-N' syntactic form. There are important differences between the anaphoric account of variable binding and previous work in restricted quantification, and in chapter 3, as we investigate in more detail the minutiae of the anaphoric account we will see how some of these differences are relevant to the resulting account of quantified noun phrases. For the purposes of this chapter, however, I will assume that my variable configuration (i)

provides a good analytical tool for the determiner-N' quantificational noun phrase.

Our task in this chapter, then, will be to show that the other two configurations -- the anaphorically bound but undistributed variable and the variable neither anaphorically bound nor distributed (which I will call from here on the free variable) -- serve to account for the full array of noun phrases falling outside the determiner-N' structure.

### **§2.1.3.1 Variables in Natural Language**

Before starting to defend the utility of my anaphoric account of variable binding for understanding noun phrases, however, something should be said about variables in natural languages. My claim will be that all noun phrases in natural language should be understood as variables with varying degrees of logical operation (specifically, distribution or anaphoric binding) applied to them. Clearly, then, I must hold that there are variables in natural languages. The question to be answered is where these variables are.

#### **§2.1.3.1.1 Pronouns and the Naive Variable Theory**

My answer to this question is what I will call the Naive Variable Theory (NVT). According to the NVT, all pronouns in natural language are variables.<sup>52</sup> As this stands, this is a rather bold claim, and it will take us much of this chapter to even approach a full defense of the NVT.

That there is *some* connection between pronouns and variables is hardly a daring position to take. [Quine 1960], of course, explicitly analogizes variables to pronoun. Furthermore, the practice of

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<sup>52</sup>Or, if one prefers, that pronouns play the role in natural languages that variables play in formal languages, and thus that in the formal regimentation of natural into formal languages, pronouns are mapped into variables.

regimentation of natural languages sentences into formal languages carried out in any introductory logic course reveals an intuition that variables and pronouns are related. Thus, for example, a formal regimentation of:

(93) Every girl read the book **she** liked best.

into restricted quantifier notation yields:

(93') [every x: boy x][the y: girl y & y liked-best x]  
danced x,y<sup>53</sup>

Here the pronoun 'she' in (93) is mapped into the bound variable 'x' in (93'). It is common practice for logic textbooks to employ variables in this manner to capture the effects of pronominal devices. Picking three textbooks with substantially different philosophical agendas, we find in [Mates 1965]:

(11) There is an integer that is greater than every integer other than **itself**.  
( $\exists y)(x)(-Ixy \rightarrow Lxy)$  [73, emphasis added]

And in [Lambert & Van Fraassen 1972]:

13. Every man likes **himself**.  
... 13d.  $(x)(x \text{ is a man} \supset x \text{ likes } x)$  [76-77, emphasis added]

And finally in [Barwise & Etchemendy 1992]:

No dodecahedron has anything in back of **it**.  
[with its implicit translation:  $\neg(\exists x)(\text{Dodec}(x) \ \& \ (\exists y)(\text{BackOf}(y,x)))$ ] [161, emphasis added]

However, there are at least two important reasons to be hesitant about taking this connection between variables and pronouns as evidence for the NVT, reasons which must be rebutted before my project can go forward successfully.

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<sup>53</sup>There is a legitimate question here about the status of the variables in the formal regimentation (93') which do not correspond to pronouns in (93). This question is taken up in more detail in §2.1.3.1.2 below.

First, the above cases cover only a single species of the rather broad genus of the pronoun. Pronouns are a diverse class, including at least the following:

- Third-person singular pronouns: he, she, it, one, him, her etc.
- Second-person singular pronouns: you, your, yourself
- First-person singular pronouns: I, me, mine, my, myself
- Plural pronouns: we, you, they etc.
- WH-words: who, which, where, when, etc.
- Mutuality pronouns: each other

Of these, only the third-person singular pronouns (3P) are clearly shown by the examples given above to have *some* relation to variables. If the NVT is to stand, then, we must at some point argue that other types of pronouns should also be given the same treatment. The bulk of the work along those lines will be done in §2.3. Here, in order to show that all pronouns are variables, I will first cast my net even wider, looking more generally at the category of the singular term. I will argue that singular terms are themselves subject to a unified analysis once the resources of the anaphoric account of variable binding are brought to bear. The context provided by this larger unificatory project will then, coupled with some considerations on indexicality and context sensitivity in §2.3.3, allow us to see how the smaller category of the pronoun can successfully be identified with the category of the variable.

For our current purposes, however, we can retreat to what we might call the Weakened Naive Variable Theory (WNVT). This position holds only that all third-person singular pronouns are variables. A successful defense of the WNVT would at least justify us in the claim that some natural category in natural languages plays the role of variables.



However, even the WNVNT is a stronger position than most are willing to take. While it is true that many uses of 3P pronouns mimic the behaviour of bound variables, there are at least three important types of usage of these pronouns which *prima facie* do not sit well with the WNVNT.

- First, there are the so-called 'deictic pronouns', in which there is no antecedent (and hence no opportunity for a binding operator) for the pronoun. For example, in the sentence:

(94) **He** liked Pulp Fiction even more than I did.

accompanied by a nod of the head in Herman's direction, the pronoun 'he' is not in any immediately obvious way analogous to the variables of formal languages.

- Second, some pronouns are anaphoric directly on referring expressions. In sentences like:

(95) Hitchcock has a cameo appearance in each of **his** films.

(96) That looks like a blimp. **It**'s going down in flames.

we cannot treat 'his' or 'it' as bound variables, since their anaphors 'Hitchcock' or 'that' are not quantifiers which could bind variables.

- Third, instances of 'donkey' or cross-clausal anaphora have of late received much attention. In the canonical example:

(97) Every man who owns a donkey vaccinates **it**.

the final pronoun 'it' is thought to require some treatment more sophisticated than the simple bound 'she' of (93) above, since here the pronoun falls outside the scope of the quantifier 'a donkey'; a straight-forward attempt to employ the tactics of quantificational logic yields the open formula:

(97') [every x: man x & [a y: donkey y] owns x,y] vaccinates x,**y**

with its unbound final 'y'.

I will attempt in this chapter to address these three problem cases for the WNVT. In §2.2.1 below, I will suggest that cross-clausally anaphoric pronouns can be treated as a type of bound variable by the anaphoric account of variable binding, and in §2.3.1 I will show that deictic pronouns and pronouns anaphoric on proper names can both be treated as free variables.

#### **§2.1.3.1.2 Pronouns, Traces, and Variables**

In the previous section, I appealed to schematizations such as that of:

(93) Every girl read the book **she** liked best.

as:

(93') [every x: boy x][the y: girl y & y liked-best x]  
danced x,y<sup>54</sup>

for evidence backing the claim that natural language pronouns ought to be treated as variables. In doing so, I implicitly raised questions about the relation between the *other* variables in (93') and the structure of (93). In this section I want to answer those questions by way of a detour through some issues in formal syntax. In the process, we will collect further evidence for the plausibility of the WNVT.

#### **§2.1.3.1.2.1 Variables and Traces**

In Government and Binding (GB) theory, as developed in [Chomsky 1981, 1982], there are three levels of syntactic representation associated with a sentence: deep structure (DS), surface structure (SS), and logical form (LF). Roughly speaking, deep structures are generated via X-bar theory. These deep structures then give rise to surface

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<sup>54</sup>There is a legitimate question here about the status of the variables in the formal regimentation (93') which do not correspond to pronouns in (93). This question is taken up in more detail in §2.1.3.1.2 below.

structures by way of movement of various nodes in the phrase tree -- movements permitted by the general rule  $\text{move-}\alpha$  and induced by considerations imposed by case theory and by language-specific parameters. Surface structures -- the syntactic level which gives rise to phonetic and graphological realizations of sentences -- then give rise to logical forms. Logical forms are the syntactic structures well-suited for semantic interpretation, and are obtained from surface form through another series of applications of  $\text{move-}\alpha$ , this time intended to clarify syntactic issues bearing on semantics, such as quantifier scope.

I want to focus here on the relation between surface structure and logical form.<sup>55</sup> Consider a sentence such as:

(98) Every boy read some book.

which will have a surface form something like:

(98-SS) [<sub>S</sub> [<sub>NP</sub> every boy] [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> some book]]]

(98) is ambiguous in English, having one reading on which the universal NP has wide scope and one reading on which the existential NP has wide scope. We thus want there to be two logical forms associated with (98), one for each reading:

(98-LF1) [<sub>S</sub> [<sub>NP</sub> every boy] [<sub>S</sub> [<sub>NP</sub> some book] [<sub>S</sub> ... ]]]

(98-LF2) [<sub>S</sub> [<sub>NP</sub> some book] [<sub>S</sub> [<sub>NP</sub> every boy] [<sub>S</sub> ... ]]]

These two logical forms are obtained by applying  $\text{move-}\alpha$  to the two noun phrases in (98-SS). In each case, the noun phrase is moved to the head of a new sentence node dominating the original sentence; by choosing the

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<sup>55</sup>I will suggest in the next section that there is no need for an additional level of deep structure in an adequate syntactic theory.

order in which we apply move- $\alpha$  to the two noun phrases we can obtain either (98-LF1) or (98-LF2) as the final result.<sup>56,57</sup>

I have left unspecified above the contents of the smaller-scoped S-nodes in (98-LF1) and (98-LF2). Clearly these S-nodes no longer contain the NPs 'every boy' and 'some book'. However, one central

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<sup>56</sup>According to GB, movement is to the head of the sentence because (a) the noun phrase being moved already has a thematic role assigned to it, (b) no node can receive more than one thematic role assignment, (c) there is no thematic role assignment to the syntactic position of an NP c-commanding an S-node, and (d) all other syntactic positions in the sentence have thematic role assignments induced by other elements of the sentence.

<sup>57</sup>GB typically distinguishes several types of move- $\alpha$  applications between surface structure and logical form. Thus, for example, [Sells 1985] lists the following types of movement:

The set of possible movements is:

- movement to [NP, S] position (NP-movement)
- adjunction to COMP (wh-movement/wh-construal)
- adjunction to S (QR)
- adjunction to VP [47]

It is unclear to me that such distinctions are necessary. If we treat wh-phrases as a species of NP (as, it seems to me, we should), then there is no reason to treat wh-movement as adjunction to COMP rather than adjunction to S as in the standard quantifier raising (QR) case. VP adjunction can also be seen as a variant of adjunction to S in which the linear ordering, but not the hierarchical structuring, of the nodes is inverted. Movement to [NP, S] position is a somewhat more difficult case, and involves tricky issues concerning passivization. The standard GB analysis of passives is that one begins with a DS in which one argument place -- the subject position -- is empty, and the verb is marked with a passive inflection. The passive inflection removes the ability of the verb to assign case, and thus the case filter forces the argument of the verb to move to a cased position, of which the only open one is the empty subject position:

(FN 18) [S [NP  $\emptyset$ ] [VP hit<sub>p</sub> [NP Bob]]

(FN 19) [S [NP Bob] [VP hit<sub>p</sub> [NP t]]

Were we to reduce NP-movement to the QR paradigm, we would have to assume that the object of the passive verb actually moved to a position dominating the original S-node:

(FN 20) [S [NP Bob] [S [NP ???] [VP hit<sub>p</sub> [NP t]]]

but such movement would raise problems about the content of this subject position of the original S-node. I find the GB treatment of passives not entirely satisfactory in any case -- the inability of the verb to assign case in the presence of the passive inflection seems ad hoc, the ability of (FN 19) to interact well with a semantic theory seems suspect, and the use of the case filter to induce movement of the (caseless) argument of the verb sits poorly with the movement of only one argument in passive ditransitive constructions such as:

(FN 21) Bob was given a book.

In any case, my comments in the main text on the connections between pronouns, traces, and variables stand whether move- $\alpha$  takes on one form or many in the surface-structure/logical-form interaction.

principle of GB theory, the Projection Principle, requires that any syntactic position occupied at any structural level of a sentence must be occupied at every structural level of that sentence. Thus, since the surface form of (98) has noun phrases in the primary S-node, these noun phrases must still exist in the logical forms (98-LF1) and (98-LF2).<sup>58</sup> However, since there is no plausible linguistic material to fill these nodes, we must introduce the idea of a trace, or an empty category. The idea here is that the application of move- $\alpha$  leaves behind the node from which the noun phrase is moved, but leaves that category empty, or filled with a phonetically null trace of the movement. The proper completion of the logical forms above, then, is:

(98-LF1) [<sub>S</sub> [<sub>NP</sub> every boy]<sub>1</sub> [<sub>S</sub> [<sub>NP</sub> some book]<sub>2</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub> [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> t<sub>2</sub>]<sub>2</sub>]]]]]

(98-LF2) [<sub>S</sub> [<sub>NP</sub> some book]<sub>1</sub> [<sub>S</sub> [<sub>NP</sub> every boy]<sub>2</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>2</sub>]<sub>2</sub> [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub>]]]]]

Note here the introduction of subscripted indexing to correlate the prefixed noun phrases with the traces they have left behind.

Traces, in addition to be required to satisfy the Projection Principle, also have observable implications in surface form, as shown by the well-known example of 'wanna' contraction. According to GB theory, in English some wh-phrases are required to move from their

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<sup>58</sup>Similarly, since the logical forms have additional S and NP nodes sitting above the S node of the original surface form, these nodes must in fact already have been present in the surface (and deep) form. Thus the correct surface form for (98) is not (98-SS) but:

(98-SS') [<sub>S</sub> [<sub>NP</sub>  $\emptyset$ ] [<sub>S</sub> [<sub>NP</sub>  $\emptyset$ ] [<sub>S</sub> [<sub>NP</sub> every boy] [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> some book]]]]]]]

The oddity of building these empty categories into the deep structure gives, I think, additional credence to my suggestion of the next section that syntactic generation begins with logical form and moves to surface form.

initial position in DS to the head of the sentence in SS. Thus the question corresponding to:

(99) I talked to Bill.

is not:

(100) \*I talked to whom?<sup>59</sup>

but rather:

(101) Whom did I talk to?

Now consider the following pair of sentences:

(102) I want to invite John to the party.

(103) I want Bill to invite John to the party.

and their corresponding question forms:

(102-Q) Whom do you want to invite to the party?

(103-Q) Whom do you want to invite John to the party?

Note that (102-Q), but not (103-Q), allows colloquial contraction of 'want to' to 'wanna':

(102-Q') Whom do you wanna invite to the party?

(103-Q') \*Whom do you wanna invite John to the party?

The proposed explanation for this difference in behaviour is that

(102-Q) and (103-Q) begin with the following deep structures:

(102-Q-DS) [<sub>S</sub> [<sub>NP</sub> you] [<sub>VP</sub> [<sub>V</sub> want] [<sub>S</sub> [<sub>V</sub> to invite] [<sub>NP</sub> whom]  
[<sub>ADVP</sub> to the party]]]]]

(103-Q-DS) [<sub>S</sub> [<sub>NP</sub> you] [<sub>VP</sub> [<sub>V</sub> want] [<sub>S</sub> [<sub>NP</sub> whom] [<sub>VP</sub> [<sub>V</sub> to invite]  
[<sub>NP</sub> John] [<sub>ADVP</sub> to the party]]]]]

Since the wh-phrases are required to move in surface structure, we obtain:

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<sup>59</sup>Although this formation is acceptable with appropriate contrastive stress.

(102-Q-SS) [S [NP whom]<sub>1</sub> [S [NP you] [VP [V want] [S [V to invite]  
[NP t<sub>1</sub>]<sub>1</sub> [ADVP to the party]]]]]]

(103-Q-SS) [S [NP whom]<sub>1</sub> [S [NP you] [VP [V want] [S [NP t<sub>1</sub>]<sub>1</sub> [VP  
[V to invite] [NP John] [ADVP to the party]]]]]]

Now note that in (103-Q-SS), there is a trace between 'want' and 'to'. The claim then is that this trace, even though it itself has no phonetic realization, blocks the contraction of 'want' and 'to' by placing an obstacle between them.

According to GB, then, there are phonetically unrealized bits of syntax called traces. Moreover, these traces, especially on the level of logical form, seem to be playing the same role that we would expect variables to be playing. Thus compare the following pairs of logical form and regimentation in restricted quantifier notation of the two readings of (98) above:

(98-LF1) [S [NP every boy]<sub>1</sub> [S [NP some book]<sub>2</sub> [S [NP t<sub>1</sub>]<sub>1</sub> [VP [V  
read] [NP t<sub>2</sub>]<sub>2</sub>]]]]]]

(98-RQ1) [every x: boy x][some y: book y][x read y]

(98-LF2) [S [NP some book]<sub>1</sub> [S [NP every boy]<sub>2</sub> [S [NP t<sub>2</sub>]<sub>2</sub> [VP [V  
read] [NP t<sub>1</sub>]<sub>1</sub>]]]]]]

(98-RQ2) [some x: book x][every y: boy y][y read x]

There are clear structural analogies between the two pairs. The variables and traces both play the role of indicating which argument position the quantified noun phrase, dislocated from argument position in order to indicate scope ordering, is associated with. It is quite plausible to think that an adequate semantic theory for natural language will process logical forms of the type given above in much the same way

that the semantics for a restricted quantificational formal language processes (98-RQ1), (98-RQ2) above.

#### **§2.1.3.1.2.2 Traces and Pronouns**

The NVT suggests that pronouns play the role of variables in natural language; the considerations of the previous section suggest that it is traces which play that role. I now want to attempt to tie together these two lines of thought by suggesting that pronouns and traces are the same thing.

That they are the same is, I think, supported by the following kind of consideration. Just as the surface structure:

(98-SS) [S [NP every boy] [VP [V read] [NP some book]]]

corresponds to the logical form:

(98-LF1) [S [NP every boy]<sub>1</sub> [S [NP some book]<sub>2</sub> [S [NP t<sub>1</sub>]<sub>1</sub> [VP [V read] [NP t<sub>2</sub>]<sub>2</sub>]]]]]

with a single trace, it would seem that the surface structure:

(99-SS) [S [NP every boy] [VP [V read] [NP some book [S, that [NP he] [VP liked]]]]]]]

should correspond to the logical form:

(99-LF) [S [NP every boy]<sub>1</sub> [S [NP some book [S, [that [NP t<sub>1</sub>]<sub>1</sub> [VP liked]]]]<sub>2</sub> [S [NP t<sub>1</sub>]<sub>1</sub> [VP [V read] [NP t<sub>2</sub>]<sub>2</sub>]]]]]

especially if the logical form is to mirror the obvious schematization into restricted quantifier notation:

(99-RQ) [every x: boy x][some y: book y & x liked y][x read y]

The trace common to both (98-LF1) and (99-LF) seems to play exactly the same semantic function as the pronoun in (99-SS): to relate the choice of boy induced by the 'every boy' quantified noun phrase to some book



liked by *that boy*. *Prima facie*, then, I am inclined to take (99-LF) as the appropriate logical form for (99-SS), and thus take the pronoun to be just a trace with a (as yet unexplained) phonetic form.

It is not, however, standard GB practice to take pronouns as a species of trace, although it is acknowledged that there is some commonality of function between the two. Furthermore, the GB theorist would seem to have a rather damning argument against seeing pronouns such as that in (99) as traces. The problem is that there is nothing for the 'he' in (99) to be the trace of. Traces are created when noun phrases are moved out of their deep structure position by move- $\alpha$ , but in (99) there is nothing which has been so moved, so 'he' cannot be the trace of a movement.<sup>60</sup> Thus the pronoun must have been directly inserted in deep structure ('base-generated'), showing that it is a different kind of thing from a trace.

In order to respond to this argument, we have to perform a substantial reworking of the GB framework. On the GB picture, as mentioned earlier, sentential formation begins through generation of a deep structure (through iterated appeal to the X-bar format) and then proceeds via transformation of that deep structure first into a surface structure and then finally into a logical form. Psychologically, however, this picture of sentence formation is unrealistic. Given that logical form is intended to be the level of syntax suited for semantic interpretation, it is hard to see how a speaker determines *which* deep structure to produce. The speaker certainly cannot, as one might

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<sup>60</sup>This problem will be made even sharper when we consider, as we do in §2.3.1 below, the status of deictic pronouns, as in:

(FN 22) He whistled.

which clearly cannot be traces of any movement.

suspect, begin by having a thought he wishes to express and then form a sentence well-suited to the expression of that thought, for he will not be in a position to determine if any given deep structure captures the content of his thought. One is left with a clearly unacceptable picture of language generation: a deep structure is formed at random, transformed into a surface structure which the speaker then speaks (by way of a phonetic form), and finally transformed into a logical form, at which point the speaker can finally come to know what it is he has said.

For a more realistic picture of language production, we might assume that speakers begin by producing not deep structures, but logical forms. The idea here would be that a speaker begins with some thought he wishes to convey. Since logical forms are well-suited to semantic interpretation, the speaker can take the semantic properties of his thought and straightforwardly implement them as a logical form.<sup>61</sup> This logical form is then transformed into a surface structure, using the same movement rules appealed to in the standard GB story about the transformation of SS into LF, but in reversed direction. The level of deep structure is then jettisoned entirely as unnecessary.

If, for example, a speaker wishes to express the thought that every boy read some book (understood with the existential having wide

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<sup>61</sup>The generation of LFs might proceed something like this: the speaker identifies the objects he wishes to speak about (or the identifying features of the unknown objects he wishes to speak about, for object-independent assertions) and the relation he wishes to specify among those objects. He then picks out lexical items suitable for those objects and that relation, and using the X-bar schema generates noun and verb phrases around those lexical heads. He then situates the newly generated phrases in a sentential frame in such a way that the appropriate semantic properties are captured, thus creating a logical form

scope), he generates through reverse application of the semantic interpretative procedures for logical forms the LF:

(98-LF2) [<sub>S</sub> [<sub>NP</sub> some book]<sub>1</sub> [<sub>S</sub> [<sub>NP</sub> every boy]<sub>2</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>2</sub>]<sub>2</sub> [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub>]]]]]

which has the correct propositional content. Move- $\alpha$  is then employed to move the existential and universal noun phrases from their initial adjoined positions to the interior of the innermost S-node, thus filling the empty categories governed by these NPs in LF. The result is the usual surface structure for the sentence 'every boy read some book':

(98-SS) [<sub>S</sub> [<sub>NP</sub> every boy] [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> some book]]]<sup>62</sup>

Here the speaker starts with a semantically unambiguous string and only later, due to the formal limitations of surface structure, arrives at an ambiguous string.

On this view, what GB calls 'traces' will now be not remnants of moved nodes, but potential landing spots for nodes in LF. These 'traces' will clearly be playing the role of variables. Furthermore, there will no longer be any reasonable bar to equating pronouns with traces/variables. If we consider a sentence such as:

(99) Every boy read some book that he liked.

with its corresponding logical form and surface structure:

(99-LF) [<sub>S</sub> [<sub>NP</sub> every boy]<sub>1</sub> [<sub>S</sub> [<sub>NP</sub> some book [<sub>S</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub> [<sub>VP</sub> liked]]]]]<sub>2</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub> [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> t<sub>2</sub>]<sub>2</sub>]]]]]

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<sup>62</sup>More properly, the surface structure of (98) will contain empty categories where the quantified noun phrases initially resided:

(98-SS') [<sub>S</sub> [<sub>NP</sub>  $\emptyset$ ] [<sub>S</sub> [<sub>NP</sub>  $\emptyset$ ] [<sub>S</sub> [<sub>NP</sub> every boy] [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> some book]]]]]

in order to satisfy the Projection Principle. It remains to be seen, of course, whether the Projection Principle serves any useful role on this reconception of GB theory.

(99-SS) [<sub>S</sub> [<sub>NP</sub> every boy] [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> some book [<sub>S</sub> that [<sub>NP</sub> he] [<sub>VP</sub> liked]]]]]]

we will no longer have to ask what movement the pronoun 'he' is a trace of and thus what movement the putative trace of the second occurrence of 't1' is a trace of. The two occurrences of 't1' will be differentiated in this approach only in that one of them -- the first -- is moved onto by a noun phrase, while the other is not. This difference is the inevitable result of the fact that (99-LF) has three bound variables and only two binding quantifiers. Given that quantification theory gives us no reason to doubt that a single quantifier can bind more than one variable, there is no reason to be surprised that such a superfluity of traces/variables can arise. The second occurrence of 't1', which is never covered by an NP in the move from LF to SS, persists into surface form and is pronounced -- as a pronoun.

The new picture, then, is that we begin with logical forms which employ a quantifier-bound variable structure, with the bound variables playing the syntactic role played by traces under standard GB theory. In the move from LF to SS, the quantificational NPs are lowered into the primary sentential node, covering some but not necessarily all of the bound variables. Those variables which are not moved onto survive and surface form and emerge in phonetics and graphology as pronouns. Variables, traces, and pronouns, then, are all the same phenomenon, and all ground the use of a theory of variable binding in analyzing the structure of noun phrases in natural language.<sup>63</sup>

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<sup>63</sup>The GB account of traces and empty categories, and consequently the prospects of the reworking of GB theory proposed here, is obviously considerably more complicated than indicated in the main text. GB theory imposes a four-fold taxonomy on empty categories. Two potential features of traces (and of other NPs) drive this taxonomy: the feature of being

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anaphoric and the feature of being pronominal. A trace is called anaphoric if it must have an antecedent and pronominal if it optional receives an antecedent. (Intuitively, third person pronouns are pronominal since, while they can be bound by earlier NPs, they can also appear deictically. Reflexive pronouns are anaphoric since they can only appear when accompanied by an antecedent). These two features give rise to four categories: those traces which are anaphoric and pronominal (+a,+p), those which are anaphoric but not pronominal (+a,-p), those which are pronominal but not anaphoric (-a,+p), and those which are neither anaphoric nor pronominal (-a,-p).

The two features of being anaphoric and being pronominal are then exploited by binding theory, which will place limits on the distribution of the various elements of the taxonomy of traces. The three clauses of binding theory are:

(A) An NP which is anaphoric must be bound under government.

(B) An NP which is pronominal must be free under government.

(C) An NP which is neither anaphoric nor pronominal is free.

Here an NP is bound under government if the first NP or S node which c-commands the original NP but is not c-commanded by it is coindexed with that NP. The further claim is that different types of movement processes create different types of traces, and thus different types of distributions of trace appearance. In particular, we have the following four categories of trace:

(i) NP movement, of the sort associated with QR, will create anaphoric (+a,-p) traces, which will be called NP-traces.

(ii) Wh-movement will create free (-a,-p) traces, which will be called wh-traces.

(iii) Those languages which, unlike English, do not require pronominal specification of all verbal arguments, will create pronominal (-a,+p) traces, which will be called 'pro'. Thus, for example, the Greek:

(FN 23) παιδευει

will be understood as having the surface structure:

(FN 23-SS) [<sub>S</sub> [<sub>NP</sub> pro] [<sub>VP</sub> παιδευει]]

(iv) What used to be called equi-NP deletion, in which infinitival complements in which the actor is the same as the subject of the sentence omit lexical specification of the actor:

(FN 24) Mary wants to go home.

as opposed to:

(FN 25) Mary wants Bob to go home.

will create pronominal and anaphoric (+a,+p) traces which will be called 'PRO'.

If all of this is incorrect, then my identification of traces and variables will need to be modified considerably, at least if the semantic story about variables set out in the first chapter is to be maintained. Also, the easy connection between variables and pronouns will be endangered, since we will need to distinguish between reflexive pronouns, which must be governed by their binder, and nonreflexive pronouns, which cannot be so governed. However, I admit to some skepticism about the details.

There are two important points here. First, it is not clear that the variations in binding behaviour of traces require that there be different types of traces (this point is recognized by [Chomsky 1982]). We might hold that there is but a single type of trace, and that this trace can be bound arbitrarily, but that certain results simply could not be obtained via movements given certain configurations of binding (this position is particularly palatable, I think, given our revised outlook on which we begin with LFs and move from there to SSs). Thus, for example, we might have an LF of the form:

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(FN 26-LF) [<sub>S</sub> [<sub>NP</sub> every man]<sub>1</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>2</sub>]<sub>2</sub> [<sub>VP</sub> said [<sub>S</sub> that [<sub>S</sub> [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub> [<sub>VP</sub> would win]]]]]]]

which would violate the binding theory if the first trace 't<sub>2</sub>' were taken as an NP-trace, since t<sub>2</sub> is not bound under government. However, what we will find, taking LF as a starting point and moving to SS, is that no movement of the initial NP 'every man' makes the trace 't<sub>2</sub>' into what would be regarded as an NP-trace -- instead it is forced to act like pro and thus, in English, to surface as a pronoun to form:

(FN 26) He said that every man would win.

Even if the above suggestion for unifying traces is successful, however, we would be left with the curious fact that different binding configurations of traces give rise to different lexical realizations. There are three manifestations of this problem: first, PRO-type traces -- those occupying agent positions in infinitival phrases in which the agent of the infinitival action is the same as the subject of the larger sentence, which have no phonetic realization; second, traces bound by wh-phrases, which also have no phonetic realization; and third, NP-traces which are realized phonetically as reflexive pronouns. On the first score, I think there is reason to be skeptical of the existence of PRO-positioned traces in the first place. The standard view is that a sentence like:

(FN 27) Mary wanted to go home.

has the surface form:

(FN 27-SS) [<sub>S</sub> [<sub>NP</sub> Mary] [<sub>VP</sub> wanted [<sub>S</sub> [<sub>NP</sub> PRO] [<sub>VP</sub> to go home]]]]]

However, the well-known observation that de se attitude ascriptions cannot be reduced to de re attitude ascriptions (see, e.g., [Perry 1979]) casts doubt on that analysis. If (FN 27-SS) is the right analysis of (FN 27), then, assuming the usual semantic interpretative mechanisms, (FN 27) ought to be equivalent to:

(FN 28) Mary wants Mary to go home.

or:

(FN 29) Mary wants herself to go home.

But it is not so equivalent, because Mary might want herself to go home -- perhaps seeing herself exhausted-looking in the mirror without realizing that she is seeing herself -- without wanting to go home. There seems, then, to be some reason to think that there is an alternative de se infinitival construction available on which there is no trace present (note, furthermore, that (FN 29) is a plausible reading of (FN 27-SS) in which the trace manifests itself, as usual, as a pronoun).

The problem of traces bound by wh-phrases is more complicated, but here too I think some response can be made. The problem here is that, for example, in:

(FN 30) Whom did you see?

the predicted surface form is:

(FN 30-SS) [<sub>S</sub> [<sub>NP</sub> whom]<sub>1</sub> [<sub>S</sub> you saw t<sub>1</sub>]]]

in which one is forced to conclude (mysteriously) that the trace t<sub>1</sub> has no phonetic realization. However, I think there is some reason to suspect that wh-phrases are not in fact variable binding operators but are rather themselves variables. (See footnote 198 below, in which I explore the possibility that 'what' and 'where' are in fact the same words as 'that' and 'there') If this is right, then the correct surface form for (FN 30) has no terminal trace. Instead, we will need some story on which question formats involve a transposition of normal syntactic ordering, a transposition which leaves the trace -- lexically realized as 'whom' (perhaps a phonetic variant on 'him') at the front of the sentence.

The third difficulty -- that of the distinction between reflexive and non-reflexive pronouns -- is one to which I have no fully adequate

## §2.2 Noun Phrases and Bound Variables

Having in the previous section laid the groundwork for the claim that there are lexical items in natural languages which play the role of variables, I want in this and the next section to further develop that claim by showing how these variables behave and how they lead to a satisfactory taxonomy of noun phrases. The goal in this section will be to examine the behaviour of *bound* natural language variables. However, as mentioned above, we will defer until the next chapter discussion of traditional variable binding configurations, which manifest themselves in natural language as quantified noun phrases. For now, we will take up two projects: (a) to examine the behaviour and utility of anaphorically bound but undistributed variables, and (b) to further defend the claim that all pronouns in natural language can be treated as variables. The two projects, as we will see below, dovetail conveniently.

### §2.2.1 Why Can't 'A Donkey' Bind 'It'?

I claimed earlier that all pronouns are variables. As we saw, even restricting ourselves to the WNVT view that third person pronouns are variables, there are at least three significant obstacles to this view.

First, there are 'deictic' pronouns not associated with any variable

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answer. It seems to me, however, unlikely that anything of great significance turns on this distinction, especially since it is one (unlike the previous two discussed) which is relatively easily violated in colloquial speech. Note that the following are interpretable sentences of English, despite violating binding restriction (B) in the first case and (A) in the second:

(FN 31) I bought me a new car today.

(FN 32) Herself just entered the room.

Obviously a great deal of work needs to be done to rework GB theory along the lines I have suggested here, and that work lies largely outside the scope of this work. For our current purposes it suffices if the reader is willing to accept that there is a viable conception of syntax which (a) derives surface structures directly from logical forms, (b) which logical forms employ a quantifier/variable binding structure similar to that of formal logic, and (c) allows some variables to manifest in surface syntax as pronouns.

binding operator. Second, there are pronouns anaphoric on proper names and other referring expressions which, although they are capable of providing semantic content to pronouns, cannot (on the traditional analysis) act as binders of those pronouns. Third, there are cases of cross-clausal anaphora. It is this third case which I want to take up here.

### §2.2.1.1 Situating the Problem

Cases of cross-clausal anaphora, or 'donkey' pronouns, look like they ought to be treatable as bound variables.<sup>64</sup> In simple cases of intra-clausal anaphora, such as:

(104) **Every man** admires **his** father.

in which the pronoun is anaphoric on a quantified noun phrase within its clause<sup>65</sup>, we clearly want to treat the pronoun as a bound variable, and thus see (104) as properly analyzed by:

(104') [every x: man x][the y: y father-of **x**]x admires y

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<sup>64</sup>For a wishful endorsement of this hope, see:

It would be good if our formal language allowed variables to be bound to arbitrary terms both within the sentence and across the sentential barrier in the way in which anaphoric reference takes place in natural language. The problem of how to do this in a suitably smooth way seems quite interesting. [Kaplan 1989, 589]

<sup>65</sup>More precisely, where the pronoun is anaphoric on a quantified noun phrase in whose scope it lies, or by which it is c-commanded. Determination of whether a pronoun is within the scope of a quantified NP on which it is anaphoric needs to be done at the level of logical form, as examples such as:

(FN 33) The father of **each girl** waved at **her**.

show. In this example, 'her' acts as a variable bound by the quantified NP 'each girl', even though, in surface structure, the pronoun lies outside the scope of that NP. If we consider binding operations on the level of logical form, however, we see that classical conceptions of quantification are perfectly adequate to handle this kind of example, since we have the LF:

(FN 33-LF) [<sub>S</sub> [<sub>NP</sub> each girl]<sub>1</sub>] [<sub>S</sub> [<sub>NP</sub> the father of [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub>]<sub>2</sub>] [<sub>S</sub> t<sub>2</sub> waved at t<sub>1</sub>]]

in which the pronoun (represented here by the second occurrence of the trace 't<sub>1</sub>', does lie within the scope of the NP 'each girl'. See [Neale 1990, 191-197] for further discussion.



with the highlighted variable corresponding to the pronoun of (104). In cases of donkey anaphora, we also have pronouns anaphoric on quantified noun phrases, and prima facie they ought to receive treatment similar to that of intra-clausally bound pronouns.

However, the consensus opinion now is that donkey pronouns cannot be straightforwardly treated along the lines of intra-clausal pronouns. As mentioned above, in the canonical example:

(97) Every man who owns a donkey vaccinates **it**.<sup>66</sup>

the final pronoun 'it' is thought to require some treatment more sophisticated than the simple bound 'his' of (104) above, since here the pronoun falls outside the scope of the quantifier 'a donkey'; a straight-forward attempt to employ the tactics of quantificational logic yields the non-sentential:

(97') [every x: man x & [a y: donkey y] owns x,y] vaccinates x,y with its unbound final 'y'.

#### **§2.2.1.2 Why Can't 'A Donkey' Bind 'It'?**

While it has become a commonplace that the pronouns in sentences exhibiting donkey anaphora cannot be analyzed in terms of bound variables, I think that the arguments for this conclusion are moved through much too quickly, and want here to take a closer look at why this ought to be true.<sup>67</sup> Consider again the following donkey sentence and its 'first approximation' formal analysis:

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<sup>66</sup>I assume throughout this section that (97) is to be given what have become the standard truth conditions: that it requires every man who owns any donkeys at all to vaccinate each one of them. In §2.2.2 below, we will take up reasons to question these truth conditions.

<sup>67</sup>It's worth warning the reader in advance that the vast bulk of this section consists in the construction of various inadequate formalisms for providing a bound variable account of donkey anaphora. This may seem frustrating or pointless to some, but I think the exercise provides valuable insight into what the deep issues here are.

(97) Every man who owns a donkey vaccinates it.

(97') [every x: man x & [a y: donkey y] owns x,y] vaccinates x,y

The problem, then, is that the final 'y' is unbound. But why need this be true? I don't mean to suggest here, as some have, that we might be able to construe the sentence in some way which gives a quantifier associated with 'a donkey'<sup>68</sup> wide scope and thus making a bound final 'y' possible, I agree that there are good reasons for thinking that such a route is at best theoretically unsatisfying and at worst empirically unproductive. Instead, I want to ask why, *on the very formulation given in (97')*, that troublesome final 'y' needs to be unbound.

The simple answer, of course, is that it falls outside the scope of the only 'y' quantifier in the sentence -- the '[a y: donkey y]' which occurs within the 'x' quantifier. However, like most simple answers, this one tells us little that's useful. What we ultimately want to know is why the semantic effect of the quantifier -- what we will call below the range of the quantifier -- need be limited to that quantifier's scope.<sup>69</sup> Why couldn't we redefine the range of a quantifier in such a way that we managed to get all the occurrences of 'y' in (6') within that range?<sup>70</sup>

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<sup>68</sup>In general, not an existential quantifier if we want to get the truth conditions correct.

<sup>69</sup>For current purposes, the range of a quantifier can be understood as the area within which that quantifier can bind variables. There is some sense of the word 'scope' on which the scope of a quantifier is the same as its range, but since there is also a preexisting syntactic notion of scope, I choose here to introduce new terminology for the semantic concept to avoid confusion. Classical logic, then, tacitly assumes that scope and range are coextensional.

<sup>70</sup>My project here shares with the dynamic logic project (as exemplified in [Groenendijk & Stokhof 1991]) the idea of extending the semantic effect of a quantifier beyond its syntactically provided scope. However, beyond this shared starting point the two projects have little in common. The dynamic logic project is in some ways more expansive than mine, since it also produces dynamic interpretative rules for the sentential connectives, while I abide by the classical understanding of

Again, a simple and unhelpful answer is available. We can, of course, easily redefine range to do this. I'll do so right now. Take the range of a quantifier to be the entirety of the formula in which it appears. Now the final 'y' in (97') will be within the range of '[a y: donkey y]'. The real question, though, is whether this, or some other, broadened notion of range can be incorporated into a complete semantics for English (or whatever natural language we are attempting to analyze).

#### **§2.2.1.2.1 Some Experiments With Non-Standard Quantifier Range**

How one would go about answering this question depends on what one takes an adequate semantic theory to look like. I take it, however, that the production of a truth theory for a language in which the notion of range is appropriately extended would for most represent at least a good start (and would for some be wholly sufficient). Let us, then, descend into the technical for a while and see what goes wrong with attempts to construct such a T-theory. First, consider a standard first-order language with restricted quantifiers. The language will have a lexicon as follows:

(L1) A collection  $x_1, \dots, x_n, \dots$  of variables

(L2) A collection  $P_1^1, \dots, P_1^n, \dots, P_2^1, \dots, P_m^1, \dots, P_m^n, \dots$  of predicates, where the subscript specifies the arity of the predicate

(L3) A collection  $D_1, \dots, D_n, \dots$  of determiners, each of which has some natural language equivalent  $N(D_i)$ .

---

such connectives. On the other hand, the dynamic project is in other ways more restrictive than mine, since they limit the range of dynamically interpretable quantifiers to the existential quantifier. The formal devices of [Groenendijk & Stokhof 1991] are deeply indebted to the underlying semantic framework of discourse representation theory as developed in [Kamp 1981] with the addition of certain compositionality-securing bells and whistles (of a general sort discussed in §3.4.1.4 below); as a result it shares the general inability of discourse-representation-theory-based accounts to handle generalized quantifiers (an inability further discussed in §3.2.1.3.2.4.2 below).

The language also possesses the usual range of connectives and any necessary grouping apparatus.<sup>71</sup> We can now, given a model  $M$  which assigns an extension to each predicate, give a theory of satisfaction for the language:

T-Theory: Given a formula  $\phi$  and an infinite sequence  $s$  of objects drawn from the domain of  $M$ , we say:

(T1) If  $\phi$  is atomic of the form  $P_n^m x_{i_1} \dots x_{i_n}$ , then  $s$  satisfies  $\phi$  iff  $\langle s(i_1), \dots, s(i_n) \rangle \in \text{Ext}(P_n^m)$  [where  $s(x)$  picks out the  $x^{\text{th}}$  member of the sequence  $s$ ].

(T2) If  $\phi$  is of the form  $C(\psi_1, \dots, \psi_n)$  for some  $n$ -place sentential connective  $C$ , then  $s$  satisfies  $\phi$  iff  $T_C(\text{Sat}(s, \psi_1), \dots, \text{Sat}(s, \psi_n)) = T$  [where  $T_C$  is the truth function associated with  $C$ , and  $\text{Sat}$  is the satisfaction function, which yields 'true' iff  $s$  satisfies  $\psi$ ].

(T3) If  $\phi$  is of the form  $[D x: \theta(x)] \psi(x)$ , then  $s$  satisfies  $\phi$  iff  $N(D)$  sequences, differing from  $s$  in at most the 'x' position and satisfying  $\theta$ , also satisfy  $\psi$ .

Clearly, these three clauses suffice to define satisfaction for arbitrary formulas. We then define truth as satisfaction by all sequences.

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While using 'range' in place of 'scope' adds clarity to the subsequent discussion, it unfortunately also prompts the rather ugly neologism of one quantifier *having range* over another, which should be read parallel to claims about one quantifier having scope over another.  
<sup>71</sup>Neither functional nor constant expressions seem germane to the points being made here, so for the sake of simplicity I have chosen to omit them.

As it stands, the truth theory I have given is committed to the classical view that quantifier range is quantifier scope. We can correct this, however, by modifying clause (T3) slightly. Let RANGE be a function which maps from occurrences to quantifiers to substrings of particular formulas, defined so that it picks out the range of any given quantifier.<sup>72</sup> [Thus, for example, in the usual first order semantics, RANGE would send each quantifier to the smallest formula immediately to the right of the quantifier]. The most obvious modification of (T3) would be:

(T3-1) If  $\phi$  is of the form  $[D x: \theta(x)] \psi(x)$ , then  $s$  satisfies  $\phi$  iff  $N(D)$  sequences, differing from  $s$  in at most the 'x' position and satisfying  $\theta$ , also satisfy  $\text{RANGE}([D x: \theta(x)])$ .<sup>73</sup>

So let's try to use (T3-1) to make sense of (97') by changing our definition of range. In general, we want quantifiers embedded inside other quantifiers to be able to bind variables bound by the larger quantifier but outside the conventional scope of the embedded quantifier. We can formalize this through the following new definition of range:

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<sup>72</sup>Of course, the range of any quantifier must always consist of formula-sized pieces, if it is to be usefully evaluable within a truth definition.

<sup>73</sup>There's a slight complication being suppressed here. If the modified scope of a quantifier is disconnected (as, in fact, it is in some of the examples I proceed to discuss) we can't simply appeal to satisfaction of this disconnected thing. Instead, we will here require simultaneous satisfaction of each disjoint piece of the disconnected whole.

(RANGE<sub>1</sub>) If  $[D y: \theta(y)]$  is a quantifier in a context of the form:  $[D' x: \varphi([D y: \theta(y)] \psi(y))]$   $\xi(x,y)$ , then  $[D y: \theta(y)]$  has range over ' $\psi(y)$ ' and over ' $\xi(x,y)$ '.<sup>74</sup>

RANGE<sub>1</sub> results in the following range assignments to the two quantifiers in (97'):

- Range of '[every x: man x & [a y: donkey y] owns x,y]' = 'vaccinates x,y'
- Range of '[a y: donkey y]' = 'owns x,y' and 'vaccinates x,y'

We thus succeed in making the final 'y' of (97') a bound variable.

Now consider whether an arbitrary sequence  $s$  satisfies, under the revised formulation of satisfaction, (97'). Since (97') is of the form  $[D x: \theta(x)] \psi(x)$ , we first apply (T3-1), and determine that  $s$  satisfies (97') if every sequence differing from  $s$  at most in the 'x' position and satisfying:

$\text{man } x \text{ \& \& [a y: donkey y] owns } x,y$

also satisfies 'vaccinates x,y'. Now consider some sequence  $s'$  differing from  $s$  at most in the 'x' position. In order to know if in fact it satisfies:

$\text{man } x \text{ \& \& [a y: donkey y] owns } x,y$

we need to know if it satisfies both 'man x' and '[a y: donkey y] owns x,y'. The satisfaction of 'man x' is unproblematic; here we just apply rule (T1). However, '[a y: donkey y] owns x,y' is more complex. In order to know if  $s'$  satisfies '[a y: donkey y] owns x,y', we need to know, by

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<sup>74</sup>Again, there are subtleties involved here. This scope assignment will obviously produce bad results if the 'y' in the final ' $\xi(x,y)$ ' is already bound by a previous 'y' quantifier. It is probably best throughout the subsequent discussion to assume that our syntax is rigged so as to ensure that no two quantifiers in the same sentence employ the same variable.

(T3-1), if there is a sequence  $s'$ , differing from  $s$  at most in the 'y' position and satisfying 'donkey y', which satisfies 'owns x,y' **and** (due to  $RANGE_1$ ) 'vaccinates x,y'.

To summarize,  $s$  satisfies (97') if and only if the following condition is met:

Whenever we put a new object in the 'x' position, as long as it is a man and there is some object we could put in the 'y' position which is a donkey owned and vaccinated by that man, the man vaccinates whatever is in the 'y' position of the original sequence  $s$ .

But the resulting truth conditions do not match those of (97). Consider a situation in which there is exactly one man  $M$  and in which that man owns exactly three donkeys,  $A$ ,  $B$ , and  $C$ . Assume furthermore that  $M$  vaccinates all three of these donkeys. Clearly this is a situation in which:

(97) Every man who owns a donkey vaccinates it.

is true. But now consider some sequence  $s$  drawn from this situation which contains in the 'y' position some donkey  $D$  not vaccinated by  $M$ . We can show that  $s$  in fact does not satisfy (97') under our current formalism.

To do so, we want, by appeal to (T3-1), to construct some sequence  $s'$  that (i) differs from  $s$  at most in the 'x' position, (ii) satisfies both 'man x' and '[a y: donkey y] owns x,y', and (iii) does *not* satisfy 'vaccinates x,y'.

• Since  $M$  is the only man in this situation, our  $s'$  must have  $M$  in the 'x' position to satisfy the first half of (ii).

• As we saw above, in order for this  $s'$  to satisfy '[a  $y$ : donkey  $y$ ] owns  $x,y$ ', some sequence  $s''$ , differing from  $s'$  at most in the ' $y$ ' position and satisfying 'donkey  $y$ ', must satisfy 'owns  $x,y$ ' and 'vaccinates  $x,y$ '. As long as we pick any of A, B, or C to occupy the ' $y$ ' position of  $s''$ , we will fulfill these conditions. Thus there do exist appropriate  $s''$ , so  $s'$  does satisfy 'man  $x$  & [a  $y$ : donkey  $y$ ] owns  $x,y$ '.

However, since  $s'$  has the same object in the ' $y$ ' position as  $s$  does, and since  $s$  was chosen so that it would not satisfy 'vaccinates  $x,y$ ', we see that  $s'$  also does not satisfy 'vaccinates  $x,y$ '. Thus we conclude that  $s$  does not satisfy (97'), and that it must be false -- proving that it is an inadequate formal representation of the natural language (97).

We can try to patch up this problem, but not very successfully. In any case, the theory we have been considering seems the most rational approach to extending the scope of the contained quantifier to the final variable, and we should suspect any modifications of ad hoc technical tinkering. The problem we ran into above is that the ' $y$ ' term of the original sequence is at no point forced to be a donkey owned by the ' $x$ ' term. In order to avoid this problem, we can extend the scope of the larger ' $x$ ' quantifier to include the internal material of the formula (old-fashionedly) bound by the ' $y$ ' quantifier. We thus introduce a second definition of modified range:

RANGE<sub>2</sub>: In a context of the form '[D  $x$ : [D'  $y$ :  $\theta(y)$ ]  $\psi(y)$ ]  $\xi(x)$ ' the range of the quantifier '[D  $x$ : [D'  $y$ :  $\theta(y)$ ]  $\psi(y)$ ]' will include  $\theta(y)$ ,  $\psi(y)$ ,  $\xi(x)$ , and any range assigned to it by RANGE<sub>1</sub>.



Evaluation under RANGE<sub>2</sub> would solve the above problem, since the original *s* would have to satisfy 'donkey *y*' and 'owns *x,y*', insuring that the final thing vaccinated by *x* really was a donkey owned by *x*.

However, this account has at least two serious flaws. First, it will get truth conditions wrong with some choices of embedded determiner. For example, if we have the sentence:

(105) Some man who owns no cats likes film.

the new range requirements will force us to find a sequence which satisfies both '[no *y*: cat *y*] owns *x,y*' and the pair 'cat *y*' and 'owns *x,y*', which is clearly impossible.

More significantly for our current purposes, note that this new statement of broadened range has the result of effectively imposing the determiner of the '*x*' quantifier onto the semantic material of the '*y*' quantifier. Thus the sentence:

(97) Every man who owns a donkey vaccinates it.

gets rewritten as:

(97-NEW) Every man vaccinates every donkey he owns.<sup>75</sup>

It's well-known that universalizing the existential quantifier in 'a donkey' will get the right truth conditions for (6)-like cases of donkey anaphora. However, if we alter the initial determiner, things don't work out so well. For example, using this method, the sentence:

(106) Most men who own a donkey vaccinate it.

is assigned the same truth conditions as:

(106-NEW) Most men vaccinate most donkeys they own.

Clearly (106) and (106-NEW) do not have the same truth conditions.<sup>76</sup>

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<sup>75</sup>Sentence (97-NEW) is a rewrite of (97) in the sense that it is a natural language sentence having the same truth conditions as the purported formal analog (97') of (97) under the current semantic rules.

### §2.2.1.2.2 Mutual Bondage and Recursive Truth Theories

So what went wrong? We did what we set out to do -- extend the range of the '[a y: donkey y]' quantifier to cover the final 'y' corresponding to the problematic 'it' -- but we failed to obtain a satisfactory semantic analysis of (6). Of course, this failure could be taken as an indication that a bound-variable analysis really is the wrong way to go, but I don't think we need to give up so soon. The problem, roughly speaking, is that, since the '[a y: donkey y]' quantifier is within the range of '[every x: man x & [a y: donkey y]'

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<sup>76</sup>My objection here clearly owes a great deal to [Harman 1972], which observes that a general project of universalizing the contained quantifier misconstrues truth conditions when the contained determiner is altered. Thus, for example,

(F34) Every man who owns at least three donkeys vaccinates them. does not have the same truth conditions as:

(F34-NEW) Every man vaccinates every donkey he owns.

I take these considerations and apply them to alterations of the initial determiner. Both objections serve to bring out, as I attempt to explain above, that certain formalisms fail to recognize adequately the role of [a y: donkey y] as an independent quantifier.

It's worth noting here that I find some of the donkey sentences obtained by altering initial determiners difficult to construe in line with the standard truth conditions. Consider each of the following:

(F35) Some man who owns a donkey vaccinates it.

(F36) Few men who own a donkey vaccinate it.

(F37) No man who owns a donkey vaccinates it.

Standardly, these sentences should be (at least) truth-conditionally equivalent to:

(F35-NEW) Some man who owns a donkey vaccinates whatever donkeys he owns.

(F36-NEW) Few men who own a donkey vaccinate whatever donkeys they own.

(F37-NEW) No man who owns a donkey vaccinates whatever donkeys he owns.

However, I am unconvinced that (F35)-(F37) necessarily require the vaccination of all owned donkeys, as do (F35-NEW)-(F37-NEW). (F37) seems the strongest case here. Surely it is false that no man who owns a donkey vaccinates it even if there is one man who owns three donkeys and vaccinates just one of them. Neale has suggested that the problem here is due to the presence of monotone decreasing quantifiers (leaving (F35) unexplained, but perhaps we can adjust our intuitions here), although no actual theory exploiting this presence has been developed. If (F35)-(F37) do differ from the standard truth conditions, it is as much a problem for my positive account as for anyone else's. For now, I leave the issue open, but see §2.2.2.1 below for alternative readings of donkey pronouns and §3.3.1.3.1 below for discussion of quantifier monotonicity in the anaphoric account of variable binding.

owns  $x,y$ ]' quantifier, we can, when evaluating the  $x$ -quantifier of larger scope, pick an  $x$  and  $y$  (as we do above) which fail to satisfy 'vaccinates  $x,y$ ', and the smaller-ranged  $y$ -quantifier never gets a chance to impose its restrictions, since its evaluation has already been completed. One way of putting this is to say that we have taken insufficiently seriously the idea that the  $y$ -quantifier has larger range than the traditional rules would indicate. The  $y$ -quantifier needs to have range not only over the final 'vaccinates  $x,y$ ' (to bind that final 'y') but also over the  $x$ -quantifier itself, so that, in the evaluation of the  $x$ -quantifier, we are forced to respect the restrictions of the  $y$ -quantifier.

A third stab at solving the problem, then, might be to assign range as follows:

RANGE<sub>3</sub>: If '[D  $x$ :  $\varphi(x)$ ]' is a quantifier appearing within a formula  $\psi$ , then the range of '[D  $x$ :  $\varphi(x)$ ]' is the entirety of  $\psi$ . Under RANGE<sub>3</sub>, in (6') each of '[every  $x$ : man  $x$  & [a  $y$ : donkey  $y$ ] owns  $x,y$ ]' and '[a  $y$ : donkey  $y$ ]' has range over the entire formula. The hope is that by giving each quantifier range over the other we can perhaps avoid the difficulties which sank the last approach.

However, trying to implement this proposal leads us to one of the deep problems with this general project. While RANGE<sub>1</sub> interacted with (T3-1) to produce a well-behaved language<sup>77</sup> our revised truth theory has hidden pitfalls lurking within it. Consider the following formula:

(107') [every  $x$ : man  $x$ ]( [some  $y$ : boy  $y$ ] taller  $y,x$ )

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<sup>77</sup>We may not have gotten the truth conditions we wanted from our (97') under the revised semantics, but we got a perfectly sensible language which could make sense out of (97') as a closed formula.

Let us again stray from the traditional range assignments, and declare that both the initial x-quantifier and the embedded y-quantifier have range over:

[some y: boy y] taller y,x

Construct an arbitrary model<sup>78</sup>, and take some sequence s of objects from that model. Does s satisfy (107')? Let's walk through the truth theory and find out. (107') is a formula of the form  $[D x: \theta(x)] \psi(x)$ , so we apply rule (T3-1). We thus need to know if every sequence differing from s at most in the 'x' position and satisfying 'man x' also satisfies:

[some y: boy y] taller y,x

Take, then, some sequence s' differing from s at most in the 'x' position and satisfying 'man x'. Now ask if s' satisfies:

[some y: boy y] taller y,x

Again we have a formula of the form  $[D x: \theta(x)] \psi(x)$ , so we again apply rule (T3-1). We need to know if there is a sequence s'', differing from s' at most in the y position and satisfying 'boy y', such that s'' satisfies:

[some y: boy y] taller y,x<sup>79</sup>

But now the problem should be obvious. To know if there is such an s'', we must again apply rule (T3-1), and ask if there is a sequence s''', differing from s'' at most in the y position and satisfying boy y, which satisfies:

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<sup>78</sup>Not entirely arbitrary, actually. I will proceed to place some conditions on the model as I proceed. For example, in the following discussion I will assume that the extension of the predicates 'man x' and 'boy y' are non-empty in the model at hand. These tacit restrictions play a role periodically in much of my subsequent discussion. They could be made explicit, but would quickly become tedious.

<sup>79</sup>Since, by the scope rule RANGE<sub>3</sub>, the quantifier '[some y: boy y]' has range over all of 'some y: boy] taller x,y'.

[some y: boy y] taller y,x

In order to answer this question, we will need to appeal to a fourth sequence s''', invoke (T3-1) again, and so on. Our quest for a satisfaction evaluation will degenerate into an infinite regress of sequence constructions.

I suggested above that in order to get an adequate formal realization of (97) along the lines of (97'), we would need to find a way to have each quantifier have range over the other. The considerations of the previous paragraph show that there is something problematic about attempts to do this. By replacing (T3) with (T3-1) and allowing for a broader notion of quantifier range, we undermine one of the implicit assumptions of a recursive truth theory. The standard Tarskian semantics relies on the assumption that any formula can be divided into semantic units in such a way that we can process a semantic unit and then be done with it, from then on treating it solely as input to the evaluation of future semantic units. More precisely, we want the units to form a strict partial ordering under the substring relation. Since any partial order can be extended to a linear order, we can then find some ordering of the semantic units (including the quantifiers and their scope) in which each can be fully evaluated all at once. This is the crucial point: it is this linear ordering which makes a recursive, Tarski-style truth theory possible -- each semantic unit requires only a single pass of the theory, and we ascend our ordering until we arrive at an evaluation of the entire formula<sup>80</sup> -- and it is this linear ordering we risk forfeiting when we seek quantifiers with range over each other.

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<sup>80</sup>We are wandering close to complex technical issues here. A Tarski-style recursive theory is not the only formal mechanism through which truth values can be assigned to sentences of a language, and other

Of course, it can't be true that it's absolutely impossible to alter the range of a quantifier without destroying the ordering required for recursive evaluation. We have already succeeded in giving a semantics which allowed for evaluation of (6') as a closed formula, albeit not as an adequate translation of the natural language (6). On the other hand, the problem is not as simple, as the example of '[every x: man x]([some y: boy y] taller y,x)' might seem to indicate, as forbidding quantifiers to have range over themselves. Consider the following formula:

(108') [every x: man x] taller x,y & [some y: woman y] smarter y,x

Let's make this into a closed formula by giving each formula range over the entire formula minus itself. In attempting to determine whether a sequence *s* satisfies (10'), we will in evaluating the initial 'x'

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methods may not be subject to the infinite regress problems found here. Trivially, we could have a theory which assigned 'true' to every sentence of the language, including problematic sentences like (107'). If we wanted to avoid making both sentences and their negations true in this way, and were willing to endorse truth-value gaps, we could use a Tarski-style theory with the single modification that any sentence whose evaluation lead to an infinite regress would be assigned a gap.

Since I see no way to survey ahead of time the infinite range of possible non-Tarskian methods for assigning truth values to sentences, I continue my paper within a Tarskian framework. I will content myself with offering the following cautionary notes to any attempt to employ an alternative truth theory to escape the regress problems and continue the line of investigation of §2.2.1.2.2. First, the new theory must respect enough of the internal structure of the sentences it operates on for the project of extending the scope of the 'y' quantifier in (97') to the final free 'y' to make sense. Second, the new theory needs to be able to assign (97') (and others like it) appropriate truth values in appropriate situations -- leaving it always a gap is inadequate. Third, the theory ought to retain most if not all of the inferential connections we intuitively feel should hold among sentences in the language (spelling out what these connections are is itself a non-trivial task; note that I have given no deductive systems for any of my extensions of traditional first-order logic).

Recent work such as [Kripke 1975] and [Gaifman 1992] has made a plausible case that non-Tarskian (and perhaps non-finite) mechanisms may have a place somewhere in the ideal truth theory. Note, however, that all of this work has focused on the impact of the object language semantic vocabulary and has taken an underlying Tarskian mechanism for granted. Eliminating this mechanism entirely and meeting the conditions set out above strikes me as a substantially nontrivial exercise.

quantifier be referred to the 'y' quantifier, and in evaluating that 'y' quantifier be referred back to the 'x' quantifier, and so on. This shows that the problem is not restricted to quantifiers looping on themselves, but that the unevaluable topologies can be more involved.

In fact, we can state a simple theorem which gives a wholly general condition under which a range definition gives rise to these infinite regress problems. First, we define the relevant notion of a Tarski-evaluable range:

(Def. 13) A range function RANGE is *Tarski-evaluable* if the explicit truth definition constructed from (T1), (T2), and (T3-1) using RANGE is total; that is, if it assigns a determinate truth value to every sentence in the language.<sup>81</sup>

Employing this definition, we can now state the following theorem:

(Theorem) A range function RANGE is Tarski-evaluable if and only if there is no formula  $\psi$  in the language containing a series  $[D_1 x_1: \phi_1], \dots, [D_n x_n: \phi_n]$  of quantifier instances such that for each  $i \in \{1, \dots, n-1\}$ , either:

(i)  $[D_{i+1} x_{i+1}: \phi_{i+1}]$  is a substring of

RANGE( $[D_i x_i: \phi_i]$ ), or

(ii)  $[D_{i+1} x_{i+1}: \phi_{i+1}]$  is a substring of  $\phi_i$ .

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<sup>81</sup>Note that this definition makes sense only under the assumption that we are pursuing a Tarski-style truth theory. The kinds of approaches I gesture toward in the previous footnote are thus not captured by my subsequent theorem. Extending the theorem to a *completely* general condition is, for roughly the reasons laid out in that footnote, in all likelihood a futile task.

and  $[D_1 x_1: \phi_1]$  is itself a substring either of  $\phi_n$   
or of  $\text{RANGE}([D_n x_n: \phi_n])$ .

The theorem can be proven through a straightforward induction on the number of quantifiers in a formula.

We can now apply this theorem to see that the infinite regress problems do arise for the scope assignment  $\text{RANGE}_2$  intended to solve the truth-conditional problems which arose earlier. In (97'), the initial 'x' quantifier contains the 'y' quantifier in its restrictor, and that 'y' quantifier then contains the 'x' quantifier in its scope, thus violating the condition of for Tarski-evaluability. Intuitively, if we ask if a sequence  $s$  satisfies (97') under these scope rules and the T-theory rule (T3-1), we will first have to evaluate the initial 'x' quantifier. In order to do so, we will need to introduce new sequences  $s'$  and consider whether they satisfy the embedded 'y' quantifier. But to settle this question, we will introduce yet more sequences  $s''$  and ask if they satisfy the initial 'x' quantifier, and we will be caught up in an evaluative loop.

#### **§2.2.1.2.3 Simultaneous Evaluation of Multiple Quantifiers**

Getting an adequate semantics for (97'), then, seems to require that each quantifier have scope over the other, but the very nature of a recursive truth definition makes this an impossible task. Nevertheless, I don't yet want to give up on this project. We wanted to give each quantifier scope over the other because if one were fully evaluated before beginning the next we got the wrong truth conditions (the first quantifier never got a chance to place the appropriate restrictions on some variable). Since we can't have each evaluated before the other,



perhaps we can once again modify our T-theory so that they get evaluated simultaneously.<sup>82</sup>

Let's return to our rule RANGE<sub>1</sub> for determining the scope of a quantifier and reconsider (97'). Under RANGE<sub>1</sub>, '[a y: donkey y]' has range over 'vaccinates x,y' but not over the larger x-quantifier. The new hope is that this ordered range assignment will, when coupled with simultaneous evaluation of the two quantifiers, avoid the failures we encountered in §2.2.1.2.1. We now replace our modified (T3-1) with the even further modified:

(T3-2) If  $\phi$  is of the form  $[D x: \theta(x)] \psi(x)$ , where  $[D x: \theta(x)]$ ,  $[D_1 x_1: \theta_1(x_1)]$ , ...,  $[D_n x_n: \theta_n(x_n)]$  are quantifiers in  $\phi$  with scope over  $\psi$ , then a sequence  $s$  satisfies  $\phi$  iff  $N(D)$  sequences differing from  $s$  at most in the  $x, x_1, \dots, x_n$  positions and satisfying  $\theta, \theta_1, \dots, \theta_n$  also satisfy  $\psi$ .

Now consider once again (97) and its would-be formal equivalent (97'):

(97) Every man who owns a donkey vaccinates it.

(97') [every x: man x & [a y: donkey y] owns x,y] vaccinates x,y

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<sup>82</sup>The idea I explore here of simultaneous evaluation of multiple quantifiers should not be confused with the type of simultaneous evaluation allowed by polyadic quantifiers. While it is true that polyadic quantification allows us to replace the analysis of a sentence like:

(98) Every boy read some book.

as involving an ordered pair of quantifiers with an analysis which appeals to a single quantifier:

(98-PQ) [every-some x,y](boy(x)  $\wedge$  book(y)  $\rightarrow$  read(x,y))

the choice of polyadic quantifier itself still carries an ordering of the two determiner concepts of universality and existentialness. Thus (98-PQ) employs a different polyadic quantifier, and expresses a different thought, from:

(98-PQ') [some-every y,x](boy(x)  $\wedge$  book(y)  $\rightarrow$  read(x,y))

What we are seeking in this section is a way to make simultaneous the two determiners, a simultaneity not provided by the move to polyadic quantification.

Unfortunately, the revised (T3-2) still gives us the wrong truth conditions. Take a situation in which (97) is unequivocally true -- assume there is only one man M who owns a (single) donkey  $D_1$ , and that he vaccinates that donkey. Now ask if an arbitrary sequence  $s$  drawn from such a situation satisfies (97'). We will need to know, by (T3-2), if every sequence differing from  $s$  in at most the 'x' and 'y' positions and satisfying 'donkey y' and 'man x & [a y: donkey y] owns x,y' also satisfies 'vaccinates x,y'. Now assume there is some additional donkey  $D_2$  not vaccinated by M, and take  $s'$  to be a sequence with M in the 'x' position and  $D_2$  in the 'y' position.  $s'$  then clearly satisfies 'donkey y' and 'man x', and fails to satisfy 'vaccinates x,y', so (97') will be satisfied by  $s$  only if  $s'$  fails to satisfy '[a y: donkey y] owns x,y'.

So we again apply (T3-2). Since there are no embedded quantifiers to deal with here, (T3-2) reduces to (T3-1), and we conclude that  $s'$  is satisfactory iff there is some sequence  $s''$  differing from  $s'$  in at most the 'y' position and satisfying 'donkey y' which also satisfies 'owns x,y' and 'vaccinates x,y'. But here we just take  $s''$  to be the sequence obtained from  $s'$  by substituting  $D_1$  for  $D_2$  in the 'y' position. Thus  $s'$  does in fact satisfy '[a y: donkey y] owns x,y', so  $s$  fails to satisfy (97'), and it is not true (and hence not an adequate translation of (97)).

However, I don't think anything has gone deeply wrong here. The problem with (T3-2) is that, while we have provided for simultaneous evaluation of the embedding and embedded quantifiers, we have paid attention to the scope only of the largest quantifier. We have, in a way, regressed back toward the original (T3); we need to write the range function RANGE into the truth clause to obtain:

(T3-3) If  $\phi$  is of the form  $[D x: \theta(x)] \psi(x)$ , where  $[D x: \theta(x)]$ ,  $[D_1 x_1: \theta_1(x_1)]$ , ...,  $[D_n x_n: \theta_n(x_n)]$  are quantifiers in  $\phi$  with range over  $\psi$ , then a sequence  $s$  satisfies  $\phi$  iff  $N(D)$  sequences differing from  $s$  at most in the  $x, x_1, \dots, x_n$  positions and satisfying  $\theta, \theta_1, \dots, \theta_n$  and  $\text{RANGE}([D_1 x_1: \theta_1(x_1)]), \dots, \text{RANGE}([D_n x_n: \theta_n(x_n)])$  also satisfies  $\text{RANGE}([D x: \theta(x)])$ .<sup>83</sup>

(T3-3)<sup>84</sup> at long last gives us truth conditions for (97') which match those of (97).

To see this, think about what it takes for a sequence  $s$  to satisfy (97') under a T-theory incorporating (T3-3). It must be the case that every sequence  $s'$  differing from  $s$  in at most the 'x' and 'y' positions and satisfying 'donkey y', 'owns x,y' and 'man x & [a y: donkey y] owns x,y' also satisfies 'vaccinates x,y'. Now in order for  $s'$  to satisfy 'man x & [a y: donkey y] owns x,y' it must in particular satisfy '[a y: donkey y] owns x,y'. Applying (T3-3) again, we see that this requires that there be some  $s''$ , differing from  $s'$  in at most the 'y' position and satisfying 'donkey y', which satisfies 'owns x,y'.

In summary, then,  $s$  must be such that, when we replace the 'x' and 'y' elements, we obtain the 'x' object vaccinating the 'y' object so long as the following conditions are met:

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<sup>83</sup>There's a further complication here which I am glossing over. Depending on how the range function is defined, the entire range of some of these quantifiers may not be present within  $\phi$ . We would thus, in general, need to rewrite the truth theory so that we always have a total formula (with all ranges finally completed) being evaluated by recursively considering substrings. The truth theory would still be recursive, but would have to have the ability to look outward beyond the substring under consideration at any given stage. Only by providing the entirety of the sentential context could we guarantee this ability.

<sup>84</sup>When coupled with the range function  $\text{RANGE}_1$ .

- (i) The 'x' object is a man
- (ii) The 'y' object is a donkey
- (iii) The 'x' object owns the 'y' object
- (iii) There is some other [not necessarily distinct] 'y' object  
which is a donkey owned and vaccinated by the 'x' object.

But given all this, we see that (97') is true if and only if, any time we pick a man and a donkey he owns, the man vaccinates that donkey -- exactly what we want to capture the truth conditions of (97).

Furthermore, our formulation, unlike the account of §2.2.1.2.1, is properly sensitive to variations of the embedded determiner. For example, if we spell out the truth conditions of:

- (109') [every x: man x & [exactly three y: donkey y] owns x,y]  
vaccinates x,y

which is the formal analog of:

- (109) Every man who owns exactly three donkeys vaccinates them.

under our revised T-theory, we see that it is true if and only if every man who owns exactly three donkeys vaccinates all of them.

(T3-3), it seems to me, must be on the right track. Unfortunately, however, we still lack a sufficiently general theory. While permutations of the embedded determiner are unproblematic, changing the determiner of larger scope produces errors. Consider, for example, the following natural language/formal language pair:

- (110) Few men who own a donkey vaccinate it.
- (110') [few x: man x & [a y: donkey y] owns x,y] vaccinates x,y

Under (T3-3), (110') will be true iff few times when we pick pairs of a man and a donkey owned by the man will the donkey be vaccinated by the man.<sup>85</sup> That is, (110') is equivalent to the natural language:

(110-NEW) Few donkeys are vaccinated by their owners.

which is itself not equivalent to (110).

So what has gone wrong this time? I would suggest that we have run up against another deep issue regarding variable binding. While we may be able to make sense of more than one quantifier exerting a semantic influence at the same time (as we attempt to do in (T3-3)), what seems impossible to accommodate is the idea of more than one *determiner* coming into play simultaneously. Somewhere in our truth clause we need to say how many sequences of the right type there need to be in order to satisfy the formula, and here we have to pick some *particular* determiner to evaluate. In (T3-3) I chose the determiner of the largest quantifier. This happens to work when that determiner is 'every', but will fail in other cases. Picking the determiner of one of the contained quantifiers is no better -- here we will even more quickly obtain the wrong truth conditions ((97'), for example, will come out meaning 'Some man vaccinates some donkey he owns').

Is there no way to get all the determiners together into the quantifier evaluation clause? I see two plausible ways of proceeding, and it is informative to see why each fails. First, we could simply conjoin them, the way we do the semantic material bound by each quantifier. Thus we would end up with:

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<sup>85</sup>Working out the details here is left to the reader. It's a straightforward exercise, applying the rule (T3-3). An intuitive understanding of why this result is obtained can be gleaned from the next paragraph.

(T3-4) If  $\varphi$  is of the form  $[D x: \theta(x)] \psi(x)$ , where  $[D x: \theta(x)]$ ,  $[D_1 x_1: \theta_1(x_1)]$ , ...,  $[D_n x_n: \theta_n(x_n)]$  are quantifiers in  $\varphi$  with range over  $\psi$ , then a sequence  $s$  satisfies  $\varphi$  iff  $N(D)$  and  $N(D_1)$  and ... and  $N(D_n)$  sequences differing from  $s$  at most in the  $x, x_1, \dots, x_n$  positions and satisfying  $\theta, \theta_1, \dots, \theta_n$  and  $\text{RANGE}([D_1 x_1: \theta_1(x_1)]), \dots, \text{RANGE}([D_n x_n: \theta_n(x_n)])$  also satisfies  $\text{RANGE}([D x: \theta(x)])$ .

This approach, however, fails badly. If all the determiners are monotone increasing (decreasing), then the final result is simply equivalent to the strongest (weakest) of the list of determiners. If there is a mix of monotone increasing and monotone decreasing determiners, or if there are non-monotonic determiners in the mix, then we end up with a simple contradiction.<sup>86</sup> The point here is that what we want to do is not sum up the total effects of the determiners, but let each determiner have its effect independently.

Second, we could try to respect the independence of the separate quantifiers by separating out relevant parts of the bound material. That is, we could write a truth clause like:

(T3-5) If  $\varphi$  is of the form  $[D x: \theta(x)] \psi(x)$ , where  $[D x: \theta(x)]$ ,  $[D_1 x_1: \theta_1(x_1)]$ , ...,  $[D_n x_n: \theta_n(x_n)]$  are quantifiers in  $\varphi$  with range over  $\psi$ , then a sequence  $s$  satisfies  $\varphi$  iff  $N(D)$  sequences differing from  $s$  at most in the  $x$  position and satisfying  $\theta$ , and  $N(D_1)$

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<sup>86</sup>Requirements such as 'if every and no sequence' or 'if exactly one and exactly five sequences'. The conditions are a bit more complicated than I've indicated here (for example, if we have an empty model then the first of my two examples here is fine), but the basic thrust of the remarks stands.

sequences differing from  $s$  at most in the  $x_1$  position and satisfying  $\theta_1$  and  $\text{RANGE}([D_1 x_1: \theta_1(x_1)])$ , and ... and  $N(D_n)$  sequences differing from  $s$  at most in the  $x_n$  position and satisfying  $\theta_n$  and  $\text{RANGE}([D_n x_n: \theta_n(x_n)])$  also satisfy  $\text{RANGE}([D x: \theta(x)])$ .

Here the determiners are pulled apart and each given control only over its own variable and semantic material (if we gave each control over all the variables and material, (T3-5) would collapse into (T3-4)). We then simultaneously evaluate all of these determiner's effect on their respective ranges.

This evaluative clause also fails. When determining whether a sequence  $s$  satisfied (97'), we would now be able first to vary the 'x' term in order to find a man who owns a donkey, and then independently vary the 'y' term to find a donkey owned by not the 'x' positioned man just chosen but by the occupier -- of whatever type -- of the 'x' position in the original sequence  $s$ . Given this much freedom, we will easily be able to find sequences which fail to satisfy (97'), even when all donkey owners do vaccinate all their donkeys. We will have sacrificed an important part of the original intuition for this method of proceeding -- that the quantifiers need somehow to be evaluated together. We have effectively destroyed any semantic relation they had to each other, rewriting (97') as:

(97-NEW<sub>2</sub>) Every man who owns a donkey vaccinates everything, and some donkey owned by everything is vaccinated by all those things.

which clearly doesn't meet our expectations for a formalization of (97).

#### **§2.2.1.2.4 The Moral of the Story**

Three important points come out of this attempt to find a way to make (97') into a closed formula. First, in order to have any chance of getting the right truth conditions and to avoid the fallacy of trying to construe the existential quantifier of (97') as being a wide-scope universal quantifier, we must find some way to ensure that neither of the two quantifiers of (97') has absolute precedent over the other. Second, the very structure of a recursive truth theory prohibits in important cases -- including the case of (97') -- us from giving each quantifier scope over the other. These first two lessons seem to require us to evaluate the two quantifiers of (97') simultaneously, but our third important lesson is that simultaneous evaluation of multiple quantifiers inevitably suppresses all but one of the involved determiners.

These three lessons, I think, spell the doom of any attempt to construe (97') as a closed formula within the conventional theories of variables and variable binding. However, all is not yet lost. I now want to suggest that there is a positive suggestion hidden amidst the rubble of the prior constructions; that if we take these three lessons seriously we can see the outlines of a new theory of variables and variable binding emerging.

#### **§2.2.1.3 Undistributed Binding and Donkey Pronouns**

That new theory, conveniently enough, is my anaphoric account of variable binding. What we have discovered in the above investigation is that if we are to treat the final 'y' in (97) as a bound variable and get the correct truth conditions, we must (a) genuinely allow the



semantic effect of the 'a donkey' quantifier to extend beyond the reach of the universal quantifier in which it is imbedded and (b) nevertheless respect the fact that the universal quantifier has primacy of evaluation over the existential quantifier. We then run across the problem that (c) realize that at most one of the two existential and universal determiners can be in effect when we evaluate the donkey pronoun, since we cannot simultaneously evaluate multiple determiners.

Under the classical conception of quantification, this is the end of the story, since the determiner is all there is to the quantifier. But under my account, the distributive effect of the determiner is only half the story. Prior to the application of the determiner is the anaphoric binding of the variable. Furthermore, we can -- and in this case *should* -- make sense of a variable which is anaphorically bound but never distributed. When such a variable configuration occurs, the variable will receive semantic content from its anaphoric binder and thus come to refer plurally to all those things satisfying the binder. Since there is no subsequent distribution, the variable will simply remain a plurally referential term.

Consider now how this new category of variable can help explain donkey anaphora. In (97), the final 'it' can be anaphorically bound by 'donkey owned by x'.<sup>87</sup> That pronoun will then come to refer plurally to every donkey owned by x, for various values of x. As the universal quantifier, then, picks out various men who own donkeys, the final 'it'

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<sup>87</sup>I make no attempt in this context to determine what factors control or explain what predicative material acts as anaphoric binder of the donkey pronoun. [Neale 1990] gives a detailed discussion of a related question in his rules for determining the predicative content of a D-type pronoun, although note below that my anaphoric binding will not always match the model of the D-type pronoun.

will in turn pick out all the donkeys owned by those men -- giving the correct truth conditions. 'It', that is, may not be bindable by 'a donkey', but it is bindable by 'donkey'. Other examples of cross-clausal anaphora receive similar analyses. Thus consider:

(111) Just one man drank rum last night. **He** was ill afterward.

(112) If John owns a donkey, he vaccinates **it**.

The 'he' of (111) is anaphorically bound by 'man' and 'drank rum last night' and comes to refer to all those men who drank rum last night (of which there is only one, if the first sentence is true). Retaining that reference in its undistributed state, the pronoun gives the second sentence the appropriate truth content -- that the one man satisfying the conditions set out in the first sentence was ill afterward.

Similarly, the 'it' of (112) is anaphorically bound by 'donkey owned by John' and comes to refer to all such donkeys. (112) is thus (appropriately) interpreted as equivalent to:

(113) If John owns a donkey, he vaccinates the/all donkeys he  
owns.<sup>88,89</sup>

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<sup>88</sup>Note that if John owns no donkeys, the final 'it' becomes an empty referring expression and (112) thus fails to express a proposition. I think naive intuitions are somewhat cloudy on the status of (112) when John owns no donkeys, although there is perhaps some tendency to count it as true. This tendency, if it does exist, may be attributable to a modular approach to the semantic processing of conditions in which any conditional with a false antecedent is immediately counted as true, regardless of the status of its consequent (note, along these lines, that both the Lukasiewicz and Kleene truth tables for three-valued logics count a conditional with a false antecedent and middle consequent as true).

<sup>89</sup>[Lepore and Garson 1983] pursues a doctrine which at first blush closely resembles the position I sketch here. When Lepore and Garson assert that they "will distinguish anaphoric from semantical quantifier scope, ... allow[ing] variables to be bound even though they do not lie in the semantical scope of a quantifier phrase" (327), they certainly appear to be presaging my proposed distinction between the government and distribution of a variable by its anaphor. The similarity, however, is purely illusory. Once one untangles the particulars of Lepore and Garson's discourse representation semantics and of their anaphor

Our initial attempts to accommodate donkey anaphora within an NVT framework were foiled by the incompatible needs to evaluate simultaneously the two quantifiers and to respect both determiners. By

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replacement rules [67] and [69], their views turn out to be disappointingly parochial and superficial.

First note that since Lepore and Garson make no attempt to say what either semantical or anaphoric quantifier scope mean -- what sort of influence is in each case to be exerted on the variable above and beyond the usual (Tarskian) influence -- we must, to the extent that they have two distinct notions in mind, extract these notions from their formal mechanisms. All these mechanisms do, though, is take existential quantifiers (as in the 'a' of 'a donkey') and rewrite them as quantifiers of wide scope -- either existential or universal, depending on the context in which the original quantifier appears. Nothing is said here, and as far as I can see, no insight is provided, into the difference between the 'semantical' and 'anaphoric' scope of the resulting quantifier. Both the rewritten 'x' of the anaphoric pronoun 'it' and the contained 'x' of 'donkey x' seem to be on completely equal footing here.

Second, since all, in essence, the Lepore and Garson account does is rewrite, through a circuitous mechanism, cases of donkey anaphora into the standard doubly-universally quantified ' $(\forall x)(\forall y)((\text{man } x \ \& \ \text{donkey } y \ \& \ \text{owns } x,y) \supset \text{vaccinates } x,y)$ ', it is subject to all the problems of that standard reading. In particular it is inadequately sensitive to (i.e. will produce incorrect truth conditions under) changes in either determiner (of 'man' or of 'donkey') in donkey sentences.

Lepore and Garson recognize this point for the contained determiner, and acknowledge that their account is inadequate for sentences such as:

- (\*87) If John owns all donkeys, then John feeds them.
- (\*88) If John owns most donkeys, then John feeds them.
- (\*91) If John owns few donkeys, then John feeds them.

They insist, however, that these sentences are relevantly different from (97) in that they contain a plural pronoun. This strikes me as a bad position. The plurality of the pronoun in (\*87), (88), and (91) seems to be a purely syntactic phenomenon: we presumably do not want to hold that there is a *semantic* difference between:

- (F38) All philosophers enjoy **their** work.
- (F39) Every philosopher enjoys **his** work.

Lepore and Garson fail to recognize the problem with the large-scope quantifier, and I see no way their account can deal with sentences like:

- (F40) Most men who own a donkey vaccinate it.

They cannot help themselves to restricted quantifiers to handle the problems of 'most'; if they do they lose the internal conditional structure which triggers the universalization of the pronoun 'it' through rule [67].

Broadly speaking, the fragility of Lepore and Garson's account is explained by the paucity of the purported underlying distinction between types of quantifier scope. What we have here is not a new approach to quantifier semantics, but a theory jury-rigged to yield a well-known formalism for (some) donkey sentences.

moving to the two-part binding provided by the anaphoric account, we make these needs compatible. As we will see in chapter 3, the process of anaphoric binding is scope-independent: if 'x' is governed by 'P<sub>1</sub>' and 'y' is governed by 'P<sub>2</sub>', it makes no difference to the truth conditions whether we first attach the 'P<sub>1</sub>'-provided semantic value to 'x' and second attach the 'P<sub>2</sub>'-provided semantic value to 'y', or vice versa. We can thus take all anaphoric binding to occur simultaneously, satisfying the need to avoid having one quantifier dominate the evaluation of the final 'x'. On the other hand, distribution is order- (and hence range-) dependent in its evaluation, but we still have a Tarski-evaluable hierarchy of distribution in (97'), since the distributive power of the 'y' quantifier extends only to 'owns x,y', not to the final 'y'. By pursuing this divide-and-conquer strategy, we can thus construct an extended notion of variable binding which provides hope for accommodation of donkey anaphora in an NVT framework.

To show that the use of undistributed binding does give rise to an account which respects the morals drawn above, note that we now have the desired degree of sensitivity to choice of determiners. If we change the primary determiner:

(110) Few men who own a donkey vaccinate it.

the final 'it' will still, for any choice of x, refer to all of the donkeys x owns. Thus we get the appropriate

(110-NEW<sub>2</sub>) Few men who own a donkey vaccinate all the donkeys they own.

Similarly, changes in the internal determiner of 'donkey' yield the correct truth conditions, since the internal quantification still takes places as normal within the larger 'x' quantifier. Thus sentences like:

(114) Every man who owns three donkeys vaccinates them.

(115) Every man who owns an even number of donkeys vaccinates them.

(116) Every man who owns too few donkeys vaccinates them.

will all be given an appropriate analysis.

By altering what predicative material acts as anaphoric binder of a donkey pronoun, furthermore, we can bring certain examples of pronouns of laziness under the general banner of our account of cross-clausal binding. Consider examples such as:

(117) Most junior professors think they are underpaid.

(118) People who buy books tend to read them.

(119) John bought a brown donkey yesterday and a grey one today.<sup>90</sup>

(117), in addition to its straightforward quantificational reading, has a reading on which it says that most junior professors think (all) junior professors are underpaid. If we take 'they' as anaphorically bound by 'junior professor' and then undistributed, we get exactly these truth conditions for (117). (118), if analyzed according to the usual procedures for donkey pronouns, gives:

(118') People who buy books tend to read the books they buy.

(118'), however, is only one reading of (118). (118) allows for the weaker possibility that the book-buying people read books in general, not necessarily the ones they buy. If we take 'them' to be anaphorically bound merely by 'books', rather than 'books they buy', we get this weaker reading. (119) raises some more complicated issues, but roughly speaking we can assume that there is anaphoric binding between the

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<sup>90</sup>All three examples are borrowed from [Neale 1990].

predicate 'donkey' and a phonetically null variable, which is then subsequently distributed by the determiner 'one' to produce the correct reading:

(119') John bought a brown donkey yesterday and one grey donkey today.

Two intertwined lessons come out of the considerations we have been pursuing here. First, we find that pursuing the intuition that donkey pronouns ought to fall under the general rubric of the bound variable, we are lead naturally to the separation of variable binding and variable distribution by determiner which lies at the heart of the anaphoric account of variable binding. Second, once we accept the anaphoric account of variable binding, one of the obstacles to the NVT falls to the side, as we learn that a troublesome class of anaphoric behaviour of pronouns can be accommodated using the tools of undistributed binding. This convenient convergence and its subsequent success story gives us further reason to accept both the anaphoric account and the NVT which underlies the applicability of the anaphoric account to natural languages.<sup>91</sup>

### **§2.2.2 Further Explorations of the Theory of Undistributed Binding**

While the previous section has shown that there are good prospects for accounting for cross-clausal anaphora using the undistributed binding

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<sup>91</sup>I think, although I will not argue here, that the analysis of donkey pronouns through the devices of undistributed binding has a conceptual advantage over most other accounts available in the literature in that it explains the universal force of the donkey pronoun by deriving that universality from fundamental features of the account of quantification, rather than effectively stipulating the universality by appealing to, say, an unexplained tendency of the pronoun to be reconstructed as a definite description, or an unexplained tendency of otherwise unmarked sentences to be implicitly governed by an 'always' adverb of quantification.

option provided by my anaphoric account, it would lie well beyond the scope of this work either (a) to develop in complete detail an account of cross-clausal anaphora<sup>92</sup>, or (b) to argue that such an account was

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<sup>92</sup>For example, I say nothing here about the interaction between donkey pronouns and adverbs of quantification, which has (especially among those working in the discourse representation theory tradition) been a topic for much discussion. I omit discussion of adverbs of quantification, and other issues commonly raised in conjunction with discussion of donkey pronouns, in part because my primary goal is to sketch the large outlines of the utility of the anaphoric account of variable binding in understanding the functioning of natural language, rather than filling in the complete story. However, I also omit discussion of adverbs of quantification in particular because I am not entirely happy with the standard Lewisian understanding of the function of such adverbs. I am certainly not inclined to take indefinite descriptions as introducing free variables to be bound by the adverb of quantification, as Lewis does. I am less opposed to, but still not entirely comfortable with, taking adverbs of quantification as binding (mysteriously free) event or situation variables.

My one comment on the interaction between adverbs of quantification and donkey pronouns is that my account, should it be married with a standard account of adverbs of quantification, is better suited than most to handle what [Barker (forthcoming)] calls the *double-bind* problem. Barker focuses on sentences such as:

(FN 41) If a man vaccinates a donkey then if it has vitamin deficiency, it usually faints.

in which there is an initial occurrence of a donkey pronoun in a context which places further constraints on which individuals are being talked about followed by a second occurrence of the same donkey pronoun within the scope of an adverb of quantification. Barker observes that such construction creates difficulties for those accounts of cross-clausal anaphora which treat donkey pronouns as variables bound by an implicit 'always' adverbial quantifier (see, e.g., [Kamp 1981], [Heim 1990], [Kamp & Reyle 1993]). The difficulty for such accounts, roughly speaking, is that the second donkey pronoun is (a) taken to be identical in referent to the first donkey pronoun, since they share anaphoric ancestry, but (b) governed by a quantifier ('usually') which does not govern the first donkey pronoun (governed instead by 'always'). The net effect is that, given any particular instance of the 'always' quantifier, there is but a single value of the first donkey pronoun and hence (by the identity observed in (a)) only a single value of the second pronoun. But if there is only a single value of the second pronoun, then the 'usually' quantifier fails to impose a useful condition, since it is either trivially satisfied (when the one donkey faints) or trivially unsatisfied (when the one donkey does not faint). Thus (FN 41) receives the truth conditions of:

(FN 41-NEW) All vaccinated donkeys with vitamin deficiency faint. and gets the same analysis as:

(FN 42) If a man owns a donkey, then if it has vitamin deficiency it sometimes faints.

(FN 43) If a man owns a donkey, then if it has vitamin deficiency it always faints.

Barker concludes from double-bind cases that it is a mistake to analyze cross-clausal anaphora using the quantifier/variable-binding model, and draws the following general lesson:

superior to the other approaches currently available in the literature.<sup>93</sup> Rather than performing either of these two tasks, I want now to concentrate on two difficult cases for my use of undistributed binding. In the process of exploring these two cases, we will (a) uncover some additional linguistic applications of undistributed binding, (b) suggest a slight modification of the original notion of anaphoric binding, and (c) acknowledge some limitations of my approach's ability properly to model the behaviour of natural languages. With these goals in mind, we now turn to look at donkey pronouns (a) with

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It seems to me that such an account must accept i) to iii) below:

- i) The anaphor *it* [the second donkey pronoun] is bound by the quantifier *usually* or is associated with a variable which is so bound.
- ii) The quantifier *usually* is within the scope of a universal quantifier *Q*, and *Q* has large scope with respect to [the second donkey pronoun].
- iii) *Q* binds a variable *x* ... which is connected to the anaphora *it* in such a way that value assignments to *x* determine to some extent the semantic value of *it*.

The crucial task for a semantic approach is to articulate the relation between *it* and *x* referred to in iii) in such a way that it is tight enough to secure the anaphoric relation but still loose enough not to unduly constrain the quantification of *usually*. I cannot profess to have shown that any theory embracing the quantifier/variable-binding model will invariably find itself unable to deal with this balancing act. However, I think it is evident that achieving this balance may be a significant challenge. [33]

Note, however, that the account of cross-clausal anaphora which I have given will not, even if extended to include adverbs of quantification, accept condition ii) above. My account does not need to use universal quantification to explain the universalizing effect of the donkey pronoun. Instead, that universalizing effect falls out as a consequence of the broader theory of undistributed binding. On my account, the donkey pronoun is an undistributed plurally referential term and is perfectly capable of receiving distribution by an adverb of quantification. I take it, then, that Barker's considerations show indirectly that it is better to have an account which adequately motivates the universalizing effect of donkey pronouns rather than simply importing technical machinery to ensure this outcome.

<sup>93</sup>My account is, in fact, equivalent to most others in its empirical predictions, setting aside some anomalous and unusual cases such as those discussed in the previous footnote and in §§2.2.2.2.3, 2.3.4.3.1 below.



existential rather than universal force and (b) embedded in non-extensional contexts.

#### **§2.2.2.1 Existential Readings and Bare Plurals**

In the usual cases, donkey pronouns have a universal force. Thus, for example, in (97) above:

(97) Every man who owns a donkey vaccinates **it**.

The final 'it' picks out *all* donkeys owned by a given man. As we saw above, my account captures this universality automatically, since an anaphorically bound pronoun which is undistributed simply retains all of its referential capacity. However, there are other cases of donkey pronouns in which the pronouns seem to receive an existential force. Consider, for example:

(120) If I have a dollar in my pocket, I will give **it** to you.

An utterance of (120), it would seem, does not commit one to give away every dollar in one's pocket -- just a *single* dollar. Thus the donkey pronoun here does not have universal force, but merely existential force. Such cases pose an obvious problem for my account. If the donkey pronoun in (120) is really an undistributed bound variable, as my account claims that it is, then it ought to refer to all dollars in my pocket. Similar existential readings can be found in sentences such as:

(121) Few men who own a donkey vaccinate it.

(122) No man who owns a donkey vaccinates it.

and perhaps also:

(123) Some man who owns a donkey vaccinates it.

My account provides no ready tools for accommodating these weakened readings of cross-clausal anaphora.

Existential readings of donkey pronouns are a serious problem for my account, and not in the end one I am able to dispel entirely. Nevertheless, I think some progress can be made not only toward seeing that these readings are not quite so damning as they first appear but also toward seeing that they point the way toward an interesting extension of my account. In order to make that progress, I want to proceed on a rather circuitous route which begins with *bare plurals*.

#### §2.2.2.1.1 Bare Plurals

Bare plurals are plural common nouns which are not accompanied by a determiner. Thus, for example, the following sentence contains a bare plural:

(124) **Tigers** have four legs.

Now bare plurals have exactly the syntactic structure one would suspect of a formation to be analyzed semantically using undistributed binding. We find an anaphoric binder -- the common noun 'tiger' -- which can provide semantic content to the variable, but no determiner which could then distribute that semantic value.

If bare plurals do give rise to undistributed binding, then one would also expect bare plurals to give rise to universal readings. In fact, this is exactly what we find in (124), which has the same truth conditions as:

(125) All tigers have four legs.

It would appear, then, that there are two manifestations in natural language of undistributed binding: donkey pronouns and bare plurals.

### §2.2.2.1.1.1 Three Readings of Bare Plurals

Unfortunately, things are not so simple. *Prima facie*, we can identify three types of readings of bare plural sentences, only one of which sits well with the hypothesis that bare plurals employ undistributed binding.

- First, there are the universal readings of bare plurals, such as that of (124) above and that of:

(125) Helium atoms have two protons.

Such readings fit ideally into the undistributed binding framework.

- Second, there are existential readings of bare plurals. In, for example:

(126) Tigers ate my cow.

or:

(127) Films by Hitchcock will be shown tonight.

there is no implication that *all* tigers were involved in the eating, or that *all* Hitchcock films will be shown tonight. All that is required is that *some* tigers ate, or that *some* Hitchcock films will be shown.<sup>94</sup>

- Third, there are what we might call 'typicality' readings of bare plurals. In, for example:

(128) Conservatives have no compassion for the poor.

or:

(129) Liberals have no concern for fiscal realities.

we don't intend to be attributing properties to all conservatives or all liberals. Nor do we intend merely to assert that there are heartless conservatives or starry-eyed liberals. Instead, we intend to assert that

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<sup>94</sup>I set aside for the moment the further question of whether this 'some' condition requires just one or more than one instance for its satisfaction. See §2.2.2.1.2 below for further discussion of this and related issues.

heartlessness is a typical property of conservatives, and economic naiveté is a typical property of liberals. Even the original supposedly universal bare plural:

(124) Tigers have four legs.

might be taken as a typicality reading rather than a universal reading in order to square its truth with the occasional unfortunate three-legged tiger.

'Typicality' readings, I think, should not be taken seriously as an independent class of readings of bare plurals. Instead, such readings should be seen as falling out of general pragmatic considerations. We generally allow a fairly high degree of tolerance when evaluating the truth of other people's utterances. Thus, for example, we accept people's utterances of claims such as:

(130) The beach is deserted today.

(131) No one voted for Mondale in 1984.

even if there are a few people left on the beach and even though there were a few liberal holdouts in Minnesota. As long as what people say is close enough to the truth to sustain the conversational purposes, we tend not to object. Given this toleration principle, we are free to construe what look like 'typicality' readings of bare plurals as universal readings subject to some tolerance. Thus, for example, when we assert that conservatives have no compassion for the poor, we are strictly speaking asserting that no conservative has compassion for the poor, but our toleration principle allows for acceptance of that principle even if there are a few isolated exceptions.<sup>95</sup>

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<sup>95</sup>One might also suspect that 'typicality' readings could be accounted for by taking certain sentences with bare plurals as elliptical for claims with explicit typicality operators. Thus, for example,

If I am right in thinking that we need not admit a separate category of 'typicality' readings of bare plurals, then we are left with exactly two categories: the universal and the existential. Conveniently enough, these two categories of reading match exactly the two categories of reading we found with donkey pronouns. This convergence in interpretative distribution gives good reason to think that the same underlying semantic mechanism is at work in both cases, and the overt syntax of bare plurals gives good reason to think that that mechanism is the mechanism of undistributed binding. Unfortunately, in both cases the troubling existential readings block us from declaring complete success.

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(124) Tigers have four legs.  
might be understood as elliptical for:

(124') Tigers typically have four legs.

I am reluctant to endorse this strategy, however, because there seems to be no explanation of why sentences with bare plurals could not also be elliptical for claims with other adverbs of quantification, as in:

(124'') Tigers seldom have four legs.

(124''') Tigers never have four legs.

(124''''') Tigers only when observed by linguists have four legs.  
None of (124'')-(124'''''), however, represent even remotely accessible readings of (124).

A similar ellipsis strategy might also be proposed to deal with the universal and existential readings of bare plurals. On this strategy, any bare plural would be elliptical for some determiner-N' structure. In a sentence such as:

(125) Helium atoms have two protons.

the appropriate determiner would be 'all':

(125') All helium atoms have two protons.

while in a sentence such as:

(127) Films by Hitchcock are being shown tonight.

the appropriate determiner would be 'some':

(127') Some films by Hitchcock are being shown tonight.

However, I avoid this strategy for the same reasons appealed to above. Were bare plurals genuinely the result of elliptical contractions of full quantified noun phrases, one would expect to be able to find bare plurals which reconstructed using a variety of determiners. But there is, for example, no reading of:

(126) Tigers ate my cow.

on which it means any of the following:

(126') No tigers ate my cow.

(126'') Few tigers ate my cow.

(126''') Most tigers ate my cow.

(126''''') Exactly seven tigers ate my cow.

nor, I think, is there are bare plural sentence which admits readings of these sorts. Only universal and existential readings of bare plurals are available, and the ellipsis strategy is unable to explain this fact.

#### **§2.2.2.1.2 Existential and Minimal Readings, and the Semantics of Number**

I am unconvinced, however, that the existential readings of either bare plurals or donkey pronouns are genuine semantic phenomena. In this section, I will suggest that we have been misled by a number of pragmatic features into believing that there are such categories.

The existential readings of bare plurals are, I think, the more difficult of the two to explain. Perhaps the best one can do here is to hold that what appear to be existential readings of bare plurals are actually universal readings in which we allow what I will call *agent-agency slippage* -- attributing a behaviour to a larger group on the grounds of the actions of a smaller subgroup. Thus, for example, when we say:

(126) Tigers ate my cow.

this should be taken as a universal assertion about all tigers who, through the particular representatives who did the actual consuming, ate my cow. Plurals in other contexts allow this kind of slippage, in which the characteristics of a subgroup are projected onto a whole group. Thus, for example, we can justifiably utter:

(132) The CIA tried to assassinate Castro.

or:

(133) Careful -- they have guns.

even though only some CIA agents were involved in the assassination attempt and even though only some of the mob in front of us is armed.

That even apparently existentially interpreted bare plurals must have a universal potentiality lurking within them is confirmed by sentences such as:

(134) Tigers killed my son, so I have devoted my life to hunting them down and killing them.

Here the initial 'tigers' appears to be existential, but the subsequent occurrences of 'them' have universal force, despite being anaphoric on the initial bare plural. The behaviour of the pronouns here is, I think, most easily accounted for if we assume that the initial 'tigers' is not in fact existential; that it is universal -- blaming all tigers for the killing performed by certain agents of the group -- and passing its universality on to the subsequent pronouns.

Agent-agency slippage is, however, a less plausible explanation for the apparently existential readings of donkey pronouns. Thus it is hard to read sentences such as:

(120) If I have a dollar in my pocket, I will give it to you.

(121) Few men who own a donkey vaccinate it.

as expressing a commitment to give you all the money in my pocket in the person of a single dollar or a widespread failure of donkey owners to vaccinate all their donkeys in the person of a single injected representative. In place of agent-agency slippage, then, I want to suggest two interlocking explanations which go at least some way toward explaining these readings.

First, it seems likely that we are misled by the syntactic number of the pronoun in some cross-clausal anaphora cases. In (120), for example, some of the force toward reading the pronoun existentially seems to derive simply from the fact that it is a singular pronoun -- *it* -- and thus ought to refer to a *single* object -- the one dollar given. Some work on anaphora, picking up on the influence of number, explicitly restricts itself to cases in which the cross-clausally anaphoric pronoun

is singular (see, e.g., [Lepore & Garson 1983], [Pagin & Westerstahl 1993]).

However, it strikes me as a mistake to assume that the number of the pronoun has any semantic implication for the cardinality of the referent of the pronoun. The most obvious example of the semantic irrelevance of number comes from comparing the behaviour of 'all' and 'every'. We surely want to say that the following two sentences are semantically equivalent:

(135) Every philosopher knows logic.

(136) All philosophers know logic.

despite the fact that 'every' requires a singular noun phrase while 'all' requires a plural one. Similarly, when we couple pronouns with 'every' and 'all' we get the equivalent:

(137) Every philosopher admires **his** own work.

(138) All philosophers admire **their** own work.

showing that, at least here, the number difference between singular and plural pronouns is without semantic import. Consider also the following two pairs of semantically equivalent sentences which, for no apparent semantic reason, use pronouns of different number:

(139) Every man who bought a donkey and a mule vaccinated them.

(140) Every man who bought a donkey or a mule vaccinated it.

(141) Every man who bought more than one donkey vaccinated it.

(142) Every man who bought two or more donkeys vaccinated them.<sup>96</sup>

Examples such as these show that number is a purely syntactic feature inducing certain matching behaviour between noun phrases and noun

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<sup>96</sup>These four examples are all taken from [Neale 1990, 239].



phrases or between noun phrases and verb phrases. However, one is often tempted to read a *semantic* condition of singularity into singular pronouns, especially where such pronouns are used in conjunction with actions -- such as giving a dollar from one's pocket -- which we don't expect to be universalized.<sup>97</sup>

Appeal to the deceptive pragmatic effects of pronoun number, however, cannot thoroughly account for the existential readings of donkey pronouns, in part because the phenomenon of such readings is slightly more complex than I have indicated thus far. Consider the following sentences:

(143) A man walked in the park. He whistled.

(144) Two men walked in the park. They whistled.

(145) If I have three dollars in my pocket, I will give them to  
you.

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<sup>97</sup>The semantic irrelevance of number, I think, extends to bare plurals. Sentences with bare plurals can be truly asserted even when there is but a single object singled out by the bare plural. Thus it is true to say:

(126) Tigers ate my cow.

even if my cow was eaten by a single tiger. One way to see this is to note that if one tiger ate my cow and one tiger ate my sheep, it is certainly true to say:

(FN 44) Tigers ate my cow and sheep.

But it would seem odd to assert:

(FN 45) Tigers ate my cow and my sheep but not my cow.

as we would be justified in doing were (126) false due to a tiger deficit.

The claim of the semantic irrelevance of number may seem less plausible in the case of deictic pronouns. It would certainly be odd to assert:

(FN 46) They are philosophers.

while demonstrating a single person. However, one can imagine circumstances -- the blurry-eyed drunk mistaking the many images of one man for a crowd, the Siamese twins mistakenly taken as a single individual -- in which the 'wrong' numbered pronoun is used. In such circumstances, while *some* kind of mistaken has clearly been made, we would surely want to say that a proposition has been expressed, and thus that the number of the pronoun is irrelevant to the cardinality of the referent of that pronoun.

(143) is a (reasonably) straightforward example of an existential reading of a donkey pronoun. While there may be several men walking in the park, the subsequent 'he' picks out only one of those men.<sup>98</sup> Similarly in (144), while there may be several men walking in the park, the pronoun 'they' picks out only two of them. In (145), I obligate myself to give you not all of my dollars and not (merely) some of my dollars, but three of them.

Rather than just existential readings, then, what we have is a phenomenon of minimal readings, in which the donkey pronoun is interpreted not universally but as matching the cardinality constraint of the NP on which it is anaphoric.<sup>99</sup> Grice's maxim of Quantity (see [Grice 1967]) already creates a pull toward minimal readings, even in situations in which there are no anaphoric relations at play. Thus when I say:

(146) I saw two donkeys today.

I will generally conversationally implicate that I saw exactly two donkeys today, since, had I in fact seen three, it would have been more informative to say:

(147) I saw three donkeys today.

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<sup>98</sup>There is, of course, a problem about which of those several men are picked out. See the subsequent discussion of pretense and the discussion of free variables in footnote 59 for two suggestions on dealing with this problem.

<sup>99</sup>One might think that since in a minimal interpretation the donkey pronoun picks up the semantics of the determiner and the N', such interpretations should be understood as pronouns of laziness, in which the pronoun goes proxy for the entire NP on which it is anaphoric. But pronouns of laziness are too weak to capture the anaphoric relations at work, as the lack of equivalence between the following indicates:

(144) Two men walked in the park. They whistled.

(144') Two men walked in the park. Two men whistled.

This pragmatic pull toward minimal readings can then combine with a practice of pretense to help explain apparently minimal readings of donkey pronouns without semantic deviation from the pattern of universal readings. When we hear sentences such as (144) or (145), we tend to engage in an automatic (and easily defeasible) pretense that there is only a single man, or only two men, in the park (despite knowing that this pretense may be false). The donkey pronoun can then be universally interpreted as referring to *all* the men *in the pretense*, thus allowing us simultaneously to take a universal readings of the pronoun (relative to the pretended situation) and capture a minimal interpretation of the sentence.<sup>100</sup>

#### **§2.2.2.1.3 Some Cautionary Remarks on Natural Language Theorizing**

Despite the considerations of the last section, existential readings remain a stumbling block for my account. Especially in the case of bare plurals, my remedies do not entirely dispel the impression that the existential readings are genuine ones, and the fact that the remedial strategies are not the same in the bare plural and cross-clausal

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<sup>100</sup>Following on ideas developed in §2.3 below, we might also take those (apparently) donkey pronouns which give rise to existential or minimal readings as actually being unbound deictic pronouns. Thus in a discourse such as:

(143) A man walked in the park. He whistled.

We might take the pronoun 'he' not as anaphorically bound by lexical material in the first sentence, but rather as deictically referring to an individual raised to conversational salience by the mention in the previous sentence of a man walking in the park. That some pronouns do behave like this is shown by examples such as:

(FN 47) Few students came to the party last night. **They** were busy studying.

in which the desired reading of 'they' -- as referring to the students which did not come to the party last night -- cannot be obtained through anaphoric configurations but can be obtained through the newfound conversational salience of those students.

anaphora cases undermines somewhat the idea that both semantic phenomena are manifestations of the same undistributed binding mechanisms.

Despite these difficulties, I want to persevere in claiming that undistributed binding provides the right tools for understanding cross-clausal anaphora and bare plurals in natural languages. However, I think that the problems raised by existential readings remind us that there are certain hazards inherent in constructing one's formalism with the intent of capturing the full range of a natural language phenomenon. The basic problem is that since natural languages are not static 'found objects', but rather evolving entities at least loosely under our control, they tend to construct epicycles on their own logical devices in a way which can hamper the construction of unified theories.

Assume that we have some logical device in a language, such as proper names. Assume that these proper names have some 'real' logical role in the language -- say referring directly to objects.<sup>101</sup> The difficulty arises when users of the language begin, either implicitly or explicitly, to recognize the existence of this category in their own language and develop theories, of whatever level of crudity or sophistication, about the functioning of these devices. These theories may then get reflected back into use, quite possibly in a way which is not consistent with the 'real' logic of the terms (especially if the theory the users have developed is wrong). Recognition, for example, of the relatively unconstrained circumstances under which we can introduce names may cause some users to adopt a practice of introducing names in contexts in which there is clearly no objects available for the names to

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<sup>101</sup>I will later -- in §2.3.2 -- deny that this is the underlying logic of proper names, but the truth of the matter is irrelevant here.

attach to (i.e., in the context of a fiction). Suddenly the problem of empty or fictional names arises in the language, and the previous logic of proper names -- as tags attached directly to objects -- may now be inadequate to the expanded usage.

Similarly, recognition that there is frequently associated, especially with the names of the very famous, something like a canonical description of the person named may give rise to the telling of stories about individuals known dually as 'Clark Kent' and 'Superman' and people's confused beliefs about them. Moved by such stories, speakers may, when confronted with the information that 'Tully' and 'Cicero' are co-referential, no longer acknowledge that they had believed all along that Cicero was a famous Roman orator and begin to insist instead that they had only believed, up until being fully informed, that Tully was a Roman orator. Of course, one is free to insist that such people are simply mistaken, but there is some degree of truth to the claim that what a language means (and what its logic is) is determined by how it is used, and if this usage becomes dominant enough, one will be forced into accommodating it in one's logic, no matter how foreign it might be to the original 'mere tag' logic of proper names.

Explicit theorization about languages is thus in some way a self-defeating enterprise. Take some well-developed formal result about natural languages, such as [Barwise & Cooper 1981]'s universal U6:

(U6) Monotonicity constraint. The simple NP's of any natural language express monotone quantifiers or conjunctions of monotone quantifiers.<sup>102</sup>

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<sup>102</sup>Quantifier monotonicity will be defined and explored in §3.3.1.3.1 below.

or the transformational constraint of island constraints, which prohibit the move from:

(148) I identified the dog which bit John

to:

(149) \*The man whom I identified the dog which bit

The problem with such constraints is that we are perfectly free to extend or alter our language in such a way that it them. Thus, for example, we could introduce a new determiner 'PRIME' into our language, which has the following truth conditions:

[PRIME x:  $\varphi(x)$ ] $\psi(x)$  is true iff a prime number of  $\varphi$ 's are  $\psi$ 's.

The quantifier [PRIME x: $\varphi(x)$ ] is not a conjunction of monotone quantifiers<sup>103</sup>, but it will now be a part of natural language and violate constraint U6. Similarly, we could easily start allowing movements which violate island constraints. Go around repeating phrases like (149) long enough, and quite quickly people will come to understand what you are saying and perhaps even emulate your usage.

Of course, this doesn't mean that Barwise and Cooper, or GB theorists, are wrong in their theories. So long as their goal is merely to model current linguistic usage, they continue to be right until the types of changes I sketch above are actually implemented in the language. Even then, they can be right again just by making the appropriate modifications in their models. But if that's all there is to the project, then it strikes me as a rather dull one (also a rather pointless one, given the consequent ephemerality of the results). The interesting part of this project lies in trying to read some deep

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<sup>103</sup>It is an infinite disjunction of conjunctions of monotone increasing and monotone decreasing quantifiers.

consequences out of the formalism. We might think, and people have thought, that we can discover something interesting about the nature of language, about our ontological commitments, or about the types of brain structures responsible for language processing by looking at the best semantic theories around.

The correct response to complications of the sort mentioned above, I think, is to isolate a set of core cases of a particular phenomena and concentrate on developing an adequate account of those core cases, acknowledging all along that there may be outlying cases which do not fit into the account thus developed while insisting nonetheless that the account captures the important structural features of the language. This is my strategy in response to the existential readings of bare plurals and donkey pronouns -- even if such readings are legitimate ones, they are deviant cases provoked by misunderstandings by native speakers of the logical devices underlying their own languages, and thus can legitimately be set aside for our current purposes.

The difficulty in this approach, of course, lies in determining which cases lie in the interesting core and which are deviant phenomena to be overlooked. I doubt there is any answer to this difficulty short of actually constructing theories and seeing which cases fit best into the best and most elegant theories. I will leave it to the reader to determine, on this test, whether I have judged rightly in taking existential readings to be a deviant case.

#### **§2.2.2.2 Donkeys Past, Present, and Perspicuous**

The work of §2.2.1 suggests that it may after all be possible to give a bound variable account of cross-clausal anaphora. I now want to

go on to explore some more complex variations of such anaphora involving intensional contexts and use them to show how my account will invoke different explanatory mechanisms, and at times even yield different predictions, from other approaches in the literature. I hope that the end result of these considerations will be to lend some plausibility to the claim that a bound variable account of cross-clausal anaphora possesses theoretical advantages even independently of the quests for unified accounts of all pronoun uses and noun phrases.

#### **§2.2.2.2.1 Tense, Scope, and Binding**

I want to start by considering some minor variants on the standard donkey anaphora example. Instead of the canonical:

(97) Every man who owns a donkey vaccinates it.

let's introduce intensional contexts by looking at:

(150) Every man who owns a donkey vaccinated it.

where the final predicate is put into the past tense. We will consider some difficulties raised by the intersection of cross-clausal anaphora and intensional contexts, and then show how to resolve these difficulties within the context of undistributed binding.

#### **§2.2.2.2.1.1 E-Type Anaphora, Referential Pronouns and Intensional**

##### **Contexts**

[Evans 1977]'s E-type account of cross-sentential anaphora takes donkey pronouns to be referential terms whose reference is provided by extracting an appropriate definite description from the context on which the pronoun is anaphoric. Thus, for example, in:

(151) If John owns a donkey, he vaccinates it.



The pronoun 'it' refers to whatever is denoted by the definite description 'the donkey owned by John'.<sup>104</sup>

Since referring expressions are temporally rigid and hence unaffected by tense operators, the E-type account predicts that the pronoun 'it' will have the same semantic behaviour in each of the following two cases:

(97) Every man who owns a donkey vaccinates it.

(150) Every man who owns a donkey vaccinated it.

Furthermore, in both cases the E-type prediction yields the right truth conditions. In (150), what matters to the truth of the sentence is whether each man vaccinated, in the past, all the donkeys he *now* owns. Thus if a donkey owner who has vaccinated all his donkeys buys a new unvaccinated donkey, (150) becomes false, despite the fact that all of that owner's past donkeys were vaccinated.

However, there are other cases in which the rigidity of reference yielded by the E-type account works to its detriment. There are cases of cross-clausal anaphora in which we want the donkey pronoun to be influenced by the intensional operator in whose scope the pronoun appears. Thus consider:

(152) Right now the mayor of Boston is a Republican, but next year  
he'll be a Democrat.<sup>105</sup>

This sentence has a (preferred) reading in which the 'he' picks out not the current mayor, but the mayor next year. Such a reading is inaccessible under E-type anaphora, since on this account the donkey

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<sup>104</sup>I set aside here problems created by the unwanted uniqueness implication of this definite description. [Lappin & Francez 1993] provides an extension of E-type anaphora on which donkey pronouns can serve as plurally referential terms.

<sup>105</sup>This example is borrowed from [Neale 1990, 188]

pronoun he will refer to the (current) mayor of Boston, and will be unaffected by the future tense operator. *Prima facie*, the availability of such readings is an ominous sign for the theory of undistributed binding, since on my account donkey pronouns, once anaphorically bound, share the referentiality of the E-type pronouns.

**§2.2.2.2.1.2 D-Type Anaphora, Quantifier Scope, and the Logical Form/Surface Structure Interface**

Having seen that the combination of cross-clausal anaphora and intensional contexts can create problems for accounts which treat donkey pronouns as referential, I now want to look at ways of avoiding similar difficulties which are open to other accounts which treat cross-clausal anaphora using the resources of quantifiers and variable binding. I will focus here on the case of [Neale 1990]'s account of D-type anaphora, but the remarks carry over, *mutatis mutandis*, to other variable binding accounts such as [Heim 1990] and [Kamp & Reyle 1993].

Note first that a simple-minded attempt to implement the suggestion of D-type anaphora leads to the wrong truth conditions for (150). According to the D-type account, the pronoun 'it' goes proxy for the numberless description 'whatever donkeys he owns', where 'he' is bound by 'every man who owns a donkey'. Now when, in a formal setting, we substitute this description for the anaphoric pronoun, we obtain:

(150-RQ) [every x: man & [a y: donkey y] owns x,y] P([whe y: donkey y & owns x,y] vaccinates x,y)<sup>106</sup>

But now consider the following situation: Bob, who previously owned three donkeys which he vaccinated himself, has just purchased a new

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<sup>106</sup>Where 'whe' is the numberless description determiner, and 'P' is the past tense operator.

donkey, which he does not vaccinate. Presumably this is a situation in which it is false to say that every man who owns a donkey vaccinated it, because Bob in particular did not vaccinate his new donkey. However, it is still true of Bob that in the past he vaccinated all the donkeys he owns, so (150-RQ) is true (assuming appropriate behavior on the part of the other donkey owners).

More generally, when the D-type account reconstructs the numberless description for which the donkey pronoun goes proxy, it seems it will at times reconstruct that pronoun within the scope of intensional operators. Since the numberless description, like all quantified noun phrases, is not rigid, we will get incorrect readings in those cases in which the donkey pronoun ought to behave rigidly. The challenge for 'proxy' views of donkey anaphora, then, is to explain how the putative semantic content of 'it' escapes the grasp of the temporal operator.

*Prima facie*, there are easy responses available to this challenge. Admittedly a flat-footed application of the theory results in:

$$(150\text{-RQ}) \text{ [every } x: \text{ man } x \ \& \ \text{[a } y: \text{ donkey } y] \text{ owns } x,y] \text{ P([the } y: \text{ donkey } y \ \& \ \text{owns } x,y] \text{ vaccinates } x,y)$$

as an incorrect formal realization of (150), but once we think more carefully about the scope of the introduced definite description, we will see that the problem vanishes. Giving the description a *de dicto* reading, as we have done in (150-RQ), gives the wrong truth conditions, but reading it as *de re*:

$$(150\text{-RQ}_1) \text{ [every } x: \text{ man } x \ \& \ \text{[a } y: \text{ donkey } y] \text{ owns } x,y] \text{ [the } y: \text{ donkey } y \ \& \ \text{owns } x,y] \text{ P(vaccinates } x,y)$$

we get just what we wanted. Here we pick out the donkeys owned at the present time, and then ask if they were vaccinated at some past time. Furthermore, once we see that there are two scope readings available when descriptive pronouns are found within the scope of sentential operators, we will be able to account for the ambiguity which shows up in sentences like (152) above.

I think, however, that it is instructive to consider how this strategy fits in with a broader theory of syntax and semantics. Let's start by taking a perfectly straightforward example of using scope ambiguity as an explanation. As discussed in §2.1.3.1.2 above, the ambiguity of a sentence like:

(98) Every boy read some book.

is accounted for by correlating the one surface structure:

(98-SS) [S[NP[every boy] [VPread [NPsome book]]]

with two logical forms:

(98-LF1) [S [NP every boy]<sub>1</sub> [S [NP some book]<sub>2</sub> [S [NP t<sub>1</sub>]<sub>1</sub> [VP [V read] [NP t<sub>2</sub>]<sub>2</sub>]]]]]

(98-LF2) [S [NP some book]<sub>1</sub> [S [NP every boy]<sub>2</sub> [S [NP t<sub>2</sub>]<sub>2</sub> [VP [V read] [NP t<sub>1</sub>]<sub>1</sub>]]]]]

in which the scope orderings of the quantifiers are laid bare.

Furthermore, we move from the surface structure to the logical form by repeated application of the move- $\alpha$  species QR, which will lift the quantified noun phrases from their surface structure positions to their adjoined positions in LF.<sup>107</sup> Once we obtain the two logical forms for (98), we can appeal to some sort of Tarskian T-theory operating on (98-

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<sup>107</sup>I set aside here my earlier suggestion (§2.1.3.1.2.2) that we take logical form as primary and surface structure as derivative.

LF<sub>1</sub>) and (98-LF<sub>2</sub>) to account for the two possible truth conditions for (98).

However, notice that this general strategy does not obviously succeed when we turn to analysis of (150). Assume (150) has an S-structure something like:

(150-SS) [<sub>S</sub>[<sub>NP</sub>every man [<sub>S</sub>[<sub>NP</sub>who] [<sub>VP</sub>owns [<sub>NP</sub>a donkey]]]]  
[<sub>INFL</sub>PAST] [<sub>VP</sub>vaccinates [<sub>NP</sub>it]]]

Here QR can be applied to adjoin 'every man who owns a donkey' or the past tense inflection [<sub>INFL</sub>PAST]<sup>108</sup> to the original S-node, but neither of these raisings, in either order, yield the desired reading of (150).<sup>109</sup> It is thus not yet clear how scope is to resolve the ambiguity of (150).

The best solution to this problem is to add a new transformational rule which takes the surface structure pronoun to an LF quantifier. The rule here would look much like [Neale 1990]'s (P5), made into a syntactic device:

(P5\*) If, in S-structure, x is a pronoun that is anaphoric on, but not c-commanded by, a quantifier '[Dx: Fx]' that occurs in an antecedent clause '[Dx: Fx](Gx)', then x is replaced in LF with the most 'impoverished' definite description directly recoverable from the

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<sup>108</sup>Assuming that QR, or some similar application of move- $\alpha$ , will allow raising of sentential operators. I leave the details deliberately vague.

<sup>109</sup>Perhaps we could claim that the final 'it' in (12-SS) is in fact quantificational and thus subject to (QR). To do so would be to abandon any pretense at the autonomy of syntax, since the pronoun itself clearly is not marked as quantificational (note that we could just as well have a demonstrative pronoun in that position, or one anaphoric on some name elsewhere in the discourse).

antecedent clause that denotes everything that is both  
F and G.

Applying (P5\*) to (150-SS), we obtain:

(150-LF) [S[NPevery man [S[NPwho] [VPowns [Npa donkey]]]]]  
[INFLPAST] [VPvaccinates [NPthe donkeys he owns]]]

If we apply QR in the following order: (i) to [INFLPAST], (ii) to [NPthe donkeys he owns], and (iii) to [NP[every man [S[NPwho] [VPowns [Npa donkey]]]]], we obtain:

(150-LF') [S[NP[every man [S[NPwho] [VPowns [Npa donkey]]]]]<sub>1</sub>  
[S[NPthe donkeys [NPt<sub>1</sub>]<sub>1</sub> owns]<sub>2</sub> [S[INFLPAST] [S[NPt<sub>1</sub>]<sub>1</sub>  
[VPvaccinates [NPt<sub>2</sub>]<sub>2</sub>]]]]]

which yields the correct truth conditions when plugged into a Tarskian  
T-theory.

What of this solution? I want to make a few points about it,  
although I don't have a knock-down objection to it. First, if we choose  
to obtain our scope ambiguities by introducing (P5\*) as a  
transformational rule, we are forced into a more precise view about the  
position of D-type anaphora in our total linguistic theory than might  
seem desirable. Neale, for example, seems unwilling to commit to whether  
his (P5) is 'a linguistic rule, a processing heuristic, or a mere  
generalization'. However, if we want to use (P5\*) to solve the problem  
of (150), I see no choice but to incorporate the suggestions of D-type  
anaphora into the syntax itself.

More generally, even if (P5\*) itself is not necessary to solve the  
problem, it seems to me that any solution which appeals to scope  
ambiguities had better have the status of a genuine linguistic rule. I  
can't think of any other examples in which mere pragmatic concerns are

able to effect a scope alteration which cannot be carried out on the semantic level, and it seems plausible to suppose that this just is not the kind of operation which pragmatic concerns can give rise to.

Pragmatic factors may help us decide which of several scope readings is appropriate, but they seem to lack the fine-tuning to introduce new scope options.

Second, any serious attempt to work (P5\*) into the rules of a transformational grammar must say something about how it interacts with other rules of the grammar. We have already seen that, if (150) is to be accounted for as a matter of scope, QR must be able to take place after the application of (P5\*). However, there are other examples which seem to require that QR apply before (P5\*). For example, consider the following sentence:

(153) Every man who owns the brother of a donkey vaccinated it.

There is an (admittedly strained) reading of (153) in which 'it' picks out the donkeys whose brother the man owns. Now (153) has the following S-structure:

(153-SS) [<sub>S</sub>[<sub>NP</sub>every man [<sub>S</sub>[<sub>NP</sub>who] [<sub>VP</sub>owns [<sub>NP</sub>the brother of [<sub>NP</sub>a donkey]]]]]] [<sub>VP</sub>vaccinated [<sub>NP</sub>it]]]

If we apply (P5\*) directly to (153-SS), the final 'it' will be converted incorrectly into the description 'the donkeys', since the quantifier 'a donkey' occupies a deceptively low position in S-structure. Only if we first allow (QR) to take effect and obtain:

(153-LF) [<sub>S</sub>[<sub>NP</sub>every man [<sub>S</sub>[<sub>NP</sub>who] [<sub>NP</sub>a donkey]<sub>1</sub>] [<sub>VP</sub>owns [<sub>NP</sub>the brother of [<sub>NP</sub>t<sub>1</sub>]<sub>1</sub>]]]] [<sub>VP</sub>vaccinated [<sub>NP</sub>it]]]

will (P5\*) yield the appropriate 'the donkeys whose brothers the man owns' for 'it'. We will then need to apply Q) again to give the correct scope to the past tense operator.

There may be nothing wrong with having QR and (P5\*) able to apply alternately; that will be a question for one's particular transformational dogma to decide. The point is just that, simply by virtue of choosing a D-type analysis of donkey anaphora, one has committed oneself to some perhaps unexpected consequences for the syntactic theory.

Third, another problem with appealing to scope as an explanatory mechanism in (150) is that it is then a puzzle why we cannot read that sentence with the quantifier introduced by the donkey pronoun taking narrow scope. If we want to appeal to the use of QR to obtain the acceptable reading, we must take seriously the fact that QR is generally taken to be a transformation which applies freely -- given any quantified noun phrase, we always have the option either to raise it or to leave it where it is.

If the default assumption for QR is that it applies freely, we are owed an explanation of why it is required to apply in some cross-clausal anaphora cases. There are some accounts of restrictions on the freedom of QR in some cases. However, these restrictions, such as island constraints, are all cases in which QR is prevented from taking effect. These cross-clausal anaphora cases seem to be the only ones in which QR is required to take place. This requirement thus strikes me as somewhat ad hoc, and I think that, absent a compelling story about why QR must



occur in these cases, we should seek an explanation which does not involve scope manipulations.<sup>110</sup>

### §2.2.2.2.1.3 Undistributed Binding and Partial Binding

Accounting for the interaction between donkey anaphora and tense puts a certain amount of stress on both the E-type and D-type accounts, but for opposite reasons. E-type accounts are too inflexible to account for those readings of sentences like (152) in which the pronoun seems to inherit more than just a referent from its antecedent. D-type accounts, on the other hand, provide too much freedom, and are pressed for an explanation of the inaccessibility of some readings. I now want to indicate that, by treating the 'it' of (150) as a bound variable within the context of my extended theory of binding, we can steer a middle course between these two options.

Undistributed binding holds that in a sentence like (150), the predicates 'donkey y' and 'owns x,y' pass their semantic values on to

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<sup>110</sup>There is also the theoretical possibility of past-tensed donkey sentences which have readings unaccountable for in terms of scope. The general form of the worry is this: assume we have a donkey sentence of the form:

(FN 48) Every F who G's an H vaccinated it.

D-type anaphora can use scope considerations to account for a reading in which 'the H's that he G's' are determined at the present time or at the past time (should such a reading ever be called for). However, we might sometimes have a reading in which we want, say, Hness to be evaluated at the present time and Gness to be evaluated at the past time. Convincing examples are hard to come by, but consider:

(FN 49) Every man who owns a popular record paid too much for it.

We might take (FN 49) as calling for the final 'it' to be interpreted as 'the records he now owns that were popular (when he bought them)'. If this is right, it's a reading which no choice of scope can account for:

(FN 50) [every x: man x & [a y: popular-record y] x owns y] P(??)  
[the y: popular-record y & x owns y] P(??) x paid-too-much-for y

Neither of the two possible positions for the past tense operator get the right reading -- the definite description somehow needs to straddle the tense operator, but this is impossible in standard syntax.

the final 'it', and that those semantic values will then cause 'it' to refer to some objects. I have, however, said little thus far about what kind of thing the semantic values of these predicates are.

There is a lack of settled opinion in the field about the appropriate answer to this question, and one of the hopes of the anaphoric account of variable binding is to shed some light on the area. We might use the semantics of plain first-order logic and assign to each predicate an extension. If we take this approach, we get an account which looks much like E-type anaphora<sup>111</sup>, yielding the same predictions. We thus get some past-tensed donkey sentences, such as (150), right, but fail with others, like (152), to get all the available readings.

Second, we could take an intensional approach and allow predicates to pick out a function from times (and worlds, etc. if we want) to extensions. An approach something like this is presumably necessary to analyze tense in any case, independent of issues regarding anaphora. Unfortunately, here again we seem to get the wrong predictions, but in the reverse direction. If in (150) we merely pass along an intension to the pronoun, it will then interact with the past tense operator to yield the unacceptable reading:

(150-NEW) Every man who owns a donkey vaccinated the donkeys he  
owned.

Our theory will look like D-type anaphora in which the description always takes narrow scope (and we will, of course, have no scope to appeal to, since 'it' remains syntactically simple).

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<sup>111</sup>With the addition, I would like to think, of a more elegantly motivated story about how pronouns come to refer as they do.

I'd like to suggest that we think, when considering (150), not about the semantic value of the predicates considered as types, but of the semantic values of these particular tokens of the predicate types. We could see the predicate type as supplying the intension-oriented function I described above, and then let the instance of the type have as its semantic value the function plus values for some or all of the places in the function. Limiting ourselves to time as the sole dimension of intensionality for the moment, consider the sentence:

(154) I vaccinated Eeyore last Wednesday.

The predicate type 'vaccinate' will be a function from times to pairs <vaccinator, vaccinated>, but this particular token of 'vaccinated' will have as its semantic value that function plus the time 'last Wednesday'. In virtue of this pair of function and time, the token will pick out an extension, and the sentence will then be true iff the pair <I, Eeyore> is in that extension.

So assume that associated with the above inscription of (150), the predicate tokens 'donkey y' and 'owns x,y' both have as semantic values first a function representing their intension and second a time at which that intension is to be evaluated.<sup>112</sup> Now if we pass on these semantic values to the final 'it', we end up determining the (undistributed) reference of 'it' using the intensional function associated with the phrase type 'the donkeys he owns' and the (present) time of utterance. This combination of semantic values gets 'it' referring, correctly, to the donkeys the man currently owns, and requiring that those very donkeys have been vaccinated in the past.

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<sup>112</sup>Since both predicates appear in the present tense in (150), the times are presumably supplied by the time of inscription.

However, nothing requires us to pass on exactly this group of semantic values. We might represent formally, in an ad hoc notation, the proposed reading of the previous paragraph by:

$$(150\text{-RQ}^*) [\text{every } x: (M^{*i}, t_1^j)x \ \& \ [\text{a } y: (D^{*k}, t_2^l)y] \ (O^{*m}, t_3^n)x, y] \\ P( (V^{*o}, t_4^p)it^{k,l,m,n})$$

where predicate tokens have been replaced with the pair of their intensional function and their time of evaluation, and where matched superscripts indicate anaphoric links. This is a situation of total bondage -- where each predicate token passes along all of its semantic value to the bound pronoun. But we could also have partial binding, such as:

$$(150\text{-RQ}_1^*) [\text{every } x: (M^{*i}, t_1^j)x \ \& \ [\text{a } y: (D^{*k}, t_2^l)y] \ (O^{*m}, t_3^n)x, y] \\ P( (V^{*o}, t_4^p)it^{k,m})^{113}$$

where we pass on the function, but not the time.

Of course, taking (150-RQ<sub>1</sub><sup>\*</sup>) to be a formal representation of (150) gives us once again the wrong reading of (150) equivalent to the narrow scope D-type reading. Cases like (152), however, seem to respond well to partial bondage. Recall that in:

(152) Right now the mayor of Boston is a Republican, but next year  
he'll be a Democrat.

there is a reading in which 'he' picks out the future mayor of Boston.

Consider, then, the following formal representation of (152):

$$(152\text{-RQ}^*) [\text{the } x: (M^{*i}, t_1^j)x] (R^{*k}, t_2^l)x \ \& \ \text{Next\_Year}((D^{*m}, t_3^n)he^i)$$

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<sup>113</sup>To be more complete, I should omit the value  $t_4$  and indicate a time linkage between the predicate 'vaccinates' and the past-tense operator. I've chosen to suppress this kind of detail -- just read  $t_4$  here as being the time determined by the pastness of the operator.

By creating an anaphoric link only between the intensional function, and not the time component, of 'mayor of Boston' and 'he' (partial binding), we leave the time of evaluation for 'he' unspecified. That time is thus free to be determined by the tense operator 'next year', giving the desired reading in which we mean 'whoever is mayor next year will be a Democrat'.

Of course, if we want the other reading (the 'wide scope' reading), we can get it by using full rather than partial binding on 'he':

$$(152-RQ_1^*) [\text{the } x: (M^{*i}, t_1^j)x] (R^{*k}, t_2^l)x \ \& \\ \text{Next\_Year}((D^{*m}, t_3^n)\mathbf{he}^i, j)$$

Now 'he' inherits the current time from the earlier predicate, so we end up talking about the future political party of the very man who is now mayor of Boston. In general, we can capture a wide variety of readings when pronouns are embedded within intensional operators by choosing the appropriate degree of partial or full binding.<sup>114</sup>

Apart from its empirical successes sketched above, I think there are at least two things to say in favor of using undistributed binding combined with partial binding to account for cross-clausal anaphora.

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<sup>114</sup>If I am right that there are sentences, like my (FN 49) above, which have readings in which one predicate is to be evaluated at the time of utterance and another at a time determined by a tense operator, partial binding can account for the phenomenon. Consider the following formalism corresponding to (FN 49):

$$(FN\ 49-RQ^*) [\text{every } x: (M^{*i}, t_1^j)x \ \& \ [a\ y: (P^{*k}, t_2^l)y] (O^{*m}, t_3^n)x, y] \\ P( (T^{*0}, t_4^p)\mathbf{it}^{k,m,n})$$

By binding 'it' with the time index from 'owns' but not from 'popular', we allow 'it' to pick out those things which he currently owns (due to the anaphoric relation on time  $t_3$ ) but which were popular records in the past (as determined by the presence of the past tense operator). Thus the framework of partial binding gives us the freedom to pick out readings inaccessible using only the mechanisms of scope.

First, it looks like we need something like partial binding even outside the context of my theory. Consider the sentence:

(155) Mary, Bob, and Albert went to see *Mouchette*, and then they dropped her off and went to Chez Panisse.

There is a reading here in which the pronoun 'her' refers to Mary, and 'they' refers only to Bob and Albert. Formally, we have:

(155')  $Mary_i$ ,  $Bob_j$  and  $Albert_k$  went to see *Mouchette*, and then  $they_{j,k}$  dropped  $her_i$  off and went to Chez Panisse.<sup>115</sup>

Neither 'they' nor 'she' are bound by the entire semantic content of the sentence's subject -- we have some sort of partial binding here.

Second, there are no potentially troublesome syntactic consequences to an extended binding approach to donkey anaphora and cases like (12) and (24). Unlike quantifier raising, which achieves its semantic effects through syntactic manipulations between S-structure and LF, anaphoric linkages are generally taken to be inscribed already in D-structure and to be preserved through transformations to LF,<sup>116</sup> where they then have their semantic effect. It seems plausible enough to suppose that the semantic links posited by extended binding function the same way. Thus we don't need to worry about how transformations necessary to get desired readings of sentences might detrimentally interact with other transformations to which we are committed -- all the semantic work is done at the ground floor.

Of course, we still lack a story about why the 'narrow scope' reading of (150) is inaccessible -- why can't we partially bind 'it' in

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<sup>115</sup>See [Higginbotham 1983] for more details on such cases.

<sup>116</sup>Or, on my revised picture of syntactic levels advanced in §2.1.3.1.2.2, inscribed in LF and preserved through the transformation to SS.

(150) and have it refer to the donkeys he used to own? However, unlike theories reliant on scope mechanisms to capture various readings, whose scope-producing mechanism of QR and Move- $\alpha$  has a prior commitment to universality of application, we are positing a new type of semantic phenomenon, and thus have greater freedom simply to insist that partial binding is impossible in some syntactic constructions.

#### **§2.2.2.2.2 Intensions, Anaphora, and the Semantics of Predicates**

Having set out the basics of partial binding, I now want to use some cases of donkey anaphora combined with modal operators and with vagueness to illustrate this approach may be useful in making discoveries about the semantics of predication. Consider sentences such as the following, in which the final 'it' is within the scope of a modal operator:

(156) Every man who owns a donkey necessarily vaccinates it.

(157) Every man who owns a donkey is required to vaccinated it.

(158) Every man who owns a donkey probably vaccinates it annually.

(157) and (158) are the more natural English sentences, but I will concentrate on (156) in my formal discussion for reasons of simplicity.

In each of these sentences, there is a reading in which the modal operator to have scope only over the final 'vaccinates it', not over the entire sentence:

(156-RQ) [every  $x$ : man  $x$  & [a  $y$ : donkey  $y$ ] owns  $x,y$ ] vaccinates  
 $x,y$

Such readings are not entirely implausible. Take, for example, (158). I might be going through a list of all the donkey owners, commenting on the cautious nature of each, and finally conclude "So, every man who

owns a donkey probably vaccinates it annually." Here there is no commitment to the (probable) behavior of non-actual donkey owners, so the modal operator cannot take scope over the entire sentence.

There is a '*de dicto*' reading of (156) in which it means 'Every man who (actually) owns a donkey is such that in every possible world he vaccinates whatever donkeys he owns there.' The reading is somewhat strained (perhaps because (156) is such an unnatural sentence in the first place), but there are good reasons to think it is available. An equivalent reading is clearly available in examples such as the following from [Karttunen 1976]:

(159) Mary wants to marry a rich man; he must be a banker.

Here 'he' most plausibly picks out a different man in each world (whatever man Mary marries in that world). These *de dicto* readings can be captured using partial binding in a configuration like:

(156-RQ\*) [every x: (M<sup>\*i</sup>,@<sup>j</sup>)x & [a y: (D<sup>\*k</sup>,@<sup>l</sup>)y] (O<sup>\*m</sup>,@<sup>n</sup>) x,y] □  
vaccinates **it**<sup>k,m</sup> <sup>117</sup>

where the final 'it' is partially bound only by the intensional functions of the predicate tokens and not by their worlds of evaluation.

We can add another twist to the situation by introducing vagueness. Vagueness is a messy matter, and an area in which the current state of theories strikes me as being substantially inadequate. I need to establish a few facts about the way vague statements can function, but I want to try to avoid committing myself to any particular account of how vagueness works.

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<sup>117</sup>Where I am specifying the intensional function of each predicate and the world of evaluation for that function (and where, of course, '@' indicates the actual world).



Predicates frequently (always?) have a certain degree of vagueness to them. One result of this is that there are sentences whose truth value is either unclear or indeterminate. For example, if I say:

(160) Dianne Feinstein is a liberal.

it is not immediately clear whether what I have said is true or false. There are people who are undeniably liberals, such as Ron Dellums, and people who certainly are not liberals, such as Pat Buchanan. Dianne Feinstein is neither of those. She lies in that gray area in the middle. While it may be controversial exactly what to say about (160), it seems plausible that we can sometimes use (160) to say something true and sometimes use it to say something false. If, following the 1994 elections, we are surveying the wreckage of the left in Congress and trying to enumerate remaining liberals, it seems not unreasonable to include Feinstein on the list. If, on the other hand, we are at a meeting of the Peace and Freedom party, searching for a liberal to endorse in 2000, Feinstein does not seem to make the cut. It seems we have, within the gray area of vagueness, the ability to set, on particular occasions, standards of vagueness resolution which determine what counts and what doesn't count as falling under a predicate.

Furthermore, note that this process of vagueness resolution is directly tied to predicate tokens. One can imagine situations in which I could say:

(161) Dianne Feinstein is a liberal and Ann Richards is a liberal.

and apply different standards of vagueness resolution to each occurrence of 'liberal'. They would admittedly be bizarre situations, perhaps involving extremely long utterances in which I move from place to place, but they could occur. However, if I say:

(162) Every liberal has a secret desire to destroy his own party.  
 I can't apply differing standards of vagueness resolution to the initial occurrence of 'liberal' and to the later 'his' bound by 'every liberal'.<sup>118</sup>

Now return to (156). There isn't much room for vagueness in the predicate 'donkey' (although there is some), so let's concentrate on the concept of ownership, which certainly has considerable room for vagueness. When I say:

(156) Every man who owns a donkey necessarily vaccinates it.  
 and apply some standard of vagueness resolution to 'owns' (and to 'donkey'), that very same standard of vagueness resolution *must apply* when evaluating the final 'it'. I can't change what counts as ownership, or what counts as a donkey, as I move from world to world. According to my account theory, (156) is formally regimented as:

$$(156\text{-RQ}^*) \text{ [every } x: (M^{*i}, @^j)x \ \& \ [a \ y: (D^{*k}, @^l)y] \ (O^{*m}, @^n) \ x, y] \ \square \\ \text{vaccinates } \mathbf{it}^{k,m}$$

Here we pass on to 'it' the exact same intensional function which is associated with the initial predicate occurrences. If we assume, then, that the standard of vagueness resolution is built into this function, that same standard will be passed on to the final 'it', and we will get the appropriate reading.

One lesson to be drawn from this example is that vagueness resolution is not just a matter of specifying an appropriate extension. We can't get what we want simply by explicitly listing which objects are to count as falling under the predicate and which are not. For

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<sup>118</sup>This observation has connections to the discussion of the Fidelity Principle in the next section.

specifying an extension in order to resolve vagueness will be of no help when we want to make sure that the possibly owned possible donkeys picked out by 'it' conform to the same standards as the actually owned actual donkeys. Whatever our final concept of vagueness resolution is, it must be a concept which makes sense of the idea of using the same standard of resolution at multiple possible worlds. Without tracing out the ability of anaphoric links to pass on semantic values, we would not have been able to see this feature of vagueness resolution.<sup>119</sup>

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<sup>119</sup>There may be a reading of (156) available on which the standard of vagueness resolution is in fact allowed to change from world to world. We would then mean by (156) something like:

(156-NEW) Every man who owns a donkey is such that in every world, he vaccinates all those donkeys to which he stands in what, relative to the standards of that world, counts as the relationship of ownership.

Thus, for example, it might seem right that, if Bob is a man who owns a donkey, had Bob lived in an 'old west' society, he would have to vaccinate just those donkeys which carried his brand, while if he lived in a communal-tribal society, he would have to vaccinate all the donkeys owned by his tribe.

If this is correct, reading (156-RQ\*) would no longer be adequate, since we would not in fact want to pass on the intension determined by the standard of vagueness employed in the actual world. Instead, we would want the context of each world to be able to supply a new standard of vagueness. The best way to handle such a case would be to add a new level of intensionality to predicate semantics. Associated with a predicate we would have a function from standards of vagueness resolution to particular intensions (which would themselves be functions from times, worlds, etc. to extensions). We could thus represent the original reading of (156) (in which the standard of vagueness resolution does not change from world to world) as:

$$(156-RQ_2^*) \text{ [every } x: (M^{*i}, v_1^0, @j)x \ \& \ [a \ y: (D^{*k}, v_2^p, @l)y] \ (O^{*m}, v_3^q, @n) \ x, y] \ \square \ \text{vaccinates } \mathbf{it}^{k,p,m,q}$$

where  $v_1, v_2$ , and  $v_3$  are the standards of vagueness resolution supplied with the predicate tokens of 'man', 'donkey', and 'owns' appearing in (156).

We could then use partial binding to account for the possibility of the suggested alternative reading of (156). By formalizing it as:

$$(156-RQ_3^*) \text{ [every } x: (M^{*i}, v_1^0, @j)x \ \& \ [a \ y: (D^{*k}, v_2^p, @l)y] \ (O^{*m}, v_3^q, @n) \ x, y] \ \square \ \text{vaccinates } \mathbf{it}^{k,m}$$

we would leave the standard of vagueness used in evaluating 'it' unbound and free to vary from world to world. If this is right, I take it as a prime example of how exploration of variable binding can show what kinds

### §2.2.2.2.3 The Present Tense and the Fidelity Principle

A statement in the present tense is, generally, to be evaluated at the time at which it is made. If I now say:

(163) Clinton's relation with the Republicans isn't nearly as strained as people think.

what is relevant is Clinton's relation with the Republicans *now*, not his relation 18 months ago, or his position when the 1998 elections roll around. This doctrine is fine as far as it goes, but there's a potential problem. Statements, or speech acts in general, typically occupy some non-zero time interval. Given that, relative to what point in that interval is the truth of the statement to be evaluated? That is, does (163) need to be true when I start saying (typing) it? when I finish? at some arbitrary point in the middle? all the way through, or merely at some point during the act?

In most cases, these fine points won't matter, both because our utterances don't last very long anyway and because the situation we're trying to describe isn't changing quickly enough for the choice of time of evaluation to affect truth value. However, I think there are cases in which there are clear answers to this question. First, consider the announcer for a time service saying:

(164) The time is ten thirty-seven and ... 25 seconds.

with the emphasis on '25'. (164) is to be evaluated at the time when the announcer says '25'. (Otherwise, we get the wrong truth conditions).

Now imagine that right after uttering (164), the same announcer says:

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of semantic values (here, standards of vagueness resolution) must be built into predicates).

(165) The time is ten thirty-seven and ... 30 seconds.

(165), of course, is to be evaluated at the time of utterance of '30'.

But what if the announcer were to start sticking connectives ('and', perhaps) between his time proclamations:

(166) The time is ten thirty-seven and 25 seconds and the time is ten thirty-seven and 30 seconds.

At what time is (166) to be evaluated? Well, any particular choice of time will make it come out false.

It would be strange for (166) to come out false, though, since both (164) and (165) were true, and we have merely joined them with 'and'. I think what we have to say here is that there are two times at which (166) needs to be evaluated: first, the time of utterance of '25', and second, the time of utterance of '30'. More generally, it seems that every predicate requires its own time of evaluation in order to determine an extension (of objects satisfying the predicate at that time).

In (166) there are two predicates: the two occurrences of 'is ...'. We could thus assign to the first occurrence the time at which '25' is uttered (call this  $t_{25}$ ) and to the second occurrence the time at which '30' is uttered ( $t_{30}$ ). When combined with the intensional function associated with 'time', each of these times determines an extension for the predicate. The first extension includes the time 10:37.25, and the second the time 10:37.30, so the sentence comes out true, as we wanted.

In light of these considerations, I want to suggest that, in present tense sentences, there is no guarantee that different predicates will be evaluated at the same time. This suggestion leads to some

surprising consequences. Statements which appear to be logical falsehoods, such as:

(167) The president is not the president.

can come out to be true (by evaluating the two occurrences of 'president' with respect to different times). But if we imagine the right circumstances, it's not clear that this is a bad result. Assume that Kenneth Starr actually produces the goods on Whitewater and Clinton is forced to resign. One can imagine a solemn resignation ceremony, with Dan Rather intoning:

(167) The president (as Clinton approaches the podium) is not the president (as Clinton surrenders the office)

Of course, the mere fact that the different predicates can take different times of evaluation does not mean that they will. In fact, for the reasons I indicated earlier, it rarely matters exactly what time we assign to predicates, so I assume that there is a default assumption that they are all assigned the same time (when they are all in the present tense). It is this assumption (among other things) which makes (167) look so odd. All that matters for our current purposes is that there *is* an available reading of (167) on which the two occurrences of 'president' are evaluated at different time indexes, not that it *must* be so read.

Next, notice that bound variables do not allow introduction of new times of evaluation. Compare the following two sentences:

(168) The president left his office.

(169) The president left the president's office.

(168) is subject to the kind of reading I indicate above, in which the second occurrence of 'president' is evaluated at a different time from

the first and thus picks out a different person. However, (169) does not have such a reading available. Even if we imagine a situation like above, in which an utterance of (168) is stretched over the resignation of the president, 'his' cannot be used to pick out the new president.<sup>120</sup> (Although, tellingly, 'his office' could refer to the office associated with whatever position the disgraced president was assuming after his resignation.)

Now consider the implications of these considerations for donkey anaphora. Take the canonical:

(97) Every man who owns a donkey vaccinates it.

and ask: can the donkeys which need to be vaccinated be different from those which are picked out by the initial occurrence of 'donkey'? It seems clear that the answer is no. We must, just as in the earlier cases of variable binding, maintain exactly the same group of individuals in the pronoun as were originally picked out by the predicates.

But according to D-type anaphora, (97) is equivalent in meaning to:

(97-RQ-D) [every x: man x & [a y: donkey y] owns x,y] [the y:  
donkey y & owns x,y] vaccinates x,y

Here there are two separate occurrences of the predicates 'donkey' and 'owns', each of which will be capable of receiving its own time of evaluation, so if any objects during the utterance of (97) change their status either as donkeys (unlikely, admittedly) or as things owned by x,

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<sup>120</sup>'His office', however, can probably still be used to pick out the Oval Office, simply because we frequently allow 'the F' ('his F', 'her F')-type phrases to be read as 'the former F' or 'the future F'. Thus compare:

(FN 51) His wife left him.

we could assign different times of evaluation to two predicates of the same type and get incorrect truth conditions for (97).

Analyzing cross-clausal anaphora using extended binding, on the other hand, does not allow this problem. (97) is then formally represented as:

$$(97\text{-RQ}^*) [\text{every } x: (M^{*i}, t_1^j)x \ \& \ [a \ y: (D^{*k}, t_2^l)y] \ (O^{*m}, t_3^n)x, y] \\ (V^{*o}, t_4^p)\text{it}^{k, l, m, n}]$$

The times of evaluations of the initial (and only) occurrences of 'donkey' and 'owns' are passed on to the pronoun 'it', so the spurious reading is unavailable.

There is a general lesson to be learned from the above considerations, which I will call the Fidelity Principle:

**(Fidelity)** Do not introduce new lexical material into utterances when giving semantical analyses.<sup>121</sup>

D-type anaphora goes wrong precisely because it puts into the sentence new lexical material -- new predicates and quantifiers -- where there was originally only a variable. This new lexical material then assumes a life of its own, and gives rise to interpretive possibilities which were not present in the original sentence. We need, in order to get the right analysis, to take more seriously the idea that the final 'it' is anaphoric -- that its semantic function is *fully determined* by what has already gone on in the sentence. My bound variable approach to cross-clausal anaphora honors this intuitive anaphoric relation; D-type anaphora, it seems, does not.

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<sup>121</sup>Further reason for adhering to the Fidelity Principle will be developed in §2.3.4 below.



### §2.3 Noun Phrases and Free Variables

In this section I will examine the linguistic utility of the last possible configuration of the variable according to the anaphoric account -- the unbound and undistributed variable. I will argue that the use of unbound and undistributed variables as semantically empty syntactic placeholders gives rise naturally to a combination of semantically expressed propositional matrices with pragmatic considerations to express singular propositions.

The focus on this section will be on singular terms, which are characterized semantically by being directly referential (in the sense of [Kaplan 1977]) and rigid, and syntactically by being simple, or unstructured.<sup>122</sup> I will assume here that singular terms are all one of: proper names, demonstratives, indexicals, and deictic pronouns, although this list may leave out some marginal cases.<sup>123</sup> My claim is that all singular terms are best understood within the context of the anaphoric account as unbound and undistributed variables (which I will henceforth simply call 'free variables').<sup>124</sup> This claim will clearly trivially

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<sup>122</sup>As hinted in footnote 44 above, my view is that apparent counterexamples to the claim that singular terms are syntactically simple, such as proper names like 'Dartmouth' and 'the Holy Roman Empire', should be treated as syntactically unstructured idioms. A name like 'the Holy Roman Empire', that is, should not be treated as containing the determiner 'the' as a subcomponent any more than 'then' should be treated as containing that determiner. See [Neale 1993] for a more extended discussion of the syntactic simplicity of singular terms.

<sup>123</sup>Marginal cases may include wh-pronouns, which I suggest in footnote 62 above and 198 below might be amenable to treatment as phonetic variants on demonstratives and deictic pronouns, 'that'-clauses, which might be treatable paratactically in the manner of [Davidson 1968] (although I have considerable doubts about the success of the Davidsonian treatment), and reciprocal pronouns, for which I have no well-developed suggestion. I will proceed on the assumption that these marginal cases can eventually be accommodated in the strategy developed in the main text.

<sup>124</sup>The reader should keep in mind, however, that variables free in my sense are not exactly the same as variables free in the traditional

explain the syntactic simplicity of singular terms. Whether the claim can also explain the semantic behaviour of singular terms is a much more substantial question, one which the rest of this chapter will be devoted to answering affirmatively. Once we have seen that such an explanation is forthcoming, though, we will see that we have made considerable progress toward a unified treatment of noun phrases by (a) providing a unified account of all singular terms and (b) showing how singular terms relate to quantified noun phrases as a special case of the quantifier/bound-variable paradigm (and in particular, showing why pronouns bridge the gap between quantified and singular noun phrases).

### **§2.3.1 Deictic Pronouns**

I argued in §2.1.3 above for what I called the Naive Variable Theory, or NVT, which holds that all pronouns in natural language are variables. Focusing for the moment on the Weakened Naive Variable Theory (WNVT), which restricts its claims to third-person pronouns (we will take up the full NVT in §2.3.4), we identified three problem cases for this identification of variables and pronouns: (i) cross-clausally bound, or 'donkey' pronouns, (ii) pronouns anaphoric on proper names and other singular terms, and (iii) deictic pronouns. Case (i) was dealt with extensively in §2.2, and case (ii) will be treated in passing in §2.3.3. In this section I want to defend the claim that deictic pronouns can be accommodated within NVT.

Consider the following pair of sentences:

(98) Every boy read some book.

(170) He read some book.

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sense. Wherever conflation of the two notions is a danger, I will take care to specify which is intended.

(198) has a quantified noun phrase in its subject position; (170) has a deictic pronoun. My suggestion is that (98) and (170) differ only in the removal of the quantifier from the quantified noun phrase. The logical form of (98) (giving the universal quantifier wide scope) is:

(98-LF1) [<sub>S</sub> [<sub>NP</sub> every boy]<sub>1</sub> [<sub>S</sub> [<sub>NP</sub> some book]<sub>2</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub> [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> t<sub>2</sub>]<sub>2</sub>]]]]]

If we remove the 'every boy' quantifier from this logical form, we obtain:

(171-LF) [<sub>S</sub> [<sub>NP</sub> some book]<sub>2</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub> [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> t<sub>2</sub>]<sub>2</sub>]]]]]

which contains the free trace 't<sub>1</sub>'. Recall from §2.1.3.1.2 that we suggested that some traces in logical form will manifest in surface structure as pronouns; thus the logical form:

(99-LF) [<sub>S</sub> [<sub>NP</sub> every boy]<sub>1</sub> [<sub>S</sub> [<sub>NP</sub> some book [<sub>S</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub> [<sub>VP</sub> liked]]]<sub>2</sub> [<sub>S</sub> [<sub>NP</sub> t<sub>1</sub>]<sub>1</sub> [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> t<sub>2</sub>]<sub>2</sub>]]]]]

corresponds to the surface structure:

(99-SS) [<sub>S</sub> [<sub>NP</sub> every boy] [<sub>VP</sub> [<sub>V</sub> read] [<sub>NP</sub> some book [<sub>S</sub> that [<sub>NP</sub> he] [<sub>VP</sub> liked]]]]]

with the second occurrence of the trace 't<sub>1</sub>' in LF mapping into the pronoun 'he' in SS. The same phonetic realization of traces as pronouns will be at play in the move from (98) to (170): when we remove the initial quantifier 'every boy' the unbound trace 't<sub>1</sub>' manifests in surface structure as a pronoun, and we get (170) from the logical form (171-LF).

To add plausibility to this syntactic connection between (98) and (170), note that (98) can, without change in meaning, be rewritten as:

(98-NEW) Every boy is such that he read some book.

By simply detaching the quantifier from the head of this sentence, we obtain (170):

(170) He read some book.

By detaching a quantifier, we should be creating a free variable. If (171-LF) represents the LF of (170), this is exactly what we are doing.

The suggestion, then, is that deictic pronouns be treated as free variables. There is, moreover, a semantic plausibility to this suggestion. Under a standard Tarski-style truth theory, (171-LF) will fail to receive truth conditions, but will still receive satisfaction conditions. Abstracting away from the machinery of sequences, we can say that (171-LF) will be satisfied by all and only those objects that read some book. But surely we can say something similar about (170). It is satisfied by ('true of') all and only those boys who read some book.<sup>125</sup> These semantic comments are, I think, entirely uncontroversial as applied to the sentence *type* (170) -- everyone is in agreement that the type expresses no proposition. My claim, however, is stronger. I hold that any particular utterance of (170) has exactly the same (non-propositional) semantic content as the type (170); that there is in fact no semantic context relativity here and that the analysis of (170) as employing a free variable and thus receiving satisfaction rather than

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<sup>125</sup>There are, of course, issues about the gender information carried by the deictic pronoun 'he'. For the purposes of the main text, I bracket these issues. I think it is quite difficult to say exactly what the status of the gender information of pronouns is. Consider a case in which one utters:

(FN 52) He is a philosopher.

while demonstrating Gila Sher. Do we want to say that (FN 52) (a) is true, (b) is false, or (c) fails to express a proposition? None of the alternatives seem entirely satisfactory. It is possible that the 'multiple proposition' view I employ in the analysis of complex demonstratives (in §2.3.3.2 below) could help provide a more satisfactory analysis, although we lack here the convenient syntactic triggers of propositional multiplicity we find in complex demonstratives.

truth conditions is fully adequate. A full defense of this strong semantic position, however, will have to await the details of the next section.

### **§2.3.2 Proper Names and Singular Thoughts**

In this section I want to take the syntactic observations just made regarding deictic pronouns and extend them to the full class of singular terms. My primary focus will be on the case of proper names; I will argue that plausible semantic and pragmatic stories can be told which not only enable us to make sense of the idea that singular terms are free variables but also give us deeper insight into why singular terms and singular thoughts/propositions are related in the way they are.

#### **§2.3.2.1 Two Concepts of Reference**

Philosophers of language today find themselves in the uncomfortable position of being confronted by two robust accounts of the nature of reference. On the one hand, there is the Fregean picture of reference, which holds to the principle:

(F-Reference) All terms refer only in virtue of the reference-determining properties of their senses.<sup>126</sup>

In opposition to Frege, and in violation of F-Reference, are the Kripke-like proper names, which refer to an object without doing so under any description or class of descriptions. Kripke is thus committed to the following principle:

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<sup>126</sup>See (e.g.):

The regular connection between a sign, its sense, and what it means is of such a kind that to the sign there corresponds a definite sense and to that in turn a definite thing meant. [Frege 1892, 201]

The thesis F-Reference is not meant to entail, of course, that to every sense there corresponds a referent.

(K-Reference) All referring expressions refer directly, without the mediation of a sense.<sup>127</sup>

F-Reference and K-Reference have come to represent a deep divide in the philosophy of language, one which tends to spread to other areas as issues such as internalism and externalism become entangled in the mechanisms of world-word connection. I want to suggest, however, that it is possible to unite these divergent strains of thought -- and that, indeed, Kripke himself would have created the unified picture were he not laboring under a counterproductive presupposition in *Naming and Necessity*.

Note first that F-Reference and K-Reference are not strictly incompatible positions. One can simultaneously hold both. However, an interesting conclusion follows from the conjunction of the two principles: that there are no referring expressions. I want to endorse this strange conclusion and suggest that it provides the key to uniting Kripkean and Fregean semantics.<sup>128</sup>

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<sup>127</sup>Kripke is typically cagey in his expressions of his position in *Naming and Necessity*, but his denial of principles (5) and (6):

(5) The statement, 'If X exists, then X has most of the phi's' is known a priori by the speaker.

(6) The statement, 'If X exists, then X has most of the phi's' expresses a necessary truth (in the idiolect of the speaker).

can plausibly be read as an endorsement of K-Reference.

<sup>128</sup>My endorsement of this conclusion must be somewhat qualified due to the possibility, discussed in §2.2, of undistributed binding. Donkey pronouns anaphorically bound but not subsequently distributed are, on my account, genuine referring expressions. Whether they are Kripkean or Fregean referring expressions is a difficult question. Their semantic content is simply their referent, so they are to that extent Kripkean, or directly referential. However, one cannot come to know *what* content they have without first grasping the concept expressed by the anaphoric binder of the pronoun. We might say, abusing Kaplan's terminology somewhat, that donkey pronouns on my account have a Kripkean content and a Fregean character.

### §2.3.2.2 Kripke and Truth

Kripke's presupposition permeates *Naming and Necessity*, but it is nicely evinced in the Preface as he discusses the sentence:

(172) Aristotle was fond of dogs.

Kripke says of this sentence:

Presumably everyone agrees that there is a certain man -- the philosopher we call 'Aristotle' -- such that, as a matter of fact, (172) is true if and only if *he* was fond of dogs. The thesis of rigid designation is simply -- subtle points aside -- that the same paradigm applies to the truth conditions of (172) as it describes *counterfactual* situations. [Kripke 1980, 6]

[Dummett 1981] attacks the second half of this claim, seeing in it the roots of a violation of F-Reference. However, problems have already arisen before *counterfactual* truth conditions enter the picture. Kripke is mistaken in his analysis of (172) -- mistaken because he holds the mistaken view that (172) is the kind of sentence that receives a truth condition. In fact, it is not. It is an open sentence, and thus is neither true nor false (it 'fails to express a proposition').

An open sentence is a sentence with a free variable. Where, then, is the free variable in (172)? My claim is that the name 'Aristotle', like all proper names, functions as a free variable, and that it is in virtue of the presence of this name in (172) that it fails to form a closed sentence.

Generalizing from (172), then, we reach the surprising conclusions that (a) no proper name refers to anything and (b) no sentence that contains a proper name manages to be true or false or (stronger) to express a proposition at all. My further claim is that, despite the admittedly counter-intuitive feel of this stance, it enables the construction of a semantics that will (i) satisfy the demands of both

the Kripkean and the Fregean, (ii) explain *why* singular terms have the features each of these camps claims they have, (iii) lead to a unified account of natural language noun phrases not provided by either side, and (iv) account for some unusual uses of names that remain problem cases under a Kripkean or Fregean analysis.

More generally, our eventual target is the view that all singular terms are free variables. Generalizing on the syntactic observations from §2.3.1 regarding deictic pronouns, and relying on the semantic intuition that deictic pronouns and other singular terms form a cohesive semantic category, we will suggest that each of the following sentences:

(172) Aristotle was fond of dogs.

(173) That man was fond of dogs.<sup>129</sup>

(174) He was fond of dogs.

(175) I was fond of dogs.

share the same logical form:

(176-LF) [<sub>S</sub> [<sub>NP</sub> t<sub>1</sub>] [<sub>VP</sub> was fond of dogs]]<sup>130,131</sup>

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<sup>129</sup>The use of the complex demonstrative here complicates issues somewhat. The analysis of (173) still involves a free variable, but see §2.3.3.2 below for further discussion.

<sup>130</sup>Given that (172)-(175) all share the same logical form (176-LF), does it follow that all four are synonymous? Yes and no. The semantic rules for the language will produce the same output for each -- except for differences imposed by the different coindexing properties of the free variable (see §2.3.2.3.2 below for more extensive discussion of coindexing). However, that output will in all cases be subpropositional, and it's not clear how useful the notion of synonymy is for subpropositional, supralexic units. It will only be once pragmatic factors come into play that a complete proposition will be expressed, and it is quite plausible that the various singular terms featured in (172)-(175) will interact in a variety of ways with those pragmatic factors. Thus there is no guarantee (semantic or pragmatic) that what two speakers mean by uttering any two of (172)-(175) will be the same.

Pragmatics will thus serve to differentiate (172)-(175), but note that even the minimal semantic distinction imposed by the differing coindexing relations of the variables will provide a certain amount of articulation of the claims. Thus, for example, one might worry that one could infer (172) from (175), given that both share the same underlying form. But presumably the occurrences of 'Aristotle' in (172) and 'I' in (175) will not be coindexed, so we can no more infer (172) from (175)



The case of indexicals introduce complications that will be taken up in §2.3.4, but by the end of this discussion of proper names, some plausibility will be imparted to the stated analysis of all of (172)-(175).

#### **§2.3.2.4 A Pragmatic Story About Reference**

The syntactic considerations of §2.3.1 have, I hope, given some credence to the possibility that names and other putative referring expressions are free variables, abstracting away from the intuition that sentences like:

(172) Aristotle was fond of dogs.

do in fact express complete propositions and say something either true or false. Unfortunately, this intuition is a difficult one to abstract away from. How can we give up the view that (172) says something either true or false?

The general strategy here will be to reconstruct the naive intuitions regarding (172) on the level of pragmatics, rather than the level of semantics. There are at least four levels of meaning which can be considered with respect to a sentence like (172):

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than we can infer 'Fx' from 'Fy'. See footnote 139 below for further discussion of the inferential properties of sentences containing singular terms.

<sup>131</sup>Since we are relying in part on the syntactic behaviour of deictic and other pronouns for the plausibility of this analysis of singular terms in general, it would be nice to have some story about why proper names and indexicals, unlike pronouns, do not appear in bound in addition to free contexts. (I will observe in §2.3.3 below that sentences such as:

(FN 53) Every boy read some book that boy liked.

show that demonstratives can be used as bound variables). Since proper names, as I mention below, display their coindexing relations in their phonetic form (unlike pronouns and demonstratives), it may be that the inability to mark the putative binding operator in the same way as the proper name prevents us from using those operators to bind names. The case of indexicals is more complicated, but here strong conventions of use for those indexicals are incompatible with taking the indexicals as bound variables.

- (A) What the sentence type means.
- (B) What a token of the type (an utterance, a sentence in a context) means.
- (C) What a speaker says by uttering the sentence.
- (D) What a speaker means when uttering the sentence.

Standardly, (172) receives a truth value and expresses a complete proposition at level (A) (abstracting away from indexical features such as tense). One consequence of this standard view is that we must hold that there are (e.g.) many names 'John' in the language -- one for each person named 'John'.

Sentences with indexicals standardly receive a truth value and express a complete proposition at level (B) or (C). Thus:

(175) I was fond of dogs.

expresses no proposition as a sentence type, but does express a proposition in a context (because, importantly, it is a *semantic fact* about the indexical 'I' that it is sensitive to context in the appropriate way).<sup>132</sup> One could also move the interpretation of proper names to this level, and thus have (e.g.) only one name 'John' in the language, which is then interpreted in context just like the word 'I'.<sup>133</sup> My claim, however, is that proper names (and other singular

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<sup>132</sup>Although I will argue in §2.3.4 below that this putative semantic fact about 'I' cannot in fact be satisfactorily implemented in a full semantic theory.

<sup>133</sup>There would, however, be considerable difficulty in supplying an appropriate semantic rule for this one name 'John' which provided the correct sensitivity to context. What rule corresponding to the rule for 'I' could be given for 'John'? Apparently only something like:

(AX FN 3) 'John' refers (in a context X) to whomever the speaker in C intends to refer to.

Setting aside problems for this general type of approach discussed in [Kripke 1980], such a rule would be redundant. As [Kripke 1977] observes, Gricean considerations show us that the *speaker's reference* of a (usage of) a name will be (as determined by pragmatic factors) whatever the speaker intends to refer to. The semantics provided by (AX

terms) are best interpreted only on level (D). Sentences like (172), that is, do not themselves express a complete proposition (even with respect to a context), but may be *used by* speakers to express propositions.<sup>134,135</sup>

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FN 3) thus seem to do nothing but (needlessly) reproduce a result already provided by the pragmatics.

<sup>134</sup>As will become apparent in the discussion below, my presentation of the pragmatic devices that give rise to propositions expressed in uses of names does not sit entirely comfortably with the Gricean picture of the relation between semantics and pragmatics. The relevant propositions are clearly not at the level of what is said, because they cannot be obtained from the sentence type and the semantic conventions of the language (as described in [Grice 1987] or [Grice 1987a]). Some information about these propositions, however, can be obtained from the (semantically determined) effect of the coindexing intentions. However, these propositions also do not properly lie at the level of what is meant, as Grice uses this term. The link between the form of words and the proposition expresses is stronger than mere implicature -- note here (i) that cancellability is not a feature of the propositions expressed using names, and (ii) that a sentence using a name can, after being pragmatically (in my sense) expanded to a propositional content, still give rise to implicatures. Note further that my position, sketched below, that a speaker may be unaware of what proposition he is expressing is difficult (although perhaps not impossible) to square with Grice's intention-based theory of meaning. A substantial reworking of the Gricean framework, especially one rethinking the central role of implicatures calculably generated by the maxims of cooperation in mediating the relation between what is said and what is meant, would be necessary to accommodate my position seamlessly; for the purposes of the current work I choose simply to abuse somewhat the Gricean terminology.

<sup>135</sup>Much of the rest of this section will be devoted to giving reasons for thinking that a pragmatic treatment of reference is to be preferred to a semantic treatment. However, the bulk of the reasons given will be theory-internal reasons. Nowhere will we see a simple and intuitive gap between truth conditions and speaker's meaning of the sort exhibited in the classic Gricean example:

(FN 54) Dear Sir, Mr. X's command of English is excellent, and his attendance at tutorials has been regular. Yours, etc.  
[Grice 1967, 33]

in which the semantics of the sentence clearly issues in a different result from what the speaker means by the sentence. Since it is such clear divergences between what is said and what is meant that motivate the pragmatics/semantics distinction (in addition to general theoretical considerations), we might see the lack of such direct evidence as reason for skepticism about my exportation of reference to the pragmatics. However, there are other areas in which the gap between what is said and what is meant is quite narrow. Consider the following examples:

(FN 55) John and Susan got married and had twins.

(FN 56) Mary is a tall woman.

(FN 57) The man in the corner is drinking wine.

Here pragmatic considerations can amplify the semantic content of the sentences by providing an implicature of temporal ordering, a comparison class of women, and a speaker's reference (possibly, but not

Treating singular terms as free variables and thus moving the propositional content to level (D) will enable us to realize the four benefits claimed above for my approach. The rest of §2.3.2, then, falls into two parts. First, we sketch the functioning of my account of names, and thereby defend its plausibility. Second, we show that the developed theory does have the benefits claimed for it.

#### **§2.3.2.4.1 Semantic Incompleteness**

To set out the mechanisms that operate in our understanding of proper names and other free variables, we will first need to investigate two areas: semantic incompleteness and variable coindexing. Consider first what generally happens when a linguistic agent encounters an incomplete utterance. Note that there are two ways in which an utterance can be incomplete. Some utterances are syntactically incomplete. Syntactic incompleteness can result from omission, repetition, or scrambling of words, as in:

(177) Sean a very tall man.

(178) John owns a owns a large car.

(179) Aristotle was dogs of fond.

Cases of ellipsis, such as:

(180) Albert loves his wife, and so does Frederick.

might profitably be assimilated to the paradigm of syntactic incompleteness. When we encounter a syntactically incomplete utterance, our task as interpreters is to reconstruct a grammatical utterance,

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necessarily, satisfying the definite description) respectively. In all of these cases, what is meant is quite close to what is said. Clearly speaker's intuitions do not clearly distinguish between the semantic and the pragmatic contributions to the understanding of such utterances. Here, then -- as in my approach -- we must resort to desiderata other than simple judgements of meaning to draw the line between semantics and pragmatics.

through the insertion, deletion, and rearrangement of lexical material. Thus syntactic incompleteness can give rise to genuine ambiguities, when (e.g.) the scope of an inserted quantified noun phrase or the anaphoric indexing of an inserted pronoun is unclear.

More controversially, utterances can also be grammatically well formed but semantically incomplete.<sup>136</sup> Such incompleteness will generally result when our grammar has mistakenly analyzed the structure of a concept. Thus sentences like:

(181) It is 10 o'clock.

(182) Mary is quite tall.

(183) Robert Bresson is a better director than Carl Dreyer.

all fail to express complete propositions, because the concepts of time, tallness, and aesthetic value all require parameters not provided in the sentence -- a time zone, a comparison class, and an aspect of artistic competence.<sup>137</sup> To understand a semantically incomplete utterance, then, we need to grasp the underlying concepts, discover what arguments those concepts request which are not provided in the sentence, and then provide (add to the semantically provided propositional matrix) the necessary arguments. Note that here we provide the arguments themselves, not linguistic tools for singling out those arguments. Thus we find that semantic incompleteness does not generate the kinds of ambiguities that syntactic incompleteness does.

Open sentences are a species of semantically incomplete sentence.

One of the arguments requested by the predicate -- the argument

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<sup>136</sup>The notion of semantic incompleteness is taken up in more detail in §2.3.4.1 below.

<sup>137</sup>See [Perry 1984]'s discussion of unarticulated constituents for similar observations, although Perry holds that the (apparently) missing propositional constituents are part of the meaning of the sentence.

corresponding to the grammatical position containing the free variable -  
- is not provided by the lexical material in the sentence. To understand  
such a sentence, we must provide the missing argument to the  
semantically presented propositional matrix.

#### **§2.3.2.4.2 Coindexing**

Within the theory of bound variables, we have the notion of coindexing.  
This notion, commonly realized in formal languages using numerical  
subscripts, serves two purposes: it indicates when two variables are to  
be governed by the same variable-binding operator, and also indicates  
when two variables are to be interpreted as receiving the same object  
when considering satisfaction conditions. Free variables, of course, can  
also have coindexing relations among themselves, and while this  
coindexing obviously does not indicate sameness of binding operator, it  
does indicate sameness of object under satisfaction conditions.

Coindexing also serves this second function in natural language.  
When encountering a sentence like:

(184) He likes his car.

one needs to know whether the two pronouns are coindexed, in which case  
the sentence is about a self-car-liker, or conindexing, in which case  
it is about one man's admiration for another's car. Coindexed pronouns,  
that is, are (on the standard story) coreferential or (on my story) to  
receive the same object when considering satisfaction conditions. In  
natural language, these coindexing relations are just those which are  
standardly called, somewhat misleadingly, anaphoric relations. Similar  
coindexing relations can hold among demonstratives.<sup>138</sup>

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<sup>138</sup>Although discrete tokens of demonstratives have a greater tendency to  
be conindexing than pronouns. We might think of demonstratives as

Nothing accessible in surface syntax signals whether two pronouns (or demonstratives) are coindexed, although intonation can provide some clues. Audiences thus must guess at the coindexing intentions of the speaker. Presumably the epistemic difficulties thus introduced help account for the difficulty in coindexing widely separated (e.g., in different conversations) pronoun tokens. Now if proper names are also free variables, then they too ought to have coindexing relations among them. We already have reason to believe they do, since we can have (apparently) anaphoric relations between names and pronouns, as in:

(185) Hitchcock called his actors cattle.<sup>139</sup>

'His' here needs to be coindexed with 'Hitchcock', so there must be some indexing mechanism that operates on proper names. As with pronouns and demonstratives, coindexing relations among names indicate a syntactic constraint demanding (on the traditional story) sameness of reference for coindexed names. However, unlike pronouns and demonstratives, observable linguistic structure gives us clues about the coindexing relations among names. We thus have the following principle:

(Proper Name Coindexing Principle) Sameness of phonetic form provides a defeasible presumption in favor of coindexing; difference in phonetic form provides a (weaker) defeasible presumption in favor of contraindexing.

Thus, for example, when I use the name 'Aristotle', there is a presumption that I intend that name to be coindexed both with my own

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providing a class of 'disposable' variables which are always ready to be assigned a fresh referent.

<sup>139</sup>These coindexing relations between the proper-name-like variables and the pronoun-like variables will, when combined with the pragmatic story about understanding of utterances containing free variables, provide the material for accommodating our third problem case -- that of pronouns anaphoric on singular terms -- within the framework of the WNVVT.

prior uses of the name and with other people's uses of the name 'Aristotle'. This presumption means at least that my audience will tend to take my token as so coindexed and probably also that some special effort is necessary on my part not to have it so coindexed. Note, however, that the phonetic form is in no way constitutive of coindexing: what my use of a name is coindexed with is entirely dependent on my coindexing intentions (which may be manifest at some level of syntax). These intentions can be formulated in any number of ways. I might have intentions such as:

(CI-1) Let this token of 'Aristotle' be coindexed with all other variable tokens of the same phonetic form.

(CI-2) Let this token of 'Aristotle' be coindexed with my previous token of 'Aristotle' and anything it was coindexed with.

(CI-3) Let this token of 'Aristotle' be coindexed with my previous token of 'Aristotle' and anything it was coindexed with except Albert's tokens of 'Aristotle'.

(CI-4) Let this token of 'Aristotle' be coindexed with any token of 'Aristotle' which was intended by the speaker to refer to Aristotle.

Note here that (a) I don't need to respect old coindexing relations when I decide how my current use is to be coindexed -- a result unobtainable using, say, numerical indices to indicate coindexing -- and (b) there is nothing problematic with using the communicative intentions of other speakers in setting up the coindexing standards.

The presumption of coindexing created by phonetic form is defeasible because there may well be cases in which I don't want my use of 'Aristotle' to be coindexed with all other uses of 'Aristotle'. If,



for example, I am aware that there is a man standardly called 'Aristotle Onassis', I won't want my use of 'Aristotle' coindexed with uses of 'Aristotle' by people talking about Aristotle Onassis.<sup>140</sup> I might even suddenly decide to start referring to my cat as 'Aristotle', in which case I don't want my use coindexed with any prior uses of the name. As a fairly reliable rule, however, it is safe to assume that my use of a name is coindexed at least with all phonetically identical names used for the same communicative purpose and ideally with all phonetically identical names simpliciter (depending on the uniqueness of the name). Because names carry in their phonetic form a reliable guide to their coindexing relations, it is possible to have more extended coindexing relations among them -- my use of 'Aristotle' can be coindexed with John Milton's use of 'Aristotle' in a way in which (in general) my use of 'he' cannot plausibly be coindexed to his use of 'he'.<sup>141</sup>

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<sup>140</sup>Thus one consequence of treating proper names as free variables carrying coindexing relations among themselves is that we can hold that a language contains only one name of a given phonetic form (in opposition to the standard view, which (roughly) individuates names phonetically and semantically, and which thus claims that there are (at least) two names 'Aristotle', one referring to the philosopher and one referring to the former husband of Jacqueline Kennedy). Discrete uses of that one name will then be tracked using appropriate coindexing. We may thus be in a better position than the standard view to deal with [Kripke 1979]'s 'Paderewski' case, in which an agent has diverging attitudes toward sentences containing phonetically identical names referring to the same individual, since there is no bar to having two occurrences of the same (phonetic) name used to talk about the same individual be contraindexed. How to use this deviation from the standard position to account for troubling cases of attitude reports goes beyond the scope of this work, although see [Fiengo & May 1994] for a (not entirely successful) attempt to implement a similar idea.

<sup>141</sup>The coindexing relations among variables allow us to recapture through syntactic means inferences which can no longer be explained as truth-preserving. Clearly, for example, we want to be able to infer from:

(172) Aristotle was fond of dogs.

and:

(FN 58) Aristotle was a philosopher.

to the conclusion:

(FN 59) Aristotle was both a philosopher and fond of dogs.

### §2.3.2.4.3 The Story Applied

When a speaker uses an (open) sentence with a name in it, he does thereby express *through the semantic rules for the language* a proposition. However, there will in general be a proposition that he means to express. Note that there are two senses (corresponding roughly to wide and narrow scope readings of 'a proposition') in which a speaker can mean to express a proposition. Either the speaker can have some proposition in mind and intend to express that proposition, or the speaker can merely intend that some proposition or another be expressed by him, even if he doesn't know which one. The pragmatic mechanisms which come into play when a sentence with a name is uttered will make use of both of these senses -- one corresponding to each of the mechanisms sketched above.

The presence of a free variable in a sentence signals a semantic incompleteness in the sentence, and the grammatical position of the

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However, we can no longer simply observe that the truth of (172) and (FN 58) guarantees the truth of (FN 59), simply because none of these three is capable of being true. What we can observe, however, is that a perfectly ordinary use of the rule of 'and'-introduction allows us to infer from the premisses:

(FN 60) Fx

(FN 61) Gx

to the conclusion:

(FN 62) Fx & Gx

because the variables in (FN 60) and (FN 61) are coindexed.

Semantically, we can back such inferences through a proof that they preserve satisfaction conditions, rather than truth.

Of course, reconstructing inferences involving proper names in this manner will not allow us to infer, say, from (172) to:

(FN 63) Someone is fond of dogs.

any more than we could infer from 'Fx' to (Ex)Fx'. So we cannot completely recapture the ordinary intuitions about the inferential power of proper names. On the other hand, we are now equally incapable of deriving:

(FN 64) Something is a winged horse.

from:

(FN 65) Pegasus is a winged horse.

-- a bad inference which the traditional semantics for proper names notoriously does validate.

variable further specifies what type of semantic value is necessary to complete the proposition. As a rule, then, when a speaker chooses to utter a sentence containing a name, he will single out in his thought, via some appropriate mechanism, a particular object to which he wishes to make reference. The speaker means to express the proposition which can be formed from the propositional matrix given by the semantics of the sentence plus that object, inserted into the syntactically indicated position.

Through his coindexing intentions with regard to the variable, the speaker also means to express the proposition formed by the insertion (again, in the syntactically indicated position) of the object which other speakers were talking about when using coindexed variable tokens. The speaker thus displays an intention to be talking about the same object that others who used coindexed variable tokens were intending to talk about with their utterances.<sup>142</sup> The speaker may not be in a position himself to grasp the proposition that he intends to express, since he may be unaware of the referential intentions of previous (coindexed) speakers or may lack the cognitive resources to entertain the resultant proposition.

In using a name, then, a speaker expresses a proposition derived from his intended use of the name and a proposition derived from the

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<sup>142</sup>Note that this commitment cannot be stated as a commitment that there be some object which is such that, were the variable in question assigned that object as value, all utterances containing coindexed variable tokens would be true. The speaker may believe that other people have meant something false by their utterances using the coindexed variable; this need not stop him from coindexing with them (indeed, he will want to coindex with them if his desire is to correct them). The speaker will only refrain from coindexing in cases in which he believes that the previous speaker was talking about a different object from the one he wishes to talk about.

historical facts of uses he has singled out through coindexing intentions. In paradigm communicative cases, these two propositions will be one and the same, but in particularly problematic cases the propositions in play may multiply rapidly (as, for example, will happen when there is diversity of opinion about the 'true referent' of a name).

#### **§2.3.2.4.3.1 Paradigm Communicative Cases**

Let's start by considering a paradigm communicative case in which the two levels of propositional expression coincide. Take, for example, an utterance of mine of:

(186) Alfred Hitchcock directed *Sabotage*.

Suppose I have, through acquaintance (in Russell's sense), a concept of Alfred Hitchcock -- I am capable of having thoughts which are thoughts of Hitchcock. Furthermore, I have some thought about Hitchcock that I want to convey -- namely, the thought that he directed *Sabotage*. I thus intend my utterance of (186) to be about Hitchcock; it is to him that I attribute the property of having directed *Sabotage*. When I set out to express this thought I must pick out some linguistic realization of it. I insert a free variable to leave room for the desired object (Hitchcock), but I have a wide range of free variables available to choose from. I select the one which is phonetically realized as 'Alfred Hitchcock'. Why do I do this? Presumably because I want to show an intention to be intending to express a proposition that is about the same person that the propositions which other people (and I myself on other occasions) have intended to express when using the variable 'Alfred Hitchcock' are about.<sup>143</sup> The phonetic form of the variable, as

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<sup>143</sup>To avoid such ugly locutions in the future, I will say that I am talking about Alfred Hitchcock when using the name 'Alfred Hitchcock' if

noted above, provides a (publicly accessible) clue as to my (private) coindexing intentions. Were there another Alfred Hitchcock around, I would not want my use of 'Alfred Hitchcock' coindexed with uses by people talking about the other Hitchcock. And, of course, it is possible that I might not intend this occurrence of 'Alfred Hitchcock' to be coindexed with anything else -- I might have spontaneously invented a new name for Hitchcock that sounds just like his old name.<sup>144</sup>

We've now seen how I manage to express (pragmatically) a proposition when using a sentence of the form:

(186) Alfred Hitchcock directed *Sabotage*.

What happens (again in the ideal case) when my audience hears this sentence? They process the sentence semantically and find that it contains a free variable and is thus semantically incomplete; they thus realize that they are called on (because of the syntactic position of the variable) to provide an object for this open slot. If they are to understand me, they must provide the same object to complete the proposition that I intended to talk about. They may be able to find the right object just through the conversational context. If we have been discussing British directors who moved to Hollywood and their films, and Hitchcock is the only such director we have not yet mentioned, it's a good bet that I'm now talking about him. However, I've also provided a valuable additional clue to my communicative intentions by using the

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Hitchcock features (in the relevant position) in the proposition I am trying to express. Note that talking about Hitchcock is not the same as referring to Hitchcock. Note also, as we will show below, that I may be talking about more than one individual, and that I may not know about whom I am talking. The above claim can then be more succinctly put as: I am intending to show that I am talking about the same person as others talk about using the name 'Alfred Hitchcock'.

<sup>144</sup>Of course, it may be psychologically impossible for me to get myself into this state, knowing (loosely speaking) what his name is.

name (variable) 'Alfred Hitchcock'. The audience can thus reliably guess (because of the defeasible presumption of coindexing created through phonetic form) that I intend my use of this name to be coindexed both with my own previous uses of the name and with their previous uses of the name. By considering their linguistic past, they can determine which object I and they were talking about with those previous uses, and from this information reconstruct the proposition that I meant to express. Note, importantly, that my audience need not single out Hitchcock as the object of thought in the same way that I do. They might not be personally acquainted with Hitchcock, and instead think of him as the director of *Shadow of a Doubt* or as that man with the small instrument in *Vertigo*. However they are able to have thoughts about Hitchcock, they employ these mechanisms and come to entertain the proposition consisting of Hitchcock, *Sabotage*, and the property of directing.<sup>145</sup>

#### **§2.3.2.3.3.2 Communication Under Information Deficits**

The above is the ideal case, in which speaker and audience easily converge on a single proposition meant.<sup>146</sup> However, things will not always go so smoothly. Assume now that I utter (186) to someone who has never met and has no concept of Alfred Hitchcock. This person will then

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<sup>145</sup>A similar story must be told, of course, regarding the understanding of the free variable '*Sabotage*' in (186).

<sup>146</sup>Actually, it may not be quite true that there is only one proposition at play here. If previous users of the name 'Alfred Hitchcock' have (mistakenly, as we would like to say) used it to talk about, say, Michael Powell, then my coindexing intentions with those users will commit me to the expression of a proposition about Powell. What to say in these cases depends on exactly how one's coindexing intentions are spelled out. In general, it seems likely that we will ignore such extraneous propositions (if we are even aware of our commitment to express them), although if the subcommunity making deviant (in our view) use of the name is large or significant enough, we may specifically exempt its members' utterances from our coindexing intentions (compare 'Meanings are functions, although not in Frege's sense of 'function)').

lack the cognitive capacity to entertain the proposition I am trying to express, and in some important sense will not know what it is I am trying to say. However, they can still know *something* about what I am trying to say. They will know that there is some person to whom I am ascribing the property of having directed *Sabotage*. Moreover, they will have good reason (because, again, of the phonetic form of the variable I have chosen) to think that I am talking about the same person that I was talking about earlier when I used the name 'Alfred Hitchcock'. They will then be in a position to start building up a portfolio of information on this unknown person -- a portfolio which may, depending on one's views on these matters, eventually be sufficient for them to have thoughts about Hitchcock. A listener, then, can engage with some success in a communicative process without knowing exactly what is being said to them.

Moreover, this hypothetical listener can then become a user of the name 'Alfred Hitchcock'. After hearing me utter (186), he will then be in a position to utter:

(187) Alfred Hitchcock must dislike children.

with their use of 'Alfred Hitchcock' coindexed with mine.<sup>147</sup> On the assumption that the speaker of (187) still lacks a concept of Hitchcock, there will be no person that he has in mind as the subject of his utterance. There will thus be no proposition that he intends to express as the completion of the semantically incomplete (187). However, his

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<sup>147</sup>Of course, they were free prior to hearing me utter (186) to use a sentence of type (187), but here they could not plausibly take their use of 'Alfred Hitchcock' to be coindexed with other people's uses, not being aware that there were such other uses. Unless they had deviant coindexing intentions, then, they might well express no proposition through such an utterance.

coindexing intentions show a desire to talk about the same person I was talking about, so he expresses through these intentions a proposition about Hitchcock. He is simply unable to know what proposition he is expressing. I, however, having a concept of Hitchcock, do know what proposition that is, and thus can understand him (better, ironically, than he can understand himself). Putting together the case of the uninformed listener and speaker, we see that it is possible to engage in extended conversation without knowing at any point what it is that one is saying. All participants in the conversation may, in some cases, be in this position.

#### **§2.3.2.3.3.3 Communication Under Misinformation**

Let's take a difficult case now, one in which a speaker is misinformed and thus the two aspects of his speaker's meaning diverge. Take Kripke's hypothetical case in which the Incompleteness Result was derived by Schmidt, not by Gödel. Let Albert be a speaker who has a concept of the discoverer of the incompleteness result (we can even assume that Albert met Schmidt under this description in order to ensure that he has the cognitive capacity to have thoughts about Schmidt). Albert now asserts:

(188) Gödel was inspired by the Liar paradox in his proof.

He intends this to be an utterance about the author of the proof, so he brings to bear here his concept of Schmidt to express a (true) proposition about Schmidt. However, he also coindexed his utterance with other utterances using the name 'Gödel', and thus commits himself to talking about whoever these utterances were talking about. Now at least some of these utterances will be made by people acquainted with Gödel



and thus will be about Gödel.<sup>148</sup> Albert thus also expresses (unbeknownst to him) the (false) proposition that Gödel was inspired by the Liar. When I hear Albert utter (188), what happens depends on what I know. If I am aware that Schmidt is the real author of the proof, I will probably realize Albert's confusion and thus spot that he is expressing two separate propositions. If I am not in on the secret, I will, like Albert, assume that he is expressing only a single proposition (the one about Schmidt). Note that if Albert is informed of the facts, he is perfectly free to insist that what he meant was true (at least in part), but he must be willing to give up the particular form of language he used to express his thought.<sup>149</sup> This linguistic retraction gives rise to the impression of concession of error which can bolster the intuition that the name 'Gödel' *really refers* to Gödel.

#### **§2.3.2.3.3.4 Generalizing the Story**

My thesis is that all singular terms are free variables, not just proper names. Thus the pragmatic story sketched above will also apply to demonstratives and deictic pronouns. However, the relatively weaker coindexing inclinations of these terms will cause the intention to express a proposition about the same thing as those propositions meant by previous utterances containing coindexed variables to take a back seat to the proposition about whatever object the speaker has in mind. In the case of deictic pronouns, coindexing relations will serve merely

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<sup>148</sup>Others may be by other people confusedly believing (as it were) that Gödel derived the incompleteness result. Albert's coindexing intentions may thus give rise to the expression of multiple propositions, some of which may coincide with the proposition derived from his immediate referential intentions.

<sup>149</sup>Taking his coindexing intentions to be part of the form of language he has used.

to help obtain continuity of reference during a single conversation. In the limiting case of demonstratives, coindexing relations are quite difficult to impose. Thus the referential intentions of the speaker will dominate and an intention-based theory of demonstrative reference (hiked up to the pragmatic level) similar to that which Kaplan now favors will result.<sup>150</sup> These issues are taken up in more detail in §2.3.3 below.

#### **§2.3.2.4 Engineering a Frege-Kripke Reunion**

The above discussion shows that there are sensible pragmatic mechanisms through which successful communication using open sentences would be possible. Still, why would either the Kripkean or the Fregean give up their own theories to adopt this new position? Mere success in accounting for ordinary cases of name-using communication will provide no such reason. I next want to show both sides why they should meet in compromise on the ground I've provided. The argument involves two phases. First, I point to a class of uses of names which neither the Kripkean nor the Fregean is well-equipped to handle, and show that a free-variable account can make sense of these uses. Second, I show that my account can draw together and derive many of the major features of both the Kripkean and Fregean account of names. These derivations are intended to show each side both that they can have the desirable features of the other's account and that they can have a deeper

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<sup>150</sup>See [Kaplan 1989]:

In *Demonstratives* ... I claimed that the demonstration rather than the directing intention determined the referent. I am now inclined to regard the directing intention, at least in the case of perceptual demonstratives, as criterial, and to regard the demonstration as a mere externalization of this inner intention. The externalization is an aid to communication, like speaking more slowly and loudly, but is of no semantic significance. [Kaplan 1989, 582]

explanation of their own account's features than they previously possessed.

#### **§2.3.2.4.1 Some Deviant Cases of Reference**

On both the Kripkean and Fregean accounts, reference is the central feature of names -- on the Kripkean account as the sole semantic value of the name and on the Fregean account as the criterion of an adequate sense. This focus on reference leads to the problem of 'empty names' which has come to generate a robust cottage industry. My account, of course, has no problem of empty names, but rather one of 'full names', a problem to which the previous section was devoted to solving. On my account the use of names in fictional contexts or connected to defective baptisms is not inherently problematic; these names are no 'emptier' than any others, but may serve different types of pragmatic goals. I want to sketch three cases which have some of the features of empty names but which raise even more complicated issues for those who want reference as the basis of naming.

##### **§2.3.2.4.1.1 Naming Indeterminately**

First case: suppose Sherlock Holmes arrives on the scene of the murder, inspects the evidence, and then turns to Dr. Watson to say:

The murder was committed by two men. Call them Mr.X and Mr.Y. Mr.X and Mr.Y sneaked in through the unlocked back door. Had the victim remembered to lock the back door, then Mr.X and Mr.Y would not have killed him, but two other men, Mr.W and Mr.Z, lurking in the bedroom, would have killed him instead.

On the face of it, it looks as if Holmes has, in Kripkean fashion, introduced some names by description. Mr.X and Mr.Y are the two murderers, in the same way that Julius is the inventor of the zipper. However, there's a crucial difference here from the usual name introduction case. While Holmes has determined that 'X' and 'Y' are names for the two murderers, he has said nothing that would determine which murderer is picked out by which name. Say that the murders were actually committed by Louise and Auguste Lumiere. Then 'X' and 'Y' refer to Louise and Auguste, but nothing Holmes has said determines whether 'X', in particular, refers to Louise or to Auguste. I assume then, that there is no fact of the matter about which of the two it refers to (that is, it refers to neither).

This sort of naming is uncommon in most situations, but it shows up with some frequency in mathematics. Consider bit of mathematical reasoning like the following:

This function  $f$  has two roots -- call them  $r_1$  and  $r_2$ . Since I know that the function is quadratic in form, I know that it assumes its maximum/minimum at  $(r_1+r_2)/2$ .

Nothing in such reasoning requires that  $r_1$  refer to one of the roots in particular -- just that  $r_1$  and  $r_2$  refer to the two roots. Of course these kinds of cases, both mathematical and investigative, can be analyzed by taking the putative names as variables bound by quantifiers having scope over the entire discussion.<sup>151</sup> But they certainly look like names, and it would be nice that we could treat them as such (and not give up the intuition that variable-binding operators have their domain

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<sup>151</sup>See [Yagisawa 1984] for a defense of the claim that *all* names are bound variables of this sort.

of enforcement limited by syntactic features of scope). Note also that Holmes might just as well have arrived on the scene and started his analysis 'Mr.X and Mr.Y entered through the back door and killed the victim...', thus depriving us of any lexical material that could plausibly function as the quantifier binding the putative variables.

#### §2.3.2.4.1.2 Naming Impossibly

The second case is in a way just an extension of the 'indeterminacy of reference' phenomenon just noted, but it is a dramatic enough extension to be worth noting.<sup>152</sup> In many natural deduction systems, names are used at some point in an interesting and idiosyncratic way. Consider the following deduction of ' $(\exists x)Fx$ ' from ' $(\exists x)(Fx \wedge Gx)$ ':

1	(1) $(\exists x)(Fx \wedge Gx)$	Premiss
2	(2) $Fa \wedge Ga$	Premiss (for $\exists E$ )
2	(3) $Fa$	$\wedge E$ , 2
2	(4) $(\exists x)Fx$	$\exists I$ , 3
1	(5) $(\exists x)Fx$	$\exists E$ , 1,2,4

Put into English, this deduction goes something like the following:

something is both F and G. Call that something 'a'. Then a is both F and G, so a is F. But if a is F, then something is F.

Why is there a prima facie problem about the name here? One difficulty is that the referent of the name that we are introducing seems to be inherently underdetermined.<sup>153</sup> Since the truth of ' $(\exists x)(Fx \wedge$

<sup>152</sup>The subsequent discussion of names in natural deduction systems has its roots in Kit Fine's discussion of the use of 'arbitrary objects' in mathematical reasoning (see [Fine 1985]). I think it can be shown, in a more detailed discussion, that Fine's conclusions are driven by a confused theory of the semantic function of variables.

<sup>153</sup>We might, of course, hold that new names are not being introduced in such examples of natural deduction, but that previously interpreted names are being used and certain counterfactual suppositions about the referents of those names are being made. Note, however, that (a) in

$Gx$ )' is perfectly compatible with there being many objects which are both  $F$  and  $G$ , we have no way of knowing which of those many objects our introduced name 'a' picks out, since there is no content to the dubbing beyond 'an object which is both  $F$  and  $G$ '. And it's not just that we can't find out -- it seems that there is no fact of the matter about object 'a' names. But if the semantic content of a name is exhausted by, or determined via, its referent, and if there is no fact of the matter about what 'a' refers to, how can 'a' be a meaningful expression, and how can it combine with other terms to make meaningful (and even true) expressions such as ' $Fa \wedge Ga$ '?

Furthermore, in some cases of natural deduction, we know ahead of time that there is no object for the new name to name. Consider the following deduction of ' $(\forall x)\neg Fx$ ' from ' $(\forall x)(Fx \rightarrow (\exists y)(Gy \wedge \neg Gy))$ ':

1	(1) $(\forall x)(Fx \rightarrow (\exists y)(Gy \wedge \neg Gy))$	Premiss
1	(2) $Fa \rightarrow (\exists y)(Gy \wedge \neg Gy)$	$\forall E, 1$
3	(3) $(\exists y)(Gy \wedge \neg Gy)$	Premiss
4	(4) $Gb \wedge \neg Gb$	Premiss (for $\exists E$ )
4	(5) $\neg(\exists y)(Gy \wedge \neg Gy)$	$\neg I, 3, 4$
3	(6) $\neg(\exists y)(Gy \wedge \neg Gy)$	$\exists E, 3, 4, 5$
-	(7) $\neg(\exists y)(Gy \wedge \neg Gy)$	$\neg I, 3, 6$
1	(8) $\neg Fa$	$\rightarrow E, 2, 7$
1	(9) $(\forall x)\neg Fx$	$\forall I, 8$

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actual natural language reasoning along the patterns of these natural deduction proofs we do not typically use previously interpreted names but rather introduce new names, and (b) even if we could understand these proofs as using interpreted names under counterfactual suppositions, the proofs also remain perfectly comprehensible when previously uninterpreted new names are used.

The use of the introduced name 'a' on line (2) reproduces the problems of the previous paragraph, but the introduction of 'b' on line (4) is even more troubling. Here 'b' is introduced as a name for an object that is both G and not G. But we know *a priori* that there is no such object, and hence that 'b' is an empty name and that constructions using 'b' are meaningless. Why, then, do we countenance the use of the impossible name 'b' in our proof?

#### **§2.3.2.4.1.3 Naming Regressively**

For the third problem case, I want to show that there are comprehensible cases in which the chain of uses of a name does not trace back at any point to an object, and thus for which there is no possibility of introducing a sense or reference, no matter how indeterminate. My example here has an unfortunate science-fiction feel to it, but I think the point it raises is nonetheless valid. Suppose you walk into a crowded pub, and hear a number of people discussing heatedly the question of whether 'Alex is cheating on his wife'. Intrigued, you join in the conversation. You learn some of the affairs of this purported adulterer, and begin to form some of your own opinions about whether he is cheating on his wife. A few of the participants, citing the late hour, leave the table. Some newcomers take their place, and you and some of the others fill them in on the current consensus on Alex's sex life. The gradual turn-over of conversationalists continues. Eventually all who were there when you entered are gone, and others have taken their place. Early in the morning, you yourself leave. The next day, having forgotten the details of the conversation (you overindulged a bit during

the debate) you return to the pub and get drawn into a discussion about whether 'Alex is cheating on his wife'. And so on.

Now add just one small detail to this story. Assume it is set in a world in which time repeats itself in a 24-hour loop. The problem is then that although all uses of the name 'Alex' are linked to earlier uses, there is no initial dubbing, description, or demonstration to ground any of these uses. There is also no way to attach a sense to 'Alex' which could even potentially pick out an object.<sup>154</sup> Thus there is no way to see these people as doing anything other than trading semantically incomplete propositional matrices for hours on end.

#### **§2.3.2.4.1.4 Accounting for the Deviant Cases**

Once we give up the idea that names must refer, as my account does, we can explain what is going on in these cases. In no case, of course, is there a proposition that the speaker is (knowingly or unknowingly) expressing. Nonetheless, in each case some sensible piece of linguistic behaviour is occurring. In the first case, when Holmes says, e.g., 'Mr. X is a murderer', he expresses neither the proposition that Louise is a murderer nor the proposition that Auguste is a murderer. However, he does commit to expressing one of these two propositions. It just doesn't

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<sup>154</sup>In essence we have here a situation in which all speakers of the name 'Alex' use it in what Dummett calls the 'derivative' sense:

A speaker may be said to use a word derivatively if he employs it with the intention that it be understood in accordance with its accepted use in the language, but does not have a full knowledge of what that use is. [Dummett 1981, 591]

Dummett himself claims that such universal derivative use is impossible:

The argument is not that, if all speakers used 'sheep' and 'Socrates' in that manner, those words would lack Fregean senses: it is that it is unconditionally impossible that they should do so. [Dummett 1981, 591]

I think my example here shows that Dummett's impossibility claim is wrong and that it is derived from a view of names which focuses too single-mindedly on reference.



matter for his communicative purposes which of the two he uses, so he never decides.<sup>155</sup> In the second case, we may not care to mean anything by the sentences within the proof. A minor modification of the usual soundness and completeness proofs will show that, construing these lines as open sentences, we are still guaranteed to get the right results from our proofs. Since this is all we care about, we are free to regard the intervening steps as semantically incomplete stepping-stones to the final goal. As a rule, there's no need to assume that people are expressing propositions to make sense of their linguistic behaviour. That they think they are expressing propositions, or that they are describing a sufficiently well-restricted region of propositional space, or that they are pretending to express complete propositions, or that they are focusing on conceptual relations among propositional matrices can explain their behaviour.

#### **§2.3.2.4.2 The Reemergence of Sense**

One benefit of the view that proper names are free variables is that one may, if one wants, retain the doctrine that all reference must be in virtue of some mediating sense. Semantically, this doctrine does not commit one to any senses as parts of meaning, since we have now also purged all reference from semantics as well. The doctrine thus now has its bite at the pragmatic level: one is able to entertain thoughts only about those entities for which one has sufficient cognitive resources to single out.

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<sup>155</sup>The case is thus similar to that of a picketer who explains the union's position by pointing to signs being carried. If all the signs convey the union's position well, although differently, then the picketer need never decide which sign in particular he is pointing to.

I have taken no stand thus far on what cognitive capacity is necessary in order for an agent to have thoughts about (grasp propositions about) a particular object. We might need some description (not necessarily linguistic) which uniquely identifies that object; we might instead (or also) need something like acquaintance or causal interaction with the object. The virtue of my approach is that it allows us, *qua* semanticists, to remain agnostic on these issues. If the standard for singular thoughts is set low enough, agents will almost always be able to grasp the proposition expressed by the speaker simply in virtue of picking out the relevant object under the description 'the object the speaker is talking about'. Of course, they may also simultaneously entertain other propositions without realizing that they are entertaining multiple propositions, if they are unaware that this description and the cognitive capacities they themselves are bringing to bear unfortunately single out different objects. If the standard for singular thoughts, on the other hand, is set extremely high, few may succeed in grasping propositions expressed to them.

Let's say we do adopt a Fregean understanding of the preconditions for thinking about an object. Then when an agent hears a sentence like (186) and does grasp the proposition intended to be expressed by that utterance -- that Hitchcock directed Sabotage -- he does so only in virtue of the fact that he has a concept of the individual Hitchcock through which he can entertain thoughts about him. Is this enough to keep Frege happy? We certainly don't have the whole Fregean picture -- what is grasp is a singular proposition that has the object itself as a

constituent, rather than the sense that singles out the object.<sup>156</sup> What we do get is that one grasps this proposition only in virtue of an associated Fregean sense. Is this enough for the broader Fregean purposes? A full response to that question would involve consideration of whether we could account for agents' differing epistemic attitudes toward claims differing only in substitution of coreferential terms. Since, on my account, there are no coreferential terms per se, we would have to move to consideration of terms which are used to talk about the same object. This move from the semantics to the pragmatics would, I suspect, provide the maneuvering room necessary to draw the distinctions Frege wants. However, a satisfactory spelling-out of the details here would require first settling notoriously difficult issues in the semantics of the attitude contexts. With [Kripke 1979], I am suspicious of placing too much weight on argument centered around such contexts, so I want here to provide two small promissory notes on the larger question by showing that on my account there is neither irreducibly predicative nor contingent *a priori* knowledge resulting from proper names.

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<sup>156</sup>On the claim that the sense of a proper name affects the proposition, see (e.g.):

We now inquire concerning the sense and meaning of an entire assertoric sentence. Such a sentence contains a thought. Is this thought, now, to be regarded as its sense or its meaning? Let us assume for the time being that the sentence does mean something. If we now replace one word of the sentence by another having the same meaning, but a different sense, this can have no effect upon the meaning of the sentence. Yet we can see that in such a case the thought changes; since, e.g., the thought in the sentence "The morning star is a body illuminated by the Sun" differs from that in the sentence "The evening star is a body illuminated by the Sun." [Frege 1892, 203]

#### §2.3.2.4.2.1 Irreducibly Predicative Knowledge and Contingent *A Priori*

##### Knowledge

Consider what Dummett says about irreducibly predicative knowledge:

Frege's arguments for the sense/reference distinction should be understood as depending on a rejection of the idea that there can be irreducibly predicative knowledge. If to say of X that he knows, of z, that it is F is to give a complete characterization of the piece of knowledge being ascribed to X, then that piece of knowledge may be said to be irreducibly predicative; if, on the other hand, a complete characterization of the piece of knowledge requires a sentence of the form 'X knows that A', ascribing propositional knowledge to X, then the predicative knowledge ascribed to him may be said to rest on that piece of propositional knowledge. To say that there is no irreducibly predicative knowledge is to say that each piece of predicative knowledge rests on some suitable piece of propositional knowledge. [Dummett 1981, 325]

Does a complete characterization of my knowledge that Alfred Hitchcock directed *Sabotage* require a piece of propositional knowledge? I suppose this depends on how much one is packing into the completeness condition. The proposition that I grasp is indeed an irreducibly predicative one; it contains only the individual Hitchcock, not a description of that individual. However, to grasp that proposition, I must have some description of Hitchcock. If, then, a complete characterization of the knowledge includes an explanation of the cognitive preconditions on entertaining that knowledge, then it is not irreducibly predicative.

Consider now the question of contingent *a priori* knowledge. Kripke claims that certain introductions of names by description can give rise to such knowledge. Borrowing from [Evans 1973], assume we have introduced the name 'Julius' as a name for whoever invented the zipper. Then it would seem that we know *a priori* that Julius invented the zipper. However, it is merely contingent that Julius did so, since he

might have died in infancy and someone else might have gone on to invent the zipper. Thus we know *a priori* a contingent truth.

Note that a crucial step in achieving this knowledge is knowing that the name 'Julius' refers to Julius. This knowledge about reference allows us to take our knowledge of the claim:

(189) The referent of 'Julius' invented the zipper.

which we know simply in virtue of our linguistic intentions, to knowledge of the claim:

(190) Julius invented the zipper.

On my account, however, one does not have trivial knowledge, as one does on the Kripkean account, of reference axioms of the form:

(191) 'Julius' refers to Julius.<sup>157</sup>

If names are free variables, there are no such axioms, because there are no (semantic) referential facts for such axioms to capture. One may be unaware of what individual one is talking about with the use of name, if one is depending heavily on the coindexing relations to supply the expressed proposition. One will only know what object one is talking about if one has a concept of that object.<sup>158</sup> Because one's knowledge of one's reference is thus attenuated, the status of Kripke's examples of contingent *a priori* knowledge is complicated somewhat.

A speaker can still express the proposition that Julius invented the zipper, even if he has no concept of Julius. That speaker will

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<sup>157</sup>I leave open exactly what level of triviality is involved in the knowledge of reference axioms. I admit to some skepticism about all available stories about how we come to know such axioms for names of individuals of whom we have no concept. See §2.3.2.5 for further discussion.

<sup>158</sup>Dummett suggests ([Dummett 1981, 591]) that this restriction on knowledge of reference is adequate to block the introduction of irreducibly predicative knowledge.

simply be unable to grasp the proposition that he is expressing, although he will know, by way of knowing *a priori*:

(192) 'Julius invented the zipper' is contingently true.

that he is expressing a contingently true proposition -- just not which one in particular. The resulting position is to this extent similar to that endorsed by [Donnellan 1977], but it has a more natural answer to a criticism raised by [Evans 1979].

Both Donnellan and I hold that, under certain circumstances, a speaker will fail to understand (that is, fail to know what proposition is expressed by) his utterance of:

(190) Julius invented the zipper.

despite knowing that his utterance is true. For Donnellan, however, this failure of understanding is rather mysterious, since the speaker can be fully competent as a speaker of the language and still fail to comprehend his own utterance.<sup>159</sup> Treating the name 'Julius' as a free variable, however, removes the mystery from the position of the speaker. Complete knowledge of the semantic rules for the language no longer suffices to determine which proposition is expressed through an utterance of (190), simply because no proposition is expressed -- on the semantic level -- through such an utterance. To know what proposition is meant, one must master all pragmatically relevant facts, and no degree of *semantic* mastery can ensure one of such knowledge.

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<sup>159</sup>Or, if failure to be acquainted with the referent of 'Julius' prevents the speaker from being fully competent, then we may well discover that there are no (and never have been) any competent speakers of the language.

#### **§2.3.2.4.3 A Derivation of Kripkean Results**

I want now to turn to three distinctive aspects of the *Kripkean* account of names: the claims that names are rigid designators, refer in virtue of a causal chain of uses, and have no mediating sense. I think there is something right about all three of these claims, and want to explore the extent to which my account can recreate them. But I also want something stronger. Kripke *stipulates* these features as features of proper names; he gives no explanation of why names should behave in this way. I want briefly to illustrate that on my picture we can, in essence, derive these three features of names.

##### **§2.3.2.4.3.1 Rigidity**

Kripke famously observes that proper names are rigid, that they pick out the same individual with respect to all possible worlds. While he musters evidence for the rigidity of proper names, however, he never explains *why it is* that they are rigid. My account, on the other hand, both predicts and explains that rigidity. On my picture, our understanding of the concept lying behind the predicate of which the proper name is an argument leads us to see that an object needs to be provided where the free variable stands in order to express a proposition. Once we provide that object, however, it follows trivially from the modal law of necessary self-identity that it is that very object which is relevant also in the counterfactual truth conditions of the proposition expressed.

In a way, the very phenomenon of rigidity disappears on this account to be replaced with a much more trivial metaphysical truism. There is no longer a semantic fact which is to be held steady across

worlds; there is merely a fact about object identity which actually does remain steady across worlds. If anything, it is lack of rigidity which needs explanation on this way of looking at things -- an explanation which will be forthcoming through the possibility of scope interactions in variable binding noun phrases, the only type of noun phrase not exhibiting rigidity.

#### **§2.3.2.4.3.2 Causal Chains**

Kripke claims that if we want to find the referent of a name used by an agent, we should follow back a chain from that agent to the person from whom that agent acquired the name to the person from whom that person acquired the name and so on until we reach a person who originally introduced the name -- the baptizer. The name, as used by the last agent in the chain, will refer to whatever object the baptizer intended to refer to when he introduced the name. Again, what Kripke does not tell us is why names should behave in this fashion. The free variable picture of names provides us with an answer.

First, however, we must modify the causal chain claim slightly. It isn't true that an agent's use of a name refers to whatever the baptizer baptized with the name -- simply because it isn't true that the name refers at all. What is true is that the agent talks about, with a use of the name, whatever the baptizer baptized with the name. Of course, this may not be the only object he talks about. As noted above, just as a speaker may express more than one proposition using a name-containing open sentence, so may an agent talk about more than one object using the name. The agent is thus perfectly free to have any object at all in mind when he expresses a proposition using a sentence containing a given



name; to talk about Schmidt while using the name 'Gödel'. However, he will also be talking about Gödel, whether he realizes it or not.

This claim follows trivially from the nature of the coindexing intentions we as speakers typically have. Whether I intend my utterance of 'Gödel' to be coindexed with all phonetically identical tokens, or with the transitive closure under coindexing of my previous uses of 'Gödel', I will pick up the original use of the name 'Gödel', which will have been used to talk about Gödel, and thus I will be talking about Gödel (perhaps among others). The question thus becomes: why do I have these coindexing intentions, rather than others? Presumably the answer is something like: the whole point of picking a phonetic form for the variable was to indicate my coindexing intentions, so these intentions ought to appeal to that form, and that the very idea of coindexing has built into it a pull toward transitive closure under the relation.

Of course, nothing forces me to have these kinds of intentions. Since it's up to me what coindexing relations to impose, there can be cases in which the causal chain condition fails. We can thus also account for the 'Madagascar'-type cases of reference shift. If my use of 'Madagascar' is coindexed with all phonetically identical tokens, then I will be committed to talking about both the island and the east coast of Africa. Since I don't want to talk about the coast,<sup>160</sup> and since the very early uses of the name are not important to me, I will coindex my use only or more firmly with more recent uses. By considering in more

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<sup>160</sup>When, that is, I am aware of what I am talking about. When I'm not, then the reference shift will run entirely on the relative strengths of my coindexing intentions.

detail my reasons for coindexing as I do, we can develop more thorough accounts of reference shift.<sup>161</sup>

### **§2.3.2.4.3.3 Senselessness**

I said above (§2.3.2.4.2) that we have, in a way, regained the Fregean doctrine that all reference is in virtue of a reference-determining sense, since agents, if they are successfully to entertain the propositions in play when names are used, must have some cognitive capacity in virtue of which they are able to have thoughts about the relevant objects. Nevertheless, we also retain the force of the Kripkean claim that names refer without the intervention of a mediating sense. The reason for this is simply that these cognitive capacities are merely tools for locating the proposition to be grasped, not constitutive parts of the proposition. My acquaintance with Alfred Hitchcock enables me to grasp propositions that have him as a component, but the proposition I then entertain will have merely the individual in it, not the descriptive methods through which I single out the individual in my thought. Thus if an agent has thoughts about Aristotle under the description 'the last great philosopher of antiquity', his utterances of:

(172) Aristotle was fond of dogs.

will be used to express propositions containing only Aristotle, the individual, as a component, and which bear no important semantic relation to the proposition expressed by:

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<sup>161</sup>In particular, an Evans-style 'information-based' account of chains of reference (as developed in [Evans 1973]) can likely be derived from some natural conditions on our usual coindexing intentions.

(193) The last great philosopher of antiquity was fond of dogs.<sup>162,163</sup>

#### **§2.3.2.5 Near-Names and Extensions of the Free Variable Paradigm**

Given the apparent profitability of moving features of proper names from the semantics to the pragmatics, we might wonder how far this general strategy can be pursued. My position, as stated earlier, is that all (and only) singular terms are to be treated as free variables. This position highlights two questions: (i) how broadly are we to construe the class of singular terms, and (ii) why should we not allow other linguistic categories to receive their 'semantic' analysis entirely through pragmatics? Let's consider these questions in reverse order.

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<sup>162</sup>It is instructive to consider how my view on names compares to that developed by [Evans 1982]. Evans retains the Fregean doctrine that all reference is in virtue of a mediating sense, but develops a notion of sense in which proper names receive their sense (in the general case) via the capacity of the speaker to recognize the object named. Since such recognitional capacities will, in general, be particular to the speaker, no two agents will share the same sense for a given proper name, and thus no two speakers will express the same thought by using a given sentence with a given proper name. Evans thus lowers the standard of successful communication from that of conveying a particular thought to that of conveying a thought which is related to the thought had by the speaker by virtue of a common *Bedeutung* of the proper name.

Evans thus takes the psychological mechanisms of reference fixing, which I take to be mere pragmatic factors irrelevant to the semantics of the language, and imports them into the semantics. In doing so, he pays the price of weakening the intuitive connection between thoughts (or propositions) and publicity of information and communication. On the other hand, he retains the ability to use standards of rational acceptability as a method of thought individuation, since he can retain the idea that 'two' thoughts are genuinely distinct if a rational agent can accept one and reject the other.

<sup>163</sup>For those firmly opposed to the very idea of having thoughts about an individual rather than thoughts of a descriptive content which happens to pick out the individual, one could run a story similar to mine in which the relevant cognitive capacities do get into the proposition meant. One would then have to lower the standard of successful communication to require not grasp of the same proposition, but grasp of propositions related by exchange of co-denoting definite descriptions (roughly). There would remain technical difficulties in ensuring the desired rigidity results, although these might be surmountable using some of the techniques of partial binding developed in §2.2.2.2.1.3 above.

Of course, were we to discover that, say, we could treat all sentential connectives as free variables (in an appropriate higher-order language) and show that independently motivated pragmatic devices would naturally give rise to the desired range of readings for sentences containing such connectives, I would be all in favor of doing so. However, there are at least two substantial reasons to doubt that such a treatment is forthcoming. First, recall that one reason for treating singular terms as free variables is that analysis of noun phrases also requires parallel instances of *bound* variables, including bound uses of pronouns and demonstratives. Making singular terms into free variables thus (i) exploits a degree of quantification already at play in the language, and (ii) opens up the possibility of a more unified syntactic and semantic theory. But natural language contains no straightforward examples of quantificational binding of variables in any other than the (first-order, objectual) noun phrase syntactic position.<sup>164</sup> Thus treatment of other syntactic categories as free variables is unlikely to proceed as naturally as or to yield the same virtues as such treatment of singular terms.

Second, our (apparent) knowledge of the semantics of proper names differs substantially from our knowledge of other semantic facts.

Consider the following two semantic axioms:

(AX11) 'Aristotle' refers to Aristotle.

(AX12)  $(\forall x)(x \text{ satisfies 'snores' iff } x \text{ snores})$

To learn (AX12) requires having a concept of snoring and then drawing the appropriate correlations between that concept and uses of the

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<sup>164</sup>See §3.2.2.2.1 below for discussion of some constructions, such as VP deletion, which may be indicative of higher-order quantification in natural language.

lexical item 'snores'. But learning (AX11) requires no such inferential process. It is, one is tempted to say, simply *a priori* that 'Aristotle' refers to Aristotle. Even if knowledge of (AX11) is *a posteriori*, such knowledge certainly seems easy to come by and requires no acquaintance with or knowledge of Aristotle. This is a funny sort of knowledge, and it is one of the virtues of treating proper names as free variables that we eliminate the need for axioms like (AX11) and thus avoid altogether the epistemic questions about our knowledge of the semantics of names. Loosely speaking, then, there just isn't that much there in the semantics of names, and thus not that much to lose when we empty that semantics. But other syntactic categories have richer semantics, and we should be reluctant to abandon that semantics without adequate motivation.<sup>165</sup>

This brings us to the second question: how broadly are we to construe the class of singular terms. I have indicated that I want all of proper names, third-person pronouns, demonstratives, and indexicals to be treated as variables (free in the appropriate contexts). But consider, say, number terms, 'names' of abstract objects, or natural kind terms (of the sort argued in [Kripke 1980] and [Putnam 1970] to be importantly similar to proper names) -- terms which we will call 'near-names'. Should 'two', 'justice', or 'gold' be treated as free variables?

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<sup>165</sup>Singular terms other than proper names, of course, do (apparently) have non-trivial semantic features, such as the feature of 'I' which determines that it refers to the speaker of an utterance, or the feature of 'she' that determines that it refers to a female. I concede that these richer semantics provide a *prima facie* argument against assimilating all singular terms to the free variable treatment of proper names. However, I argue in §2.3.4 below that it is impossible successfully to square these apparent semantic features with the rigidity of singular terms. If this is correct, then we must abandon these features as semantic features anyway, so they provide no bar to a generalization of the 'free variable' strategy to these areas.

I think a definitive answer here is hindered by our currently inadequate understanding of the linguistic behaviour of near-names, but my general suspicion is that the 'free variable' strategy is not profitably extended to these areas. The semantics of near-names are importantly richer than the semantics of proper names. In particular, all of the cited terms have important semantic relations to other terms (or other uses of the same terms) which are not plausibly treated as free variables. Thus consider the following pairs:

(194a) **Two** is a prime number.

(194b) There are **two** apples of the table.

(195a) **Justice** is a virtue.

(195b) Socrates was a **just** man.

(196a) **Gold** is a metal.

(196b) Most **gold** is yellow.

No treatment of these near-names as free variables will be able to respect the important semantic connections between the highlighted terms in each pair. There must, for example, be enough in the semantics of 'two' to explain how it can be used both as a noun and as an adjective. Treating 'two' on a par with 'Aristotle' as a free variable will not allow such an explanation. The negative thesis that near-names are not free variables, of course, leaves wide open the question of what the appropriate semantics for them is -- a question which becomes more pressing once we reject proper names as a model for the desired semantics. While I suspect that a sufficiently ingenious application of the devices of generalized quantifiers, hidden definite descriptions, and *de facto* rigid designation will allow a profitable start on the

analysis of near-names, it lies well beyond the scope of this work to provide such an analysis.<sup>166</sup>

### §2.3.3 Demonstratives

The 'free variable' model of the singular term has now been given a syntactic motivation for deictic pronouns and fleshed out into a detailed semantic and pragmatic story for the case of proper names. I now want to look at the case of demonstratives. The syntactic considerations which help motivate the position for the case of deictic pronouns carry over to the case of demonstratives. Just as we can make use of pronouns as bound variables in ways which make deictic pronouns look much like free variables:

(98-NEW) Every boy is such that he read some book.

(170) He read some book.

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<sup>166</sup>For those who, despite the above considerations, find it impossible to give up the view that 'Aristotle' really does refer to Aristotle, I can offer the following consolation position. We already know there can be pragmatic constraints on the use of a word which do not rise to the level of semantic content. In more barbaric times when our language was less logically perfect, we had customs of women adopting their husband's names and of naming children based on their position in the family, customs which placed pragmatic constraints on the use of proper names. Such constraints are largely dead now (there's no reason semantic or pragmatic to believe that Quentin Tarantino is a fifth child), but there remain a host of particular pragmatic demands such as 'Use 'Aristotle' when talking about Aristotle' (these particular constraints will presumably derive from some overarching principle). I'm quite happy to concede that there are such pragmatic constraints at work with our use of names and that they can be quite strong. I would tend to interpret our desire for 'he' to talk about someone male, or for 'I' to talk about the speaker, as manifestations of these kinds of constraints. Notoriously, the exact line between pragmatic and semantic constraints on the use of language is hard to draw. The use of icons in computer programs provides a nice example. There's no determinate point at which the use of a bold '**B**' ceases merely to be a useful tool for getting people to realize that this icon changes text to bold and starts instead to mean 'Bold!'. At some point, when a pragmatic constraint becomes hardened enough, it probably becomes silly not to recognize it as a semantic constraint. The fan of semantically referential proper names is free to argue that names have reached this point.

we can also make use of demonstratives as bound variables in ways which make some demonstratives look much like free variables:

(197) Every boy is such that that boy read some book.

(198) That boy read some book.<sup>167</sup>

Also, just as we could appeal to pragmatic mechanisms to move from semantically presented incomplete propositional matrix to pragmatically conveyed complete proposition in the case of proper names, we can similarly appeal to such mechanisms to explain communication using demonstratives.

The groundwork, then, for incorporating demonstratives into the free variable model has already been laid. In this section I want to take up two problems which particular to the case of demonstratives. First, we will consider whether the apparent context sensitivity of demonstratives implies a richness of semantic content governing their behaviour inconsistent with the free variable analysis. Second, we will consider whether the complex demonstrative construction threatens the very possibility of distinguishing cleanly between referential and quantificational noun phrases.

#### **§2.3.3.1 Demonstration and Referential Intention**

Demonstratives are apparently context sensitive, in that what a given demonstrative refers to depends on the context in which it is used (or the context with respect to which it is evaluated). This context

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<sup>167</sup>There is a question here about why we use the complex demonstrative 'that boy' rather than the simple demonstrative 'that':

(FN 66) Every boy is such that that read some book.

Perhaps here the subsequent descriptive material provides the kind of phonetic correlation between binding operator and bound variable the absence of which made it impossible to use proper names as bound variables. For more details on complex demonstratives, see §2.3.3.2 below.



sensitivity in itself poses no threat to my free variable model. Since free variables have no semantic content, what they are used to talk about will depend on facts about the psychology of the speaker, facts which are liable to change from context to context. What does threaten the free variable model is that the context sensitivity of demonstratives appears to be rule-governed in a way which is inconsistent with my model. On my model, the context sensitivity of a term is nothing more than a semantic void which leaves the speaker free to use it as he desires. Any semantic constraints on the behaviour of a singular term in a context contradicts my claim that there is no semantic content to such terms. In this section, then, I want to address two ways of spelling out such semantic constraints, and show that neither is adequate to explain the behaviour of demonstratives.<sup>168</sup>

#### **§2.3.3.1.1 Demonstratives and World-Centered Context Sensitivity**

The first approach to the semantics of demonstratives (exemplified in, e.g., [Davidson 1967]) is to treat them as going proxy for, or as being interpreted by, certain definite descriptions or other quantified noun phrases. Thus, for example, we might incorporate into our semantics an axiom of the form:

(AX13) 'That is F' is true iff the object demonstrated by the  
speaker is F.<sup>169</sup>

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<sup>168</sup>In §2.3.4 below I take up again these two approaches to context sensitivity (in a rather more general form) and argue that for deep theoretical reasons neither is in principle capable of capturing the phenomenon of context-sensitive rigid reference. For now I make no in principle argument against these methods as such; I merely argue that they cannot explain the particular uses to which we put demonstratives.

<sup>169</sup>This is, of course, an overly simplistic version of the sort of axiom needed to get the right truth conditions. When intensional contexts are considered, we will need to actualize the relevant description in order to avoid picking out the wrong object when considering world in which the speaker demonstrates differently from the actual world. We will also

This approach thus treats demonstratives as world-centered context sensitive terms. The context sensitivity of the demonstrative is captured by appealing to the prior context sensitivity of predicates like 'speaker' and 'demonstrate', predicates which obtain their context sensitivity via a semantic sensitivity to the state of the world.

The worry with this approach however, is that the necessary world-centered context sensitivity may not be available. While the notion of a speaker is itself not without problem, I want here to focus on the question of whether any adequate notion of demonstration is available to lie behind the appeal to demonstrated objects in the semantic analysis. I take it that it is obvious that no purely physicalistic notion of demonstration will serve the needed role. We cannot, for example, simply trace out a straight line from the finger of the speaker to find the demonstrated object. Setting aside pedantic (but still significant) worries such as determining the exact angle of the ray from a less than perfectly straight finger, we will almost always get the wrong truth conditions, since the first object encountered by such a line will presumably be some small subatomic particle which is not the appropriate demonstratum.<sup>170</sup> Attempts to fix this difficulty, such as appeal to the first medium-sized or prominent object along the line of demonstration,

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need to modify the description in order to accommodate cases in which the speaker uses multiple demonstratives and thus makes multiple demonstrations, as in:

(FN 66) That is larger than that.

The first of these two problems is taken up in §2.3.4.4.1 below. The second is largely an engineering difficulty, although it is worth noting that my own account, due to its theory of coindexing, has no comparable difficulty.

<sup>170</sup>Assuming, that is, than any suitably physicalistic notion of the first object along the line is available in the first place. Presumably any number of mereological sums are encountered at the same moment that the first particle is encountered.

inevitably either founder when unexpected types of objects are demonstrated or illegitimately import psychological notions.

An adequate account of demonstration, then, must take notice of the psychological states of the demonstrator. Furthermore, consideration of how demonstratives are actually used will reveal that the appeal to the psychological states of the demonstrator cannot be further bolstered by additional appeal to physical facts about demonstration or to broader interpersonal psychological facts about conversational salience. First note that demonstratives can be used to refer to past, future, or wholly abstract objects to which there is clearly no possibility of a physical demonstration:

(199) Those were annoying people; I'm glad they left.

(200) This is my favorite scene in the movie coming up.

(201) That argument is full of holes.

Note also that a demonstrative can be used spontaneously to refer to some object which one has just thought of, even if that object is not available to demonstrate and even if it has played no role in the conversation and hence has no conversational salience. Thus if one was earlier trying to remember which was Hitchcock's first film, and then later in an unrelated conversation one remembers and blurts out:

(202) That's it -- the one with all the fog.

one can successfully refer to *The Lodger* (even though other participants in the conversation likely will not follow the remark).

In light of such cases, it seems to me that the only fact relevant to determining the referent of a demonstrative is the referential

intention of the speaker.<sup>171</sup> We might thus introduce a new semantic axiom of the form:

(AX14) 'That is F' is true iff the object the speaker intended to refer to is F.<sup>172</sup>

We do have here a semantic analysis with the appropriate world-centered context sensitivity. The worry now is whether such an axiom plays any useful role in the larger semantic theory. Following [Kripke 1977], we can make a distinction between semantic referent of a term, which will be provided by the rules of the language, and the speaker's referent with that term, which will be determined via pragmatic considerations. Assuming a roughly Gricean intention-based theory of meaning, the pragmatically conveyed proposition will be that proposition that the speaker intends his audience to grasp, and thus the pragmatically

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<sup>171</sup>The classical case for distinguishing between the importance for demonstrative reference of referential intentions and demonstrative acts is due to [Kaplan 1989]. Here Kaplan sitting at his desk points behind him and says:

(FN 67) That is one of the smartest men of the twentieth century. He believes that there is a photo of Carnap behind him, but unbeknownst to him that photo has been replaced by a photo of Spiro Agnew. The question, then, is which person he refers to. The honest answer to the question, it seems to me, is that intuitions are severely divided here. One wants to say that there is some sense in which he refers to both. However, Kaplan's case strikes me as being in principle in capable of favoring a demonstration-based account over an intention-based account. Even if one feels wholeheartedly that Kaplan refers to Agnew, one can explain this by noting that his referential intentions, to refer to the man in the picture behind him, do in fact single out Agnew. The real problem here is that when one singles out an object of thought, one typically does so through a cluster of mental capacities, including descriptions, memories, sensory impressions, and recognitional capacities. However, there is obviously no guarantee that all members of this cluster hook onto the same object, so we must decide what to say about cases in which there is a divergence. One response is to hold that the agent thinks about several objects without realizing that they are several. This response would lead to the view that Kaplan refers to both Agnew and Carnap as a result of his (fractured) referential intentions.

<sup>172</sup>Modulo, of course, the worries about this form of axiom raised in footnote 168 above.

conveyed speaker's referent will be whatever object the speaker intends to refer to. We thus have the following generalization:

(SpR) For all uses  $u$  of term  $t$  by speaker  $s$ , the speaker's referent of  $t$  on  $u$  is the object  $s$  intended to refer to with  $t$  on  $u$ .

But if we then adopt a semantics along the line of (AX14) for demonstratives, as it seems we must, then the semantic referent and the speaker's referent will be determined by exactly the same procedure. Methodological principles of minimalism, motivated by something like Grice's Modified Occam's Razor, would seem to speak against having this sort of redundancy built into one's theory. Since the speaker's referent falls out from general considerations about the norm-governed nature of communication and the intentional basis of meaning, the most obvious route for eliminating the redundancy is to eliminate the semantic rule.

The upshot, then, is that the only way to use world-centered context sensitivity to capture the referential behaviour of demonstratives is to introduce a rule which is wholly redundant in the face of preexisting pragmatic devices. In light of this, theories which exploit such world-centered context sensitivity seem a poor alternative to an account, such as that proposed here, which recognizes the referential adequacy of the pragmatically provided speaker's reference and thus makes no attempt to build context sensitivity into the semantics (in my case, by leaving the semantics entirely empty).

#### **§2.3.3.1.2 Demonstratives and Agent-Centered Context Sensitivity**

The second approach to the semantics of demonstratives (exemplified in, e.g., [Kaplan 1977]) treats them as agent-centered context sensitive

terms. The standard method for introducing agent-centered context sensitivity is to replace the notion of truth simpliciter with the notion of truth *with respect to a context*.<sup>173</sup> We can construe a context as an ordered n-tuple of objects specifying various relevant features of the environment of the speaker. Thus, for example, a context of the form:

$$C = \langle s, a, t, p, w \rangle$$

which specifies a speaker, an audience, a time of utterance, a place of utterance, and a world of utterance will allow us to introduce appropriate semantic axioms for various agent-centered context sensitive terms:

$$(AX15) \text{Ref}('I', C) = C_s^{174}$$

$$(AX16) \text{Ref}('you', C) = C_a$$

$$(AX17) \text{Ref}('now', C) = C_t$$

$$(AX18) \text{Ref}('here', C) = C_p$$

$$(AX19) \text{Ref}('actual', C) = C_w$$

This method of implementing agent-centered context sensitivity captures the rigidity of the various indexicals quite naturally, since we can assume that the various elements of the context are named rigidly and thus are immune to the effects of intensional contexts.<sup>175</sup>

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<sup>173</sup>More generally, for any semantic property *P*, we replace the property *P* simpliciter with the property *P* with respect to a context. We may, however, want to have some semantic properties (such as Kaplanesque character) which are not relativized to context.

<sup>174</sup>Where (a) reference has been relativized to a context and (b)  $C_s$  refers to the speaker element of the context (*mutatis mutandis* for  $C_a$  etc.).

<sup>175</sup>In fact, when we spell out such a theory in detail, we will find that the references to elements of the context are actually bound variables, bound by an initial specification of the context. Since that specification, being metalinguistic, will be outside the scope of any intensional operators in the sentence under analysis, the bound

By extending our notion of context, we can use the same methods for providing a semantic analysis for demonstratives. We might thus replace the ordered five-tuple used above with an ordered six-tuple of the form:

$$C = \langle s, a, t, p, w, d \rangle$$

where the sixth element picks out the object demonstrated in context. We would then have an axiom of the form:

$$(AX20) \text{Ref}('that', C) = C_d^{176, 177}$$

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variables will be indifferent to such operators. See §2.3.4.4.2 below for further discussion.

<sup>176</sup>Presumably also a similar axiom for 'this'. I set aside for the current purposes the (rather difficult to spell out with precision) distinctions between 'this' and 'that'. Similar axioms will also serve for 'these' and 'those', although we will need a theory which accommodates plural reference (see §3.1.1 below).

<sup>177</sup>In fact, the details are more complicated than this. Since we can have more than one demonstrative in a single sentence, with the different demonstratives referring to different objects, as in:

(FN 66) That is larger than that.

we will have to have more than one argument position in the specification of context for demonstrated object. Since a sentence can in fact have any arbitrarily large finite number of demonstratives -- consider:

(FN 68) That is larger than that and that and that and ...

-- we will need an infinite series of demonstrated objects in the context. Demonstratives will then be indexed to various positions in the context  $\omega$ -tuple and will receive their referent from context according to their argument.

Looking at demonstratives in this way makes very forceful the analogy between a demonstrative and a variable under an assignment, with the context playing the role of the sequence assigning a value to the variable. One might think as a result that in general the notion of a variable under an assignment is a better model for the singular term than my notion of a free variable, since a variable under an assignment (i.e., with respect to a sequence (context)) does have a referent. However, I hope that the considerations of the next paragraph will show that the use of context specifications to explain the semantic behaviour of demonstratives is seriously flawed. I thus set aside in the main text the idea of treating singular terms as variables under assignment.

Note that on the anaphoric account of variable binding there is no need for the very notion of a variable under an assignment. The use of satisfaction by sequences in the Tarskian semantics is essentially a technical device to square the fact that the semantics of bound variables is unresolved until a binding operator has its effect on them with the desire for a compositional semantics working from the inside out. My semantics, on the other hand, is openly non-compositional in its appeal to anaphoric relations which pass content from predicate expressions down the syntactic tree to variables, and once these

Using such an axiom, we can assign truth conditions relative to a context for sentences such as:

(203) That is a tree.

determining that is true iff  $C_d$  is a tree (at  $C_t$ ).

While we can make sense of a sentence with a demonstrative considered with respect to any given context, the difficulty comes in knowing with respect to which context we ought to consider the sentence. Suppose someone utters to me:

(204) That is a bomb.

I know that with respect to context  $C^1$  in which  $C_d^1$  is the unopened package on my desk that (204) is true iff the package is a bomb, and that with respect to context  $C^2$  in which  $C_d^2$  is the suitcase standing by the door that (204) is true iff the suitcase is a door. But clearly I need to know more than this to make good use of an utterance of (204). I need to know whether I ought to consider it relative to  $C^1$  or to  $C^2$ , or to some other context.<sup>178</sup>

I need, that is, some method for translating the world into a formal context of the form given above. Some of that translation, of course, is straightforward. I can supply the appropriate first element in the six-tuple --  $C_a$  -- quite easily if I know who the speaker of (204) is. But how do I determine what  $C_d$  is? We face here again the problems of the last section in spelling out the notion of

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anaphoric transfers have taken place, the variable has a semantic content which obviates the need for sequences. See §3.4 for further discussion.

<sup>178</sup>The problem I raise here is that of getting truth (*simpliciter*) of an utterance out of a system designed to issues in judgements of truth-in-a-context. The worry is thus similar to that raised by [Lepore 1983] against the reliance on the notion of truth-in-a-model by model theoretic semantics.



demonstration. An agent-centered theory will be in just as much need as a world-centered theory of an account of demonstration if it is to serve in understanding of actual utterances, rather than just in abstract grasp of truth conditions relative to an (unspecified) context. But we saw in the last section that notions such as pointing or other physical demonstrations or such as interpersonal conversational salience fail to capture the true behaviour of demonstratives.<sup>179</sup> I conclude, then, that there is no satisfactory way of specifying the appropriate context of utterance which does not, as in the last section, needlessly duplicate work already being done in the pragmatics.

#### **§2.3.3.2 Complex Demonstratives and Appositives**

Much of the difficulty in producing an adequate account of demonstratives comes from the case of complex demonstratives. Complex demonstratives are constructions which concatenate a demonstrative with an N' phrase, to create phrases such as:

(205) that man in the corner

(206) those admirers of Godard's early work

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<sup>179</sup>[Kaplan 1977] takes the semantic content of a demonstrative to be provided via a demonstration, but [Kaplan 1989] retreats from this position:

In *Demonstratives* ... I claimed that the demonstration rather than the directing intention determined the referent. I am now inclined to regard the directing intention, at least in the case of perceptual demonstratives, as criterial, and to regard the demonstration as a mere externalization of this inner intention. The externalization is an aid to communication, like speaking more slowly and loudly, but is of no semantic significance. [Kaplan 1989, 582]

The more recent Kaplanian position seems to me naturally to lead toward my conclusion that the communicative import of the speaker's referential intentions are best captured pragmatically through the notion of speaker's reference.

Complex demonstratives thus *prima facie* straddle the gap between quantificational and referential noun phrases. On the one hand, they exhibit the same syntactic structure as quantificational noun phrases, combining (what certainly in this context seems to be) a determiner with an N' serving as the restrictor on the quantifier. This similarity to quantificational noun phrases has led some to try to treat complex demonstratives quantificationally, either by taking 'that' and other demonstratives as quantifiers themselves or by taking such demonstratives to induce (actualized) definite descriptions.<sup>180</sup> On the other hand, complex demonstratives clearly have some semantic allegiance with the (clearly referential) case of pure demonstratives, and continue to exhibit the rigidity characteristic of referential noun phrases. Thus in a sentence such as:

(207) That man in the corner could have stayed home tonight.

the phrase 'that man in the corner' continues to refer to the demonstrated man even with respect to worlds in which he is not in the corner (or even in which he is not a man, if there are such).

The challenge for the theorist, then, is to develop an account of complex demonstratives which can respect the intuition that complex and pure demonstratives are of a class while explaining the syntactic form of complex demonstratives with its apparent appeal to a semantic role

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<sup>180</sup>There are difficulties with both of these routes. The first route requires a semantic understanding of quantification which allows for context sensitivity in quantifiers (combined, perhaps, with an explanation of why no other quantifiers exhibit such sensitivity); the second requires an adequate explanation of the rigidity of complex demonstratives and faces difficulties in cases of misidentification or failed demonstration. See [Ludwig & Lepore (forthcoming)] for a more thorough treatment of difficulties facing quantificational treatment of complex demonstratives, although the positive proposal of that paper is itself not without difficulties.

for demonstrative words quite different from that found in pure demonstrative cases.

#### **§2.3.3.2.1 Disanalogies Between Complex Demonstratives and Quantified Noun Phrases**

The first step in meeting the challenge just posed lies in arguing that quantificational noun phrases are not in fact the right syntactic model for complex demonstratives. While there is certainly a *prima facie* similarity between the two constructions, I want to point to two important differences between them, differences lying at the intersection of syntax and semantics and differences which serve to cast doubt on the claim that the two constructions are importantly related.

The first difference is that complex demonstratives, unlike quantificational noun phrases, do not engage in scope interactions. Ordinary multiple-quantifier sentences, of course, will not serve to establish this conclusion. While it is true that a sentence such as:

(208) Every film critic loves that scene with Joseph Cotton.

admits of only interpretation while the related sentence with a quantificational noun phrase in place of the complex demonstrative:

(209) Every film critic loves some scene with Joseph Cotton

admits of two, this lack of ambiguity is to be expected of a quantifier which necessarily denotes a single object and is shared by (what I take to be) the uncontroversially quantificational definite description:

(210) Every film critic loves the scene with Joseph Cotton.

Just as (210) allows for two truth-conditionally equivalent but differently-scoped readings:

(210-RQ<sub>1</sub>) [every x: film-critic(x)][the y: scene-with-Joseph-Cotton(y)] loves(x,y)

(210-RQ<sub>2</sub>) [the y: scene-with-Joseph-Cotton(y)][every x: film-critic(x)] loves(x,y)

we are free to hold that (208) does involve a quantificational complex demonstrative and merely gives rise to two readings which are truth-conditionally equivalent:

(208-RQ<sub>1</sub>) [every x: film-critic(x)][that y: scene-with-Joseph-Cotton(y)] loves(x,y)

(208-RQ<sub>2</sub>) [that y: scene-with-Joseph-Cotton(y)][every x: film-critic(x)] loves(x,y)<sup>181</sup>

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<sup>181</sup>Matters are admittedly more complicated with plural complex demonstratives. A sentence such as:

(FN 69) Those men read a book.

might seem to share with:

(FN 70) The men read a book.

(FN 71) Both men read a book.

a scope ambiguity which yields one reading on which all the men read the same book and another on which each is allowed to read a different book. I am inclined, however, to take this feature of plural complex demonstratives as resulting not from scope interactions but rather from general features of plural reference. Thus note that:

(FN 72) John and Albert read *Hippolytus* and *The Trojan Women*.

leaves open whether both read both plays or whether each read a different play. Thus a similar ambiguity results here where, since there are no quantifiers, there is no possibility of a scope-based interaction.

As further evidence that the ambiguity of plural complex demonstratives results from features of plural reference rather than features of quantifier scope interactions, note that plural complex demonstratives can also give rise to collective readings of various complexity. Thus:

(FN 73) Those men pushed a car up the hill.

can be true if the men collectively pushed a single car up the hill, while:

(FN 74) Each man pushed a car up the hill.

(FN 75) Both men pushed a car up the hill.

do not allow the collective reading. ((FN 75) may in fact allow the reading as slightly strained, and:

(FN 76) The men pushed a car up the hill.

certainly allows the collective reading. See §3.1.1 below for general discussion of the behaviour of plural reference and §3.3.2.2.3.2 below for a related discussion of cumulative quantification.)

Even cases which will serve to show that definite descriptions undergo scope interactions, such as the ambiguity of constructions involving negation or negation-containing monotone decreasing quantifiers:

(211) The king of France is not bald.

(212) Few Frenchmen have seen the king of France.<sup>182</sup>

do not provide fully convincing arguments that complex demonstratives do not undergo scope interactions, since the lack of ambiguity of the corresponding:

(213) That king of France is not bald.

(214) Few Frenchmen have seen that king of France.

is to be expected given that (a) the two readings of (211), (212) above are distinguishable only in the case when there is no unique king of France (i.e., when the definite description fails to denote) and (b) it is a semantic feature of complex demonstratives that when they fail to denote, there is no proposition expressed and thus no opportunity for two propositional readings with different truth conditions.

To obtain convincing arguments that complex demonstratives fail to exhibit scope interactions, we need to look at cases in which such demonstratives interact with intensional operators. Thus consider cases such as:

(215) That man in the corner could have stayed home tonight.

(216) That governor of California used to be a Democrat.

(217) Albert believes that upright citizen is a spy.

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<sup>182</sup>These examples, and much of this discussion of scope interactions, are borrowed from [Neale 1990, 119-121].

If complex demonstratives are quantified noun phrases giving rise to the usual scope possibilities, then there ought to be two readings of each of the above:

(215-RQ<sub>1</sub>)  $\diamond$ [that x: man-in-the-corner(x)] stayed-home-tonight(x)

(215-RQ<sub>2</sub>) [that x: man-in-the-corner(x)]  $\diamond$ stayed-home-tonight(x)

(216-RQ<sub>1</sub>) P([that x: governor-of-California(x)] Democrat(x))

(216-RQ<sub>2</sub>) [that x: governor-of-California(x)] P(Democrat(x))

(217-RQ<sub>1</sub>) Albert-believes([that x: upright-citizen(x)] spy(x))

(217-RQ<sub>2</sub>) [that x: upright-citizen(x)] Albert-believes(spy(x))

However, each of (215)-(217) above is in fact unambiguous, so we have here a considerable difficulty for the claim that complex demonstratives undergo scope interactions.<sup>183</sup> The best that can be done in the way of

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<sup>183</sup>The case of complex demonstratives and propositional attitude contexts is perhaps somewhat less straightforward than the other two, although I think careful consideration here will indicate that (217) is unambiguous and that any perceived ambiguity is of the same sort as that apparently exhibited by the interaction of proper names and propositional contexts, as in:

(FN 77) Albert believes that Superman can fly.

[Ludwig & Lepore (forthcoming)] argues that there are cases in which scope distinctions with complex demonstratives can be perceived, relying on examples such as:

(FN 78) Necessarily, that dog with the blue collar has a blue collar.

and:

(FN 79) No one doubts that necessarily that dog with the blue collar has a blue collar.

Their claim is that (FN 78) allows for a true reading on which the complex demonstrative has small scope with respect to the modal operator, that this true reading is obscured by the fact that it is trivially true, and that (FN 79) helps evoke the true reading of (FN 78). However, I find that there is no true reading, no matter how distantly accessible, of (FN 78), and to the extent that (FN 79) is judged acceptable at all I think it is because the initial epistemic operator 'doubts' inclines us to read the subsequent modality as an epistemic modality. If we take 'necessarily' as indicating epistemic necessity, then the claim may appear true, since presumably any speaker who picks out an object for demonstration under a particular description must believe that the object fits that description and thus must take it as epistemically necessary that that F be F. However, such considerations for the acceptability of (FN 79) require no appeal to scope considerations.

retaining this claim is to hold that complex demonstratives are quantifiers with an unaccountable tendency always to take wide scope. Even if all the problems in such a proposal can be worked out<sup>184</sup>, we are left with complex demonstratives being alone among all quantified noun phrases with this drive toward wide scope<sup>185</sup>, and thus still have reason to doubt that such demonstratives really belong to the quantified NP class.

Complex demonstratives thus fail to display the scope interactions we expect of quantified noun phrases. The second blow against treating complex demonstratives as quantified noun phrases is that they do not allow the kind of binding into the restrictor that quantified noun phrases allow. Each of the following is well-formed:

(218) Every man watched the movie he liked best.

(219) Every man watched several movies he had selected.

(220) Every man found few movies he liked.

In each case, the second quantified noun phrase contains a pronoun 'he' acting as a variable bound by the initial quantifier 'every man':

(218-RQ) [every x: man(x)][the y: movie(y)  $\wedge$  liked-best(x,y)]  
watched(x,y)

(219-RQ) [every x: man(x)][several y: movie(y)  $\wedge$  selected(x,y)]  
watched(x,y)

(220-RQ) [every x: man(x)][few y: movie(y)  $\wedge$  liked(x,y)]  
found(x,y)

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<sup>184</sup>See the Preface to [Kripke 1980] for the most famous problems for generic wide-scope treatment of certain quantifiers.

<sup>185</sup>Despite the (oft-refuted) claims of [Hornstein 1984] to the contrary.

In general, then, a determiner can bind any kind of N' phrase, even one containing a free variable. What the resulting quantified noun phrase denotes will then be dependent on what value is supplied for that free variable.

When we look at complex demonstratives, however, we find that they do not allow binding into their restrictors. Thus the following is ungrammatical:

(221) Every boy read this book he liked.

The ungrammaticality of (221) is unaccountable if we assume that complex demonstratives fall under the rubric of quantified noun phrases.

Some kinds of binding, however, are permissible in complex demonstratives. We can easily allow pronouns within a complex demonstrative bound by proper names external to the demonstrative. Thus:

(222) Albert liked that movie he saw.

is perfectly interpretable and equivalent to:

(222') Albert liked that movie Albert saw.<sup>186</sup>

We can even, in a very limited sense, allow binding of pronouns in complex demonstratives by quantifiers. Thus the following is interpretable:

(223) Several eyewitnesses described that assailant they saw.

Note, however, that it is interpretable only as:

(224) Several eyewitnesses described that assailant the  
eyewitnesses saw.

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<sup>186</sup>Note that despite the interpretability of:

(222) Albert liked that movie he saw.

we cannot perform existential generalization on this sentence to derive:

(FN 80) Someone liked that movie he saw.

since this latter is not interpretable. This is an indication that the syntactic form of complex demonstratives is more mysterious than surface appearances indicate.



The pronoun 'they', that is, must be interpreted as a donkey pronoun picking out *all* the describing eyewitnesses rather than as a genuine bound pronoun. To see this, note that the complex demonstrative 'that assailant they saw' cannot take on different referents for different eyewitnesses. We thus observe two interrelated facts about binding into complex demonstratives. First, such binding cannot create relativity in the referent of the complex demonstrative -- the demonstrative refers always to the same object, regardless of the quantificational behaviour of the lexical context. Second, such binding is always of the cross-clausal rather than the intra-clausal sort. These two facts about binding provide further evidence for the claim that complex demonstratives are not best served by the quantified noun phrase, but they also provide a further puzzle for any adequate account of complex demonstratives.<sup>187</sup>

#### **§2.3.3.2.2 Appositives as Syntactic Model for Complex Demonstratives**

Having cast doubt on the assumption that quantified noun phrases provide the right syntactic model for complex demonstratives, I want to explore the semantic behaviour of another syntactic phenomenon which I will suggest does supply the right model for understanding such

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<sup>187</sup>There are apparently cases in which genuine binding into complex demonstratives is permissible. Thus consider:

(FN 81) Every senator voted for those bills supported by his party.

Here the complex demonstrative 'those bills supported by his party' would seem to pick out different bills depending on the party of the senator, and thus involve standard quantificational variable binding. The issue is complex, but I am inclined to here 'those' in this context as having a distinct sense from the normal demonstrative use (the OED singles out such a distinct use) and as serving rather as an emphatic form of definite description. Note that similar constructions are at best extremely forced with the 'near' demonstratives 'this' and 'these' rather than the 'far' demonstratives 'that' and 'those':

(FN 82) ?Every senator voted for these bills supported by his party.

demonstratives. The phenomenon in question is that of appositives. Appositives are noun phrase or N' constructions concatenated onto other noun phrases, frequently separated off by commas. Thus we have the following:

(225) Aristotle, man of the people, was fond of dogs.

(226) Plato, the greatest metaphysician of antiquity, wrote the *Cratylus*.

(227) The man in the yellow hat, who owns a poodle, is a spy.<sup>188</sup>

#### **§2.3.3.2.2.1 A Multi-Propositional Semantics for Appositives**

Evaluating the truth conditions of sentences with appositives raises interesting issues. Consider again sentence (226). If Plato was in fact the greatest metaphysician of antiquity and was in fact the author of the *Cratylus*, then (226) is straightforwardly true. If, on the other hand, Plato was neither the author of the *Cratylus* nor the

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<sup>188</sup>It would be nice to have some explanation of the clear syntactic relation between appositional unrestrictive wh-clauses like that in (227) and non-appositional restrictive wh-clauses, as in:

(FN 83) The man in the yellow hat who owns a poodle is a spy. The interaction between appositional wh-clauses and quantified noun phrases is somewhat complex. The two can be combined quite naturally when the noun phrase is a definite description, as in (227) above, or an indefinite description, as in:

(FN 84) A senator from Idaho, whom I won't name, accepted a bribe. The two can also be combined with other noun phrases, but here the effect is apparently different. Thus consider:

(FN 85) Most junior professors, who have heavy teaching loads, are overworked.

(FN 86) Several philosophers, who are required to study logic, attended the conference.

(FN 87) No linguists, who have little interest in semantics, attended Davidson's lecture.

In each of (FN 85)-(FN 87), the appositive clause modifies not the quantified noun phrase, but rather the bare plural N' to which a determiner is attached to form that quantified noun phrase. Thus in (FN 84) one asserts not just that most junior professors have heavy teaching loads, but that junior professors simpliciter have heavy teaching loads. See footnote 192 below for my account's ability to explain this feature of appositive/quantified noun phrase interaction.

greatest metaphysician of antiquity, then (226) is unambiguously false.

The interesting cases, then, are:

(S1) Plato was the greatest metaphysician of antiquity; Plato did not write the *Cratylus*.

(S2) Plato was not the greatest metaphysician of antiquity; Plato did write the *Cratylus*.

My intuitions here are that (226) is probably false in situation (S1), but in situation (S2) one wants, I think, to say that there is something right and something wrong about the sentence, and thus to avoid a univocal evaluation. At the very least, this tells us that appositives are not to be treated simply as forming conjunctions; that (226) is not to be understood as:

(226-NEW) Plato wrote the *Cratylus* and Plato was the greatest metaphysician of antiquity.

since (226-NEW) is straightforwardly false in situation (S2).

The right reaction to the split in our intuitions regarding (226) in situation (S2) is, I think, to hold that (226) in fact expresses two propositions. In particular, (226) will express the following two propositions:

(226-P1) that Plato was the greatest metaphysician of antiquity.

(226-P2) that Plato wrote the *Cratylus*.

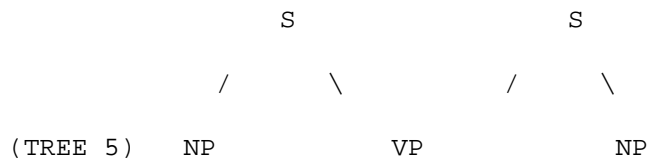
These two propositions will not be further united into a single proposition through the means of any sentential connective, so there is no single unit to which we can ascribe a truth value. Asking simply for a truth value of (226), then, is much like asking for a truth value of:

(228)  $2+2=4$ .  $3+3=7$ .

When the two propositions expressed have different truth values, we will thus be reluctant to call the sentence itself either true or false.

The next question, of course, is why sentences with appositives express two propositions. My suggestion is that it is because such 'sentences' are in fact *two* sentences. In a phrase structure grammar, a sentence is represented by a (or an ordered n-tuple of) phrase structure tree, which is a collection of nodes with a partial ordering imposed on them. At the top of the resulting tree sits the root node, which represents the entire sentence. Assuming a roughly compositional understanding of semantics, we then have semantic values passed upward through the tree via some compositional rule until a final semantic value for the root node (a proposition, a truth value) is achieved.

Phrase structure grammars thus impose the strong condition that the partial ordering which imposes the hierarchy on the nodes contain a maximal element. However, we can also consider grammars which do not carry this maximality condition. In such grammars, trees may have more than one root, and thus the 'tracing upward' of semantic values will result in two termini, representing two propositions expressed. Thus we have a grammar giving rise to 'trees' such as:



in which two sentences share the same verb phrase but supply different noun phrases as subject.<sup>189</sup> That particular construction, of course, is

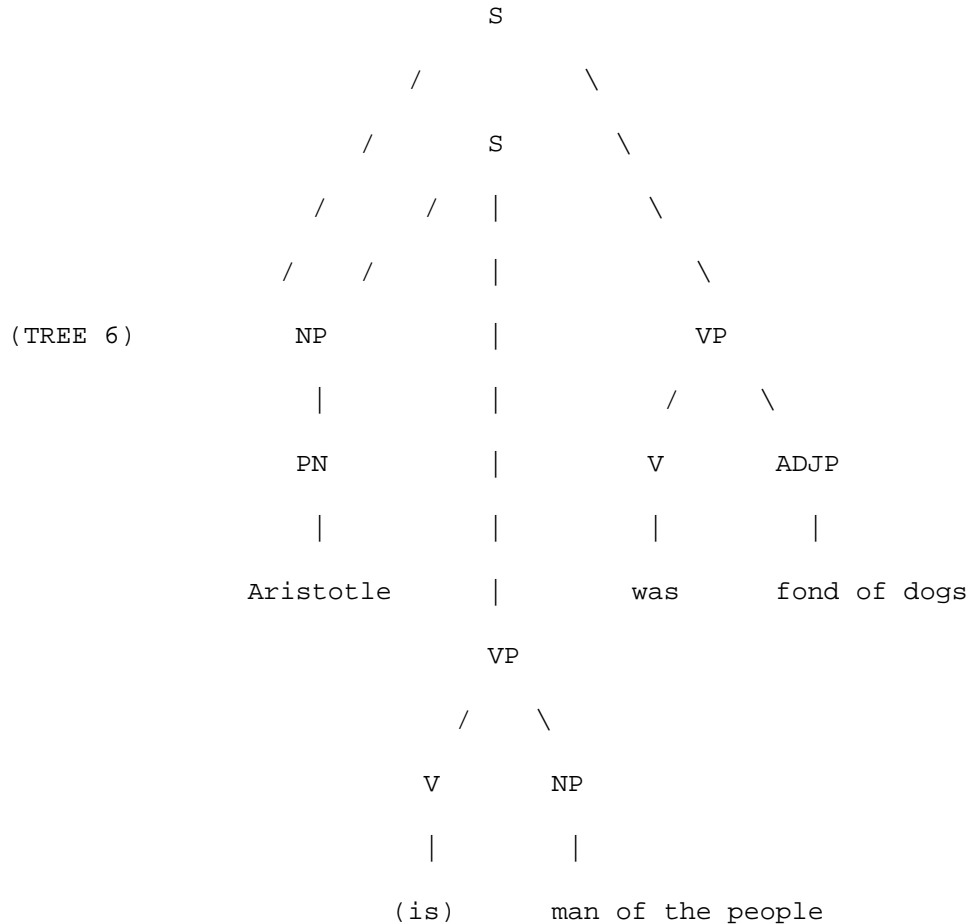
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<sup>189</sup>Considerable work, of course, will be needed in order to adjust the generative mechanisms of the syntactic theory -- whether rewrite rules, X-bar theory, or some other paradigm -- to allow for the generation of multi-headed structures. I leave open here the question of how best to accomplish this work.

not available in English, but my proposal is that appositives give rise to a similar structure. Consider again:

(225) Aristotle, man of the people, was fond of dogs.

We can ascribe a multi-rooted tree of the following form to (225)<sup>190</sup>:



(225) thus contains two top-level S nodes, one dominating the sentence 'Aristotle was fond of dogs' and one dominating 'Aristotle is a man of the people'.<sup>191</sup> As a result, it expresses propositions corresponding to these two sentences, and when the two propositions diverge in truth

<sup>190</sup>(TREE 6) below is best envisioned as three-dimensional, with the second (lower) S node at the same vertical level as the now-higher first S node but protruding at right angles from the plane of the primary tree.

<sup>191</sup>I assume here that the grammar of apposition in English allows for suppression of the copula linking the NP with its appositional description.

value, we are left with conflicted intuitions on the truth value of the whole sentence (as we strive, driven by a mistaken theoretical assumption that there is a one-one correlation between sentences and propositions, to resolve our intuitions into a single truth value).<sup>192</sup>

Appositives are not the only construction in English which call for a syntactic structure more complex than that allowed by standard phrase structure grammar. Natural languages allow for a great deal of flexibility in expressing more than one idea in the same sentence. Most clearly calling for multi-headed tree structures are cases in which we simply interrupt one sentence to insert another, as in:

(229) I told Kripke -- he's teaching a seminar on colors this semester -- that I admire his work.

(229) clearly expresses two separate propositions -- it would be foolish to ask whether (229) was true or false if Kripke was not teaching a seminar on colors but I did express my admiration for his work, or vice versa -- and clearly does so through the incorporation of two primary S nodes into a single 'sentence'.

Other constructions involve more complicated syntactic structures. Sentences which use of slashes to list various options, as in:

(230) Terms which are modally/temporally rigid refer to the same object with respect to every world/time.

arguably express multiple propositions; here:

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<sup>192</sup>This multi-propositional picture of complex demonstratives owes much to [Neale (forthcoming)]'s account of multiple propositions. However, it differs from Neale's picture in deriving the multiplicity of propositions from multi-headed syntactic structures via a standard compositional meaning theory, rather than via Neale's multi-pass propositional construction picture of the syntax-semantics interface imposed on single-headed syntactic structures.

(230-P1) that terms which are modally rigid refer to the same object with respect to every world.

(230-P2) that terms which are temporally rigid refer to the same object with respect to every time.

Working out the exact details of the multi-headed tree for (230) is complicated, but presumably something like two trees superimposed except for the adverbial modifier of 'rigid' and the final noun is involved. Even more complex is the use of parenthetical comments, as in:

(231) Some definite descriptions are rigid (although only *de facto* rigid).

(232) Some definite descriptions are *de facto* rigid (denote the same object with respect to every possible world).

(233) Some definite descriptions are (on Donnellan's view) rigid.

Sentences with parenthetical comments will typically express two propositions, one corresponding to the sentence without the parenthetical comment and one corresponding to a sentence in which the parenthetical remarks supplement or supplant parts of the original sentence. Thus corresponding to (231)-(233) above we have the following propositions expressed:

(231-P1) that some definite descriptions are rigid.

(231-P2) that some definite descriptions are rigid, although only *de facto* rigid.

(232-P1) that some definite descriptions are *de facto* rigid.

(232-P2) that some definite descriptions denote the same object with respect to every possible world.

(233-P1) that some definite descriptions are rigid.

(233-P2) that on Donnellan's account some definite descriptions are rigid.

The intended relation between the core proposition and the parenthetically expressed proposition can be quite complex. In cases like (231) above, both propositions are straightforwardly asserted, with one merely amplifying the other. In cases like (232), one proposition (the parenthetical proposition) is intended to give the sense of the other (core) proposition. In cases like (233), one proposition (the core proposition) is asserted within the context of a pretense (here that Donnellan's account is the right account) while the other (parenthetical) proposition serves to make explicit the relevant pretense.<sup>193</sup>

#### **§2.3.3.2.2.2 Some Consequences of the Multi-Propositional Semantics**

In this section we will provide additional support for the multi-propositional appositional semantics by drawing out two consequences of that semantics. These consequences, it turns out, will also be the key to solving the problem of complex demonstratives.

Note that appositives do not enter into scope relations with operators in the main sentence. Thus, for example, if one utters a negated sentence with an appositive, such as:

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<sup>193</sup>The possibility of such complex relationships between the various propositions expressed will hopefully help explain why, in the case of appositives, we get the disanalogy between the two situations:

(FN S1) Appositional claim true, main claim false

(FN S2) Appositional claim false, main claim true

Intuitions here seem to be that in (FN S1), the sentence as a whole is straightforwardly false, while in (FN S2) we are reluctant to call the sentence either true or false. If we assume that appositives give rise to a primary proposition and a secondary proposition corresponding to the main claim and to the appositional claim respectively, we may be able to explain this disanalogy by holding that falsity of the primary claim obviates the need to consider the secondary claim.



(234) Aristotle, man of the people, was not fond of dogs.

the presence of the negation does not affect one's commitment to Aristotle being a man of the people. There are two aspects of the appositive's independence from the negation. First, there is no structural ambiguity in (234) resulting from various relative scopes of the negation and the appositive phrase. Second, the one unambiguous reading has the appositive unaffected by, and hence outside the scope of, the negation.

These two aspects of independence are a general feature of appositive/operator combinations. Thus:

(235) Aristotle, man of the people, might have been fond of dogs.

(236) Last year Plato, greatest living metaphysician, visited Sicily.

(237) Albert doesn't believe that Frege, author of the *Begriffsschrift*, owned a dog.

All of (235)-(237) are unambiguous. There is no reading of (235) on which it is Aristotle's merely possible common touch, rather than his actual populism, which is relevant. Similarly, (236) unambiguously requires that Plato surpass his current philosophical competitors, not that he have been unsurpassed a year ago. (237) has no reading on which Albert's beliefs about Frege's authorship of the *Begriffsschrift* are relevant. Furthermore, all of (235)-(237) are unambiguous in the same way as (234) above -- in every case, the appositive lies outside the semantic reach of, and hence presumably also outside the syntactic scope of, the intensional (or hyperintensional) operator.

The multi-propositional semantics for appositives provides a natural explanation for this refusal to enter into scope interactions.

On this semantic proposal, the appositive is governed by a different S node from that governing various operators in the main sentence, so those operators can no more affect or enter into scope relations with the material of the appositive than the negation in the first sentence below can affect the interpretation of the second:

(238) Aristotle was not fond of dogs. Aristotle is a man of the people.

The multi-headed syntax effectively places the appositional phrase in a different syntax from all of the main sentence except the NP to which it is appositional; hence, the syntactic constraints on scope prevent operators in the main sentence from having any semantic impact on the appositive.

Appositives also do not generally allow binding of outside operators into the appositive. Thus the following is uninterpretable:

(239) Every man admires Aristotle, friend of his father.

where 'his' is taken as bound by 'every man'. Again, this bar against binding in is explained by the multi-headed syntax lying behind appositional constructions. Since (239) is effectively equivalent to:

(240) Every man admires Aristotle. Aristotle is a friend of his father.

we can no more have binding of the pronoun by the quantified noun phrase in one than we can in the other.<sup>194</sup> Note, however, that certain kinds of binding into appositives are acceptable. Anaphoric relations between proper names and pronouns are unproblematic:

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<sup>194</sup>Note, however, that binding placed entirely within the appositive is unproblematic. Thus:

(FN 88) Aristotle, friend of every man who admires his father, was fond of dogs.

(241) Aristotle told Plato, his former teacher, that Forms played  
no explanatory role.

Also, cross-clausal anaphora between pronouns and quantified noun phrases (or, on my account, undistributed binding of pronouns by N' constructions) is acceptable into appositional phrases:

(242) Most analytic philosophers admire Frege, founder of their  
tradition.

The acceptability of cross-clausal anaphora into appositives is again predicted by the syntactic model lying behind the multi-propositional semantics.<sup>195</sup> That appositional phrases allow all and only those forms of binding which can operate cross-clausally is strong evidence that these phrases lie outside the scope of the primary S node and thus that the multi-headed syntax is the correct model.

#### **§2.3.3.2.2.3 Appositives and Complex Demonstratives**

Having developed and defended this multi-propositional model of appositional phrases, the next step is perhaps obvious. Complex demonstratives share with appositives several features. Neither complex demonstratives nor appositives enter into scope interactions with other operators in the lexical context; both complex demonstratives and appositives allow only those forms of binding in which can operate

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<sup>195</sup>Although the matter is complicated, the multi-headed syntax of appositional phrases may also help explain the behaviour of unrestrictive wh-clauses appositional to quantified noun phrases. Thus:

(FN 85) Most junior professors, who have heavy teaching loads, are  
overworked.

expresses the secondary proposition that (all) junior professors have heavy teaching loads, rather than that most junior professors have heavy teaching loads. The ability to bind cross-clausally with undistributed binding (via 'junior professors') but not with undistributed binding ('most junior professors'), combined with the placement of the appositional clause under a separate S node, may help explain this reading, although more work is needed to make the explanation complete.

cross-clausally; both complex demonstratives and appositives give rise to conflicted intuitions on truth value when the main claim is true but the description contained in the complex demonstrative (appositional phrase) is inaccurate. Clearly, there is reason to suspect that the same semantic mechanisms are at work in both cases.

My proposal, then, is that a sentence containing a complex demonstrative, such as:

(243) That student of Plato wrote several books.

be seen as the structural analog not of a sentence containing a quantified noun phrase, such as:

(244) Every student of Plato wrote several books.

but of a sentence containing an appositional phrase, such as:

(245) Aristotle, student of Plato, wrote several books.

A false analogy between the 'that' of complex demonstratives and the determiners of quantified noun phrases has led us to treat the two constructions as the same, but in fact they employ radically different syntactic paradigms.<sup>196</sup>

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<sup>196</sup>The false analogy with quantified noun phrases presumably also encourages abandonment of the typical punctuation marks of an appositive, although note that constructions like:

(FN 89) That, student of Aristotle, wrote several books. is acceptable and apparently synonymous with (243) above. ((FN 89) sounds somewhat unusual because we do not typically use simple demonstratives to pick out persons).

A fully adequate story about the syntax of complex demonstratives raises complicated questions. If one takes syntactic facts to be constituted by facts about the psychology of speakers, then it may be hard to deny that complex demonstratives share a syntactic form with quantified noun phrases, since speakers seem to assimilate the two in their understanding of the language. One would then need either to make a distinction between the attributed syntactic structure and the genuine syntactic structure of a string, or provide different syntax/semantics interfaces for quantified NPs and complex demonstratives. Neither route appears entirely satisfactory. If, on the other hand, one takes syntactic facts to be theoretical constructs in the process of imposing a semantic analysis on sentences, then a distinction between complex demonstratives and quantified NPs becomes less problematic, but issues

A sentence with a complex demonstrative of the form:

(246) That F is G.

thus expresses two propositions: the proposition that that is G and the proposition that that is F. It does so by using a multi-headed phrase tree which has one S node linking the NP 'that' with the VP 'is G' and another S node linking that NP with the VP 'is F'. By adopting this position, we are able to explain people's conflicted intuitions on such sentences when the demonstrated object is G but not F, to explain the failure of scope interactions between complex demonstratives and other sentential operators, to explain the pattern of permissible binding into complex demonstratives, and to provide a plausible syntactic model for complex demonstratives which respects their structured form while insisting that the core demonstrative 'that' serves the same function here as it does in the case of simple demonstratives.<sup>197</sup>

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of speaker's competence become more so. I make no attempt here to settle these issues.

<sup>197</sup>I suggested earlier (footnote 62) that wh-phrases in English could be seen as phonetic variants on various demonstratives and pronouns. We would thus have a set of correlations such as:

what ↔ that  
which ↔ this  
where ↔ there  
when ↔ then  
who ↔ thou/he  
whither ↔ thither  
whence ↔ thence  
wherefore ↔ therefore

The wh-morphology would thus indicate a change in the force of the sentence, rather than in the semantic content of the sentence. Whereas the standard th-morphology indicates assertoric force, and thus triggers the implementation of pragmatic mechanisms to complete the semantically presented propositional matrix, the wh-morphology will indicate questioning force. This force will indicate a desire not to express a proposition but to discover the possible (true) completions to the propositional matrix. The open sentence format I propose for sentences containing demonstratives and other singular terms is ideal, given a change in force, for such questioning purposes -- note that standard accounts of the semantics of questions, such as [Higginbotham & May 1981] or [Hintikka 1975], correlate sentences with sets of satisfying sequences in their semantic analyses. 'Which F' and 'What G' questions

#### §2.3.4 Indexicality and Context Sensitivity

Quine, in 'Two Dogmas of Empiricism', employs an argument against understanding analyticity in terms of synonymy construed as interchangeability *salva veritate* in all contexts. Although this argument is *prima facie* seriously flawed and largely ineffective, there is on further reflection an interesting way to defend it. This section is not about Quine or analyticity; we are not concerned (for these purposes) about whether Quine's larger argument stands up. I just want to explore the territory we wander into in defending Quine on this one point.

Quine says that interchangeability *salva veritate* is not a useful test for analyticity because in a purely extensional language:

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could then be analyzed in accordance with the above suggestions on complex demonstratives, with a question like:

(FN 90) Which philosopher wrote *Word and Object*?

being understood as a simultaneous expression of:

(FN 91) Which wrote *Word and Object*? Which is a philosopher?

with the two occurrences of 'which' coindexed. Multiple wh-questions of the form considered by Higginbotham and May, such as:

(FN 92) Which boy read which book?

will also be treatable in the same way, without resorting to the polyadic quantification (or, indeed, any quantification at all) used by Higginbotham and May, although considerable details obviously need to be spelled out here. Note that only 'what' and 'which' support wh-word + N' constructions, on analogy with the behaviour of the th-words.

The identification between wh- and th-phrases, however, is not unproblematic. First, the symmetry between wh- and th-phrases is threatened by the apparently question-forming-only 'why' and 'how', although these constructions might be written off as a false analogy to the wh-pattern (and note the wherefore/therefore pair, which occupies the same semantic position as 'why'). Second, we lack an account for the rather different syntactic distribution of wh-phrases from th-phrases. Wh-phrases prefer prefixed positions, as in:

(FN 93) Where did you park the car?

(FN 94) Whom did the man see?

while th-words prefer lowered positions:

(FN 95) I parked the car there.

(FN 96) The man saw him.

Note, however, that the converse constructions are acceptable, if somewhat strained:

(FN 97) You parked the car where?

(FN 98) The man saw whom?

(FN 99) There I parked the car.

(FN 100) Him the man saw.

Interchangeability *salva veritate* is no assurance of cognitive synonymy of the desired type. That 'bachelor' and 'unmarried man' are interchangeable *salva veritate* in an extensional language assures us of no more than that 'All bachelors are married men' is true. There is no assurance here that the extensional agreement of 'bachelor' and 'unmarried man' rests on meaning rather than merely on accidental matters of fact, as does the extensional agreement of 'creature with a heart' and 'creature with kidneys'. [Quine 1950, 31]

First step: what's wrong with this argument? Well, *prima facie*, if the language under consideration really were extensional, then there could be nothing more to the synonymy of 'bachelor' and 'unmarried man' than that they had the same extension. Is it not characteristic of extensional languages, after all, that their semantics is exhausted by specifying referents for the singular terms and extensions for the predicates? And if two terms agree at every semantic level assigned by the language, what possible bar could there be to taking them as synonymous.

Second step: what's wrong with this objection? Look at how Quine introduces the idea of an extensional language:

Now a language of this type [i.e., having the syntactic resources of first-order logic] is extensional, *in this sense*: any two predicates which agree extensionally (that is, are true of the same objects) are interchangeable *salva veritate*. [Quine 1950, 30 (emphasis added)]

This setup of the problem highlights the often-overlooked fact that there are *two* senses in which a language can be extensional. First, a language might have no operators within it which are sensitive to more than just the extensions of terms within the semantic scope of those operators. Thus languages which contain, e.g., possibility and necessity operators, tense markers, and indexical adverbs like 'now' and 'actually' are not extensional in this sense. Call a language which has

no such operators *functionally extensional (FE)*. Second, a language's expressions (taking the predicate case as paradigmatic) could possess only extensions as semantic values, thus possessing no semantic values which reveal more than just which objects actually satisfy those terms. Call such a language *content extensional (CE)*.<sup>198</sup> A language which, e.g., treated predicates as functions from worlds to extensions would not be CE.

Corresponding to these two ways in which a language can be extensional are two ways in which a language can be non-extensional. A language which did possess non-extensional operators would be functionally non-extensional (FNE). Similarly, a language whose (e.g.) predicates did carry functions from worlds to objects as semantic values would be content non-extensional (CNE). We need not commit to the technical particularities of intensions as semantic values to realize content non-extensionality. Any semantics which gives predicates the desired sensitivity will give rise to CNE.

Ordinarily, FNE and CNE (or, correspondingly, FE and CE) go hand in hand. Thus in traditional implementations of modal logic, we allow predicates to take on intensions -- functions from worlds to extensions (at worlds) -- to make the language a CNE language, and also add the operators ' $\Box$ ' and ' $\Diamond$ ' to act on those intensions, making the language also FNE.<sup>199</sup> However, there is nothing in principle which requires these two components of an intensional language to come together. Clearly there would be a certain oddity in having a language which was FNE but

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<sup>198</sup>If use 'FE' and 'CE' (and also their counterparts 'FNE' and 'CNE') ambiguously as nouns and adjectives in the following discussion.

<sup>199</sup>I use the term 'modal' throughout as shorthand for metaphysical (or, if you prefer, logical) modality.



not CNE -- we would then have operators engaged in a futile search for absent semantic values on which to operate. However, there *seems* to be no conceptual barrier to a language which is CNE but not FNE, a language which possesses semantic values which it lacks the logical tools to exploit.<sup>200</sup>

The focus of this section is on the consequences of having languages which are CNE but not FNE. Before turning to these consequences, however, let us close this section (and our discussion of Quine) by noting that such languages allow us to validate Quine's argument against using intersubstitutability *salva veritate* as a test for analyticity. Consider a language which is extensional only in the sense of being FE. Such a language will then be CNE. In such a language, 'chordate' and 'renate' may well have different intensions, and thus different semantic values. However, since the terms have the same extension, and since there are no operators in the language creating contexts sensitive to anything more than those extensions, they will be everywhere intersubstitutable *salva veritate*. Thus such intersubstitutability cannot suffice for cognitive synonymy.<sup>201</sup>

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<sup>200</sup>Although there may seem no such barrier, we will find below that some philosophers, driven by the Priority Thesis, reject the very possibility of languages which are CNE but not FNE.

<sup>201</sup>The separation of types of extensionality also threatens the success of the compositionality solution to the extensionality problem for Davidsonian semantics. Here the hope is that 'bad' but true T-sentences of the form (e.g.):

(FN 101) 'Grass is green' is true iff snow is white  
will be blocked because the intensionality of the object language will be sufficient to prohibit intersubstitution of non-synonymous expressions in all contexts (it is exactly this substitution which is necessary, on one method, to generate the anomalous T-sentences (there are other methods)). Once one acknowledges that CNE may outrun FNE in a language, one sees that two terms which are not intersubstitutable *salva significatio* may be intersubstitutable *salva veritate* simply because the object language lacks operators of sufficient sensitivity. I think this

#### §2.3.4.1 Semantic Incompleteness

We will shortly explore further the nature of languages which are CNE but not FNE. Before doing so, however, I want to set some groundwork by setting out the concept of semantic incompleteness. Imbedded in many approaches to the philosophy of language is the assumption that the (syntactically) complete sentence and the (semantically) complete proposition go hand in hand:

**(Correlation Principle)** In natural language, syntactic and semantic completeness correlate: any syntactically complete utterance is semantically complete, and any semantically complete utterance is syntactically complete.<sup>202</sup>

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shows that any hopes of forcing interpretive truth theories through the intensional sieve of the object language must be abandoned.

The dual consequences of linguistic incompleteness for Quine and Davidson should not be surprising. On the assumption that a success criterion for a Davidsonian truth theory is that meaning is preserved between object language sentence and metalinguistic interpretation, Davidson is precisely attempting to define synonymy, and to define it using a test of intersubstitutability *salva veritate*.

<sup>202</sup>That there is a correlation between syntactically complete utterances and semantically complete propositions is not to imply that there is a correlation between parts of the first and parts of the second. Thus the Correlation Principle does not imply anything like a 'picture theory of meaning', in which the utterance mirrors the internal structure of the proposition. Moreover, the correlation need not be 1-1, since multiple (syntactically quite distinct) utterances can all express the same proposition.

My focus in this paper is primarily on violations of the Correlation Principle in which syntactically complete utterances fail to express semantically complete propositions. One could also violate the principle by having semantically complete propositions expressed by syntactically incomplete utterances. One plausible example would be in reading the (sub-sentential) answer 'yes' to a question like:

(FN 102) Have you read *Naming and Necessity*?

as expressing the proposition:

(FN 103) I have read *Naming and Necessity*.

My own preference here is to take 'yes' as elliptical for:

(FN 104) Yes, I have read *Naming and Necessity*.

and thus as not on its own expressing a complete proposition. However, I remain open to this general sort of violation of the Correlation Principle.

I find this assumption both poorly motivated and hindering to adequate theorization in some areas. Our eventual goal will be to explore, via the distinction between CNE and FNE languages, one reason for challenging it -- for accepting the possibility of widespread semantic incompleteness in our languages.

One might, of course, hold that the Correlation Principle is analytically true. That is, one might take propositions to be whatever semantic value it is that (syntactically complete) sentences have. But this approach ignores a viable intuition behind the idea of a proposition, that which we express when we say that a proposition is a 'complete thought'.<sup>203</sup> Indeed, the viability of taking the propositional attitudes to be attitudes toward propositions, when coupled with the possibility of non-linguistic believers, seems to depend crucially on the possibility of severing propositionhood and linguistic expression.

Just how significant a departure from the Correlation Principle would be depends on one's views on the nature of propositions. For those who believe that propositions are truth-bearers, a rejection of the Correlation Principle is equivalent to the claim that some utterances lack truth value.<sup>204,205</sup> Few philosophers would be troubled by occasional

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<sup>203</sup>I don't intend to supply here a general criterion for what counts as a proposition. I do hope, however, that rejection of the Correlation Principle will clear the air for productive work toward such a general account -- as I indicate below, I suspect that some have allowed implicit reliance on that principle to mask their need for a story about what makes complete propositions complete.

<sup>204</sup>I focus here, of course, on declarative sentences. Presumably those who take propositions to be truth-bearers will hold that there is, for each nondeclarative sentence, some important semantic relation between that sentence and some declarative sentence which does bear a truth value, and thus that those two sentences express the same, or closely related, propositions.

<sup>205</sup>That propositions are the bearers of truth values is the majority view among philosophers; see (among many others) [Ayers 1946], [Barwise & Etchemendy 1987], [Carnap 1947], [Frege 1956], [Lewis 1986], [Prior & Fine 1977], [Russell 1918], [Salmon 1986], [Salmon & Soames 1989],

such violations of the Correlation Principle: worries about, e.g., vagueness or the semantic paradoxes might motivate a departure from the principle in certain anomalous cases.<sup>206</sup> Widespread violations of the principle, however, would be less acceptable.

Many philosophers, however, hold that propositions need not bear truth values.<sup>207</sup> For such philosophers, violations of the Correlation

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[Soames 1987], [Wittgenstein 1921]. Unfortunately, this position is often developed in less detail than could be desired. Frequently the following two views are left undistinguished: (a) that propositions are *essentially* the bearers of truth-value, that *all* propositions have truth values or (stronger) that the nature of propositions is in some way tied to the notion of truth; and (b) that those things which bear truth values are propositions (but not necessarily that all propositions bear truth values). If one accepts bivalence, the two views collapse into one. (I ignore here the possibility that there are truth values other than 'true' and 'false'.) Those philosophers who understand the claim that propositions are truth-bearers in manner (b) are best assimilated to those philosophers discussed in the next paragraph; all those cited above at least hint that they stand in camp (a).

<sup>206</sup>Such occasional violations would then correspond to sentences which lacked a truth value. For examples of philosophers who claim that some sentences lack truth value, see (e.g.) [Fine 1975] on vagueness, [Kripke 1975] on the semantic paradoxes, [Strawson 1950] on presupposition failure, and [Van Fraassen 1966] on reference failure.

<sup>207</sup>For examples of philosophers holding that meaningful utterances may lack truth values, see [Parsons 1984], [Strawson 1950], [Taylor 1966], and [Van Fraassen 1966]. It is often difficult to determine if philosophers take cases of truth-value gaps to express propositions. Consider, along these lines, Frege's position. He speaks as if sentences express thoughts which are neither true nor false:

The sense of the sentence 'William Tell shot an apple off his son's head' is no more true than is that of the sentence 'William Tell did not shoot an apple off his son's head.' I do not say that this sense is false either, but I characterize it as fictitious. [Frege 1897, 130]

In nearby passages, however, he speaks as if all thoughts must be *complete* thoughts, and thus as if cases of fiction fail to introduce thoughts, but merely act as *if* introducing thoughts:

Assertions in fiction are not to be taken seriously: they are only mock assertions. Even the thoughts are not to be taken seriously as in the sciences: they are only mock thoughts. ... The logician does not have to bother with mock thoughts. ... When we speak of thoughts in what follows we mean thoughts proper, thoughts that are either true or false. [Frege 1897, 130]

Consider also:

The words 'this tree is covered with green leaves' are not sufficient by themselves to constitute the expression of thought, for the time of utterance is involved as well. Without the time-specification thus given we have not a

Principle exemplify something stranger than just a claim which fails to achieve a truth value; we have instead (speaking metaphorically) a propositional matrix missing some structural element crucial for the formation of a complete proposition.<sup>208</sup>

For some, the Correlation Principle is more than a working assumption; it is a way of life. Consider the constraints imposed by the very methodology of the Davidsonian approach to linguistic theorizing. On this approach, roughly speaking, we are (as radical interpreters) to study the object language speakers in their environment and attempt to develop a correlation between utterances by those speakers and assertions that we would tend to make in that situation.<sup>209</sup> Taking the resulting list as the T-sentences governing our theorizing, we then develop a finitely axiomatized theory (possibly meeting other formal constraints) which yields these T-sentences as output. Every syntactically complete utterance, then, gets assigned truth conditions, and these truth conditions are instrumental in deriving the further semantic properties of the language. The pure Davidsonian approach has no place for syntactically complete, semantically incomplete utterances.

Often serving to defend the Correlation Principle is the following widely-held assumption:

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complete thought, i.e., *we have no thought at all*. [Frege 1956, 308. Emphasis added]

I draw heavily on [Evans 1981] for this discussion of Frege's views.

<sup>208</sup>Note that those who hold that complete propositions may fail to achieve a truth value have a special pressure toward accepting the Correlation Principle. For if one gives up the idea that syntactic and semantic completeness go hand in hand, and if one has already abandoned true (or falsity) as the distinctive mark of the complete proposition, then one may well find oneself facing considerable difficulty simply in saying what it is for a proposition to be complete. Of course, those who do cling to truth as a mark of propositionhood may well face the same difficulty once they come to spelling out their notion of truth.

<sup>209</sup>Assuming, via the Principle of Charity, that the object language speakers and we are largely in agreement in our beliefs about the world.

**(Priority Thesis)** The meanings of sentences are in some important sense (ontologically, conceptually, methodologically) prior to the meanings of individual words.<sup>210,211</sup>

If one holds this thesis, the details of the syntax are in principle incapable of posing a threat to the Correlation Principle. Thus, to take a strong case, suppose we were to discover (much to our surprise) that:

(247) is a philosopher.

was a syntactically complete sentence. One might, *prima facie*, suspect that such a sentence would fail to express a complete proposition because it lacked a subject-position noun phrase to specify what is a philosopher. But if one accepts the Priority Thesis, such reasoning puts the cart before the horse. One could then *assume* that (1) expressed a complete proposition, and rework one's (theory-internal) semantic axioms for particular words to reflect this. In our subsequent discussion, the Priority Thesis will often lurk in the background, although at no point do I explicitly challenge it.

I worry that by holding dogmatically to the Correlation Principle we close our eyes to a range of theory-constructing possibilities and thus run the risk of missing valuable philosophical insights residing in

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<sup>210</sup>Thus Davidson:

Words have no function save as they play a role in sentences; their semantic properties are abstracted from the semantic features of sentences. [Davidson 1984a, 221].

The Priority Thesis is generally taken to originate with Frege's Context Principle:

Never ask for the meaning of a word in isolation, but only in the context of a proposition. [Frege 1980, x]

I have, however, refrained here from explicitly identifying the Priority Thesis and the Context Principle to avoid tying the thesis to the particularities of Frege's understanding of the Context Principle.

<sup>211</sup>See §3.4 below (especially §3.4.2.1) for an examination of a price to be paid for maintaining this Priority Thesis.

the resulting blind spot. I will point toward two places in which I feel that our semantic theories have been unduly influenced by the Correlation Principle, but my main purpose here is not to explore the potential benefits of constructing semantic theories open to the possibility of syntactically complete, semantically incomplete utterances.<sup>212</sup> Instead, I want to set out, using the vocabulary developed in the previous section, one interesting route via which the Correlation Principle could come to be violated.

Before returning to that route, however, I want to begin by sketching two simple mechanisms by which a reasonable understanding of language not assuming the Correlation Principle can be obtained.<sup>213</sup> Since the Correlation Principle is so deeply embedded in our thinking about language, it is important to see that it can be coherently and uncontroversially violated. First, pragmatics can take a subpropositional semantic output and use it in the derivation of a propositional speaker's meaning. Second, speakers may fail to *realize* that incomplete propositions are being expressed.

For an example of the first, take a sentence like:

(248) Mary is tall.

On the assumption that one is always tall relative to some comparison class, we *might* assume that (2) fails to express a (complete) proposition because no comparison class is provided. The sentence can, however, give rise to a complete proposition on the level of speaker's

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<sup>212</sup>One can view §2.3.2 above as an extended consideration of the potential benefits of setting aside the Correlation Principle for the particular case of proper names.

<sup>213</sup>Both of these mechanisms are employed in §2.3.2's defense of the claim that proper names act as free variables and thus give rise to semantically incomplete utterances. Here, however, I give considerably less controversial examples of their utility.

meaning as some contextually relevant comparison class is provided.<sup>214</sup>

There's no need, if such a story is available, to provide all the propositional material in the sentence itself.

For an example of the second, consider sentences with empty names, like the infamous 'Vulcan'. One approach to a sentence like:

(249) We almost spotted Vulcan last night.

is to assume that, due to the failure of 'Vulcan' to refer, the sentence does not express a complete proposition, but that, since the audience mistakenly believes that 'Vulcan' does refer, they fail to notice that no complete proposition is in the air.

Neither of these methods for understanding violations of the Correlation Principle, however, provide our central topic in this section (although both will hover in the background from time to time). Instead, I want to motivate and explore a novel approach to such violations -- the notion of linguistic incompleteness. After developing this idea and showing the types of violations of the Correlation Principle it provides, I apply the tools developed by (a) showing that

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<sup>214</sup>One might, of course, build it into the semantic analysis of 'tall'-containing sentences which provide no explicit comparison class either that Mary is tall relative to some comparison class:

(FN 105) 'Mary is tall' is true iff there exists some collection X such that Mary is tall compared to members of X.

or that Mary is tall relative to the speaker's intended comparison class:

(FN 106) 'Mary is tall' is true iff the speaker has in mind some collection X such that Mary is tall compared to members of X.

or, as in [Ludlow 1989], that Mary is tall relative to a comparison class provided by the lexical environment:

(FN 107) 'Mary is tall' is true iff there is some collection X of entities of the same type as Mary and Mary is tall compared to members of X.

All of these approaches are examples of what I will call the Hidden Operator Strategy, on which a quantifier-like operation, not explicitly marked in the syntax, is applied in the semantic evaluation. There seems little reason to make such a move unless one is being driven by the Correlation Principle. I take up the Hidden Operator Strategy in greater detail in §2.3.4.2.2.1 below.



we gain valuable insights into (i) the reasons for natural languages to contain rigid designators and (ii) the semantic means through which rigidity could be achieved, and (b) exploring a puzzle about the semantics of indexicals and suggesting a route for avoiding that puzzle.

#### **§2.3.4.2 Linguistic Incompleteness**

Let us now return to our recently uncovered distinction between FNE and CNE languages. Call a language which is CNE but not FNE an *incomplete* language. Our first task will be to explore the concept of linguistic incompleteness, in order to show (i) that linguistic incompleteness is a possible route to violations of the Correlation Principle, and (ii) that we need to take the idea of linguistic incompleteness seriously when theorizing about natural languages.

##### **§2.3.4.2.1 An Example of Linguistic Incompleteness**

Let me begin by spelling out a scenario to make it clear that the idea of linguistic incompleteness is at least *coherent*. Imagine that, at some point in the distant past of linguistic evolution, speakers use a language *Pretense* in which there are no tenses and no operators, such as 'yesterday', 'in the past', etc., with which to control the time with respect to which one's utterance is to be evaluated. Nonetheless, the speakers of *Pretense* might be aware, perhaps only in a dim sense, that the state of the world changes over time. They have not yet encoded this awareness into the structure of their language, but they are willing to treat a predicate sometimes as being satisfied by objects in its current extension, sometimes in its past extension, and so on.<sup>215</sup> The determination of the proper extension for understanding the speaker

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<sup>215</sup>See the specific examples below for details on what it is for speakers to treat a predicate in these various ways.

will, of course, be a more pragmatic matter than it is in the more structured languages we speak.

Consider a speaker of this language who utters a token of:

(250) A fire burns down your house.

How this utterance will be intended and understood depends on the context of utterance. If the speaker says this while offering a gift to a fellow citizen who recently lost all his possessions in a fire, he may mean, and it will be natural for the audience to understand, his (250) as our:

(251) A fire burned down your house.<sup>216</sup>

Here the speaker brings to bear the past extensions of 'fire', 'burn', and 'house', as provided by the intensional content of these predicates, in constructing the relevant proposition. If, however, the speaker utters (250) frantically while carrying a bucket of water, it will be understood as equivalent to:

(252) A fire is burning down your house.

Or, if the speaker utters (250) in a dire tone of voice while pointing to a pile of laundry next to a stove, he can be understood as making an assertion about the future.<sup>217</sup>

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<sup>216</sup>That is, (a) the speaker will (intend to) express, and the audience will grasp, the proposition expressed by our (251); or (b) the speaker will say (250) in those circumstances in which, and the audience will react to (250) in ways in which, we would use or react to (251).

<sup>217</sup>In the preceding discussion, I speak as if tense were implemented in English by means of temporal operators. [Evans 1985a], of course, has given powerful arguments for doubting that formal tense logic provides the right model for thinking about the temporal aspects of language (and hence, perhaps, also for doubting that tense is a form of intensionality understood as I describe in §2.3.4.2.3.2 below). For those troubled by Evans-type concerns, or those who simply find unrealistic the concept of linguistic speakers so non-introspective about the structure of time as I take the speakers of Pretense to be, I note that a similar example could be constructed around modality. Here we would have a language *Immodality* in which the predicates were sensitive to ways things could

#### §2.3.4.2.2 The Semantics of Pretense

Pretense is intended as a comprehensible example of an incomplete language. Thus the predicates in Pretense carry temporal intensionalities -- they can, if you like, be represented as functions from times to extensions. What, then, are we to say about the meanings of entire sentences in Pretense? My preferred explanation is that we have here a violation of the Correlation Principle. Utterances in this language fail to express a proposition and fail to be either true or false.<sup>218</sup> Think of these assertions as being like Kaplanesque

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have been, but in which there were no modal operators. Immodal speakers could then use a sentence like:

(250) A fire burns down your house.

to convey (through appropriate exploitations of the latent intensionality of 'fire', 'house', and 'burns') the claims:

(FN 108) A fire could have burned down your house.

(FN 109) A fire definitely would have burned down your house.

More interestingly, Immodal speakers could use sentences such as:

(FN 110) If you pile your laundry there, a fire burns down your house.

(FN 111) If you build your house too near the fire pit, a fire burns down your house.

to express the counterfactual conditionals:

(FN 112) Were you to pile your laundry there, a fire would burn down your house.

(FN 113) Were you to build your house too near the fire pit, a fire would burn down your house.

I choose in the main text to rely on the temporal rather than the modal example simply because the requisite situations of use are easier to sketch convincingly.

<sup>218</sup>My thought here is that the expression of a complete proposition by a sentence like:

(250) A fire burns down your house.

requires that the predicates provide extensions to the proposition, so that the proposition can be connected to the world via truth.

Furthermore, I assume that in a CNE language, predicates carry intensions as their sole semantic value, and that it is only via these intensions that extensions are produced. In the absence, then, of explicit instructions on how to exploit these intensions, no extension results and no proposition is expressed.

This model for intensional languages finds an analog in remarks by (e.g.) [Salmon 1989] and [Evans 1985a] on the understanding of present-tense sentences. Thus:

How are we to accommodate the fact that a simple present-tense sentence such as:

(1) I am busy

is capable of achieving truth-value when standing alone as a declarative sentence without an additional temporal

propositions with the relevant properties loaded into place, but without any information about when to evaluate those properties. One can, if one like, think of this language as using sentences to express swaths, rather than points, of logical space.

Since neither sentences nor utterances of Pretense express propositions, there is nothing that speakers say (in Grice's sense).<sup>219</sup> Propositional meaning in a community of Pretense speakers, then, is entirely on the level of speaker's meaning. Speakers of Pretense use the propositional matrices provided by the semantics to express, through pragmatic means, complete propositions in which the intensions of the predicates are exploited, in conjunction with a particular time (or range of times), to provide an extension. Only once an extension has

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operator? On this theory, such uses are regarded as involving an implicit use of a specific, indexical temporal operator such as 'now'. For example, sentence (1) standing alone would be seen as elliptical for (12), represented formally as:

(12) Now(Present Tense[Busy(I)])

... We may call this the ellipsis theory of present tense.  
[Salmon 1989, 385]

Again, the idea here is that sentences whose parts carry intensions as semantic values cannot achieve a truth value (or express a proposition) without the presence of explicit instruction on how to convert that intension to an extension -- here accomplished via a 'now' operator. Should the Salmon strategy prove to be the best way to approach English present tense sentences, we would further hope that that strategy would distinguish itself from the Hidden Operator Strategy mentioned in footnote 213 above by having the 'now' operator syntactically marked via the present tense inflection.

<sup>219</sup>Sentences (types) clearly can express no proposition since they contain no indication of the time with respect to which the predicates are to be evaluated. Even utterances of Pretense (or, if one prefers, sentences of Pretense relative to a context) express no proposition, since such utterances contain no element which is semantically sensitive to contextual features in the way that indexicals are. Thus Pretense sentences are unlike sentences of the form:

(FN 114) I am a philosopher.

which, while expressing no proposition simpliciter, do express a proposition relative to a context and which thus also give rise to something *said* by the speaker. We can, of course, speak of the sentence meaning of Pretense sentences just as we can speak of the sentence meaning of (FN 114). In neither case, however, will that sentence meaning be propositional.

been provided do we have the logical apparatus needed to complete the proposition.

#### **§2.3.4.2.2.1 Competing Explanations of Pretense**

One might well be tempted to interpret Pretense so as to avoid the need either for linguistic incompleteness or for violations of the Correlation Principle. I want to mention two such interpretations and indicate some shortcomings of each.

We might think of all utterances of Pretense as containing a phonetically null wide-scope operator serving to control the intensionality of the predicates. Such an operator could be either (a) a 'now' or 'present-tense' operator, designed to evaluate utterances of Pretense at some default temporal setting (e.g., the present); or (b) a simple existential quantifier over times.<sup>220</sup> Such moves, clearly, are variations on the Hidden Operator Strategy discussed above (see footnote 213).

I find the introduction of a universally hidden, syntactically aberrant operator in this case methodologically ad hoc. If the syntactic structures of Pretense are to have any psychological reality behind them, this operator must reflect some fact about the psychology of the speakers in the example. One should then consider whether the behaviour of the speakers remains comprehensible with the subtraction of that psychological fact.<sup>221</sup>

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<sup>220</sup>I feel, however, that the introduction of such a default setting is inimical to the scenario setup as one in which the speakers have *no* linguistic conventions about the temporal evaluation of predicates.

<sup>221</sup>Furthermore, if this default null operator is a genuine part of the syntax, one ought to expect it to show up in positions other than the wide-scope slot. But were it to do so, readings which ought not be available are predicted by the theory. This multiplicity of scope options is a general problem with the Hidden Operator Strategy as a solution to threats of semantic incompleteness. Thus, for example, if

More generally, one might wonder what explanatory gain is achieved by the introduction of such null operators. Here we see the theoretical fallout of overly slavish adherence to the Correlation Principle. A survey of the literature will show that variations of the Hidden Operator Strategy are rampant. All of [Crimmins & Perry 1989], [Davidson 1967], [Evans 1977], [Heim 1990], [Kamp 1981], [Lewis 1975], [Ludlow 1989], and [Richard 1983], for example, insist (with some minor variations) that some utterances are to be interpreted as the *syntactically unsignalled* existential closure of certain open formulae - an insistence which seems to serve no purpose other than satisfying the Correlation Principle and which thus (a) needlessly clutters the theory and (b) hinders the achievement of a smoothly compositional semantics.<sup>222</sup>

As a second strategy for interpreting Pretense without violation of the Correlation Principle, we might hold that the *semantics* of the language assigns purely extensional concepts to the predicates, and that

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'tall'-containing sentences are taken to contain a phonetically null existential quantifier ('NULL') over comparison classes, then the sentence:

(FN 115) Mary is not tall.

should be interpretable not only as:

(FN 116) NULL(-Mary is tall-for-an-x)

but also as:

(FN 117) -NULL(Mary is tall-for-an-x)

This last is equivalent to:

(FN 118) Mary is not tall compared to anything.

which (a) is logically false and (b) seems to sit ill with intuitive judgement. An adequate implementation of a Hidden Operator Strategy will thus, in general, need to include the motivation of adequate constraints on the syntax blocking the undesired scope possibilities.

<sup>222</sup>In some cases, the type of quantificational closure is not simply existential, but the general point runs through all cases: some sort of semantic operation, not signaled by the syntax, is added to create complete propositions. [Ludlow 1989] runs a more sophisticated version of the Hidden Operator Strategy, taking his closure operator to be part of the syntax but phonetically null. Here we have at least the possibility that a mature syntactic theory will provide methods for determining where phonetically null syntactic elements reside.

speakers then through *pragmatic* devices bring to bear intensional concepts which enable the richer interpretations I sketch above. But consider how such a pragmatic mechanism would work. Take an utterance of:

(253) John snores.

We must assume that the speakers have an extensional concept of snoring, which is the semantic value of the predicate 'snores'. They must also have an intensional, time-sensitive concept of snoring, which they bring to bear in their pragmatic understanding of (253). Moreover, they must recognize that there is a conceptual link between the extensional and the intensional concepts of snoring -- that one is an enriched version of the other. Once they have all of this apparatus at their disposal, though, why would they not move to taking the intensional concept of snoring, which they must recognize as more fully adequate for their communicative purposes, as the semantic value of their predicates? The predicate is in place already; all that is necessary is for speakers of Pretense to treat it as carrying the intensional concept as its meaning. The maneuvering room for having those speakers always treat the predicate as carrying this concept pragmatically but not semantically seems narrow at best.<sup>223</sup>

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<sup>223</sup>If the behaviour and cognitive resources of speakers of Pretense are sufficient to show that they will take their predicates as carrying an intensional concept as semantic value, why (in a parallel manner) are those behaviour and resources not adequate to show that they take their sentences as possessing intensional operators? Such a parallel argument would then undermine my proposed preferred semantics for Pretense and instead make it a language both CNE and FNE. However, there remains a crucial disanalogy between the CNE (predicate) and FNE (operator) cases: the predicates already exist in the syntax, and need only have their semantic values altered to reflect the growing temporal awareness of Pretense speakers, whereas there simply are no temporal operators in the syntax to inherit the appropriate semantic values from that awareness. It's easier to modify the (perhaps already nebulous) meaning of a preexisting lexical item than it is to alter the syntactic rules of the

#### **§2.3.4.2.3 Natural Language and Semantic Incompleteness**

Pretense is an example of an incomplete language. Such a language has resources of meaning which lie beyond the control of the meaning-governing structure of the language. We may now ask the following question: are real natural languages, as opposed to the fairy tale discussed above, linguistically incomplete? Before attempting to answer this question, however, I want to distinguish between two forms of linguistic incompleteness.

##### **§2.3.4.2.3.1 Strong and Weak Linguistic Incompleteness**

Return to our speakers of Pretense, the tenseless but CNE language. Assume that these speakers, becoming more aware of the temporal sensitivity of their predicates, start adding some operators to their language to exploit and control that sensitivity. Let's say they add the operators 'formerly', 'currently', and 'forthcomingly', with the obvious meanings. Their language is then also FNE and thus is no longer incomplete (as we have defined incompleteness), but there is still an important sense in which it is deficient. Although they can now control whether an utterance of:

(250) A fire burns down your house.

refers to a past, present, or future fire, they still cannot control whether a fire *next week* or a fire *next month* is at issue. The temporal

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language in order to introduce new lexical items. Indeed, if one takes the meaning of a term to be the richest realization of the speaker's cognitive resources compatible with the logical positioning of that term in the syntactic environment, then the switch of predicates from extension to intension will happen automatically as the speakers begin to think in temporal terms. The syntax, however, cannot update itself automatically in this manner, so explicit temporal operators will tend to lag behind temporal intensionalities in the predicates. In the end, Pretense simply doesn't contain temporal operators, while it does have predicates.



sensitivity of the predicates is more fine-grained than the three-fold distinction imposed by the operators.<sup>224</sup> Not until the speakers of Pretense develop a scheme for naming instants of time<sup>225</sup> -- which itself requires a realization that time is linear -- and add a class of operators of the form 'at time t' for all such names t will they have complete control over the temporal intensionality of their language.

I thus want to distinguish two senses of incompleteness in a language, strong and weak:

**(Strong Incompleteness)** A language L is strongly incomplete if predicates in that language are sensitive to some dimension of intensionality for which there are no operators in the language.

**(Weak Incompleteness)** A language L is weakly incomplete if it has some dimension of intensionality such that the operators in that language which affect that dimension of intensionality are insufficient to specify precisely the indices at which predicates are evaluated in that dimension.

There are two types of weak incompleteness which will be of particular interest to us. One is the lack of a 'homing' operator, which causes the predicates to be evaluated at the privileged index for a particular intensionality, should there be one. Both 'now', for tense, and 'actually', for modality, are homing operators. The second is the lack of a 'totalizing' operator, which causes the predicates to be evaluated at every index of a particular intensionality. 'Always' and

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<sup>224</sup>Actually, iterations of the operators can create more than three temporal distinctions, but the result is still inadequate for complete temporal mastery.

<sup>225</sup>Or intervals of time, whichever is actually metaphysically primary.

'necessarily' are, respectively, temporal and modal totalizing operators.

### **§2.3.4.2.3.3 What is an Intensionality?**

My definitions of strong and weak incompleteness appeal to dimensions and indices of intensionality. To cite some examples, modality and tense are dimensions of intensionality to which our predicates are sensitive, with worlds and times (respectively) being their indices of evaluation. Clearly at this point it would be desirable to give some *general account* of what a (dimension of) intensionality is. Broadly speaking, take an intensionality to be a mode of sensitivity in the way our predicates serve to distinguish among objects. We know that what objects our predicates distinguish depends on the time and way things are we are considering, and *prima facie* it is reasonable to suppose that there might be other such sensitivities (either in our languages or in possible languages).

Consider, for example, the non-extensional deontic operator 'it ought to be the case that'. By considering sentences like:

(254) It ought to be the case that the rich paid the majority of the taxes.

we see that the predicate 'paid' is picking out different pairs of objects than it does in extensional contexts. Thus the predicate, in addition to having the capacity to single out things it used to, will, or could apply to, also has the ability to single out things it ought to apply to.<sup>226</sup> What the indices of deontic intensionality (or, more

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<sup>226</sup>Note that it's *prima facie* possible that the predicate ought to apply to objects which it is metaphysically necessary that it not apply to.

generally, whether deontic intensionality *has* indices or even represents a single dimension of intensionality) I leave an open question.

Because intensional operators (in the sense of intensionality under consideration here) cause predicates to alter their object-distinguishing potential, such operators will in general be non-extensional, in the sense that they will not support substitution of coextensive predicates or of materially equivalent sentences *salva veritate*. However, it does not follow that all non-extensional operators mark the presence of intensionalities. Quotational contexts, for example, are not intensional ones, since they do not highlight anything about the way in which predicates distinguish objects; rather, they remove the object-distinguishing power of predicates. Other cases, such as causal or psychological attitude contexts, are less clear. We simply know too little about such contexts to know whether they exploit a sensitivity of the contained predicates or operate through some unrelated mechanism.<sup>227, 228</sup>

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<sup>227</sup>If my later remarks on the function of rigid designators in language are on the right track, then it ought to be characteristic of intensional (as opposed to more generally non-extensional) contexts that they support the substitution of coreferring singular terms *salva veritate*. If, contra the direct reference theorists, we take attitude contexts not to support such substitutions, then we have *prima facie* reason to doubt that such contexts are intensional ones. Causal contexts, of course, do allow the necessary substitution, and are thus still a viable candidate to be intensional.

Causal contexts introduce further complications due to difficulty in determining (a) whether such contexts do affect the behaviour of predicates and (b) whether we can even impose a coherent logic on such contexts. Clearly causal contexts do not allow free intersubstitution of materially equivalent sentences *salva veritate* (in the vocabulary of [Neale 1995], they are not +PSME). For the following will not generally have the same truth value:

(FN 119) That Perot ran in 1992 caused it to be the case that Clinton was elected president.

(FN 120) That arithmetic is incomplete caused it to be the case that Clinton was elected president.

However, it is at least *plausible* that codenoting definite descriptions can be intersubstituted freely in such contexts (i.e., that causal contexts are +I-SUBS). Thus:

### §2.3.4.2.3.3 The Threat of Linguistic Incompleteness

I suspect that English as we now speak it is *strongly* incomplete, but for obvious reasons it's hard to provide examples of this incompleteness. However, if one considers a language as an evolving mechanism, one shouldn't expect that the realization that predicates are sensitive to certain types of variation (in time, possibility, mode of representation, etc.) will coincide with the introduction of explicit linguistic tools to deal with that type of variation. People could have

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(FN 121) That the red wire shorted out caused it to be the case that the house burned down.

(FN 122) That the object exactly 2000 feet due east of the northeast corner of the Eiffel Tower shorted out caused it to be the case that the house burned down.

It shouldn't matter here, the intuition runs, *how* we pick out the object, so long as we do single out the causally efficacious object. There are, unfortunately, difficulties here in correctly introspecting scope properties of the definite descriptions involved (see [Neale 1995], [Neale & Dever 1997] for further discussion). But if causal contexts are +*t*-SUBS, then on minimal additional assumptions (e.g., that such contexts allow, *salva veritate*, exchange of logical equivalents (are +PSLE) or of Gödelian equivalents (are +*t*-CONV)) we will be forced to conclude that those causal contexts are also truth functional, despite the earlier evidence of (FN 119), (FN 120) to the contrary.

<sup>228</sup>My approach to intensionality here takes its inspiration from the assumption that there is some notion of intensionality common to tense and modality, a notion which then admits of generalization into other areas. Should we become convinced (by, for example, the arguments of [Evans 1985a]) that it is a mistake to construe tense along the lines of modal logic, the motivation for my generalized view of intensionality would diminish.

We might, then, conclude that the sole example of intensionality is modal intensionality (although we would still have the deontic constructions to deal with). Should this occur, then the strength of my subsequent arguments will clearly be considerably vitiated -- although not, I think, dispelled entirely. First, the question still remains whether we have convincing evidence for believing that our language currently analyzes (all of) its modal operators in the correct way. Second, the general strategy I pursue here can be extended with some creativity to other areas of the logic of language. The general point is that the creation of complete propositions often requires contributions -- and frequently contributions whose appropriate logical shape is by no means self-evident -- from multiple points in the syntax. Whenever such a semantic situation is found, we can ask whether it is conceivable that languages might develop failing to realize in their syntax the demand that all of these parts be supplied. Any language which *does* develop in this way will exhibit a phenomenon suitably similar to that which I have called linguistic incompleteness.

been (perhaps dimly) aware for some time that they were using language to describe not just how the world actually was but also how it could have been before introducing a new mood (the subjunctive) to mark this particular use of language. People's intuitions about the nature of the intensionality may in fact be too vague to introduce explicit operators into the language.

These evolutionary considerations give good reason to think that natural languages *could be* strongly incomplete. To give some reason to think that natural languages are strongly incomplete, consider the following controversial example. *Vagueness* might be seen as a dimension of intensionality of predicates. If it is, it is certainly one which is ill understood. We have some attempts at operators to deal with the intensional factor of vagueness. The vague claim:

(255) Frederick is bald.

can be influenced in its choice of extension for 'bald' by adding terms like:

(256) Frederick is definitely bald.

(257) Frederick is barely bald.

Some, however, have felt that there is more than one degree of intensionality at work in the general phenomenon of vagueness, and point to the diversity of readings available in sentences like the following:<sup>229</sup>

(258) Technically, Richard Nixon is a Quaker.

(259) Esther Williams is a regular fish.

(260) Strictly speaking, the tomato is a fruit.

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<sup>229</sup>These examples are taken from [Lakoff 1972] and [McCawley 1993].

(261) Loosely speaking, whales are fish.

If this is right (and I certainly don't want to commit to it being so), there is good reason to suspect that the dimensions of vagueness may outstrip the array of operators for indicating vagueness we have available. This is simply an area in which we (as a linguistic community, not as a group of philosophers -- although the second may influence the first) haven't thought through our semantics in great detail. So our language may well be strongly incomplete with respect to vagueness, and is almost certainly weakly incomplete.

It may require a certain lack of linguistic introspectiveness to fail completely to recognize a certain brand of intensionality in one's predicates, but the mere recognition of the intensionality is a far cry from the introduction of a complete set of mechanisms for controlling it. In order to eliminate *weak* incompleteness, one would need a (scientific or philosophical) *theory* of the structure of the intensionality. Such theories are not easy to come by. Weak temporal incompleteness can only be eliminated, for example, by deciding whether time is ultimately composed of instants or of intervals, and by replacing the current 'formerly' and 'forthcomingly', which presuppose a linear structure to time, with a new set of temporal operators which mirror the more complex structure posited by special relativity by comparing times only relative to an inertial frame.<sup>230</sup>

The search for the necessary set of tools for eliminating weak modal incompleteness drives a robust recent literature on the

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<sup>230</sup>Assuming, of course, that the temporal intensionality of our predicates is a sensitivity to how things change over time *as time really is*, not as time is according to our flawed folk physics. Were the intensionality of the latter sort, we would be doomed to widespread falsehood.

comparative expressive powers of standard modal logics, modal logics with explicit quantification over worlds, modal logics with actuality and Vlach operators, modal logics with possibilist and actualist quantifiers, and so on.<sup>231</sup> While the average speaker is presumably aware of the use of the subjunctive to speak of alternative representations of the world, it is clearly unreasonable to suppose that we must have performed the kind of detailed philosophical analysis needed to ground possible worlds talk and settle the controversies alluded to above *before* we allow modal talk into the language. How long might it have been to sufficiently separate epistemic and metaphysical necessity so that we could introduce semantic markers for each?<sup>232</sup>

Eliminating weak incompleteness from a language, then, requires in general the completion of various scientific and philosophical research projects. It cannot plausibly be introduced as a constraint on natural language that it await such completion before developing its semantic resources (the two projects are likely to go hand-in-hand). Even if the science and philosophy has already been done, the completeness of the language is always dependent on the (necessarily uncertain) correctness of the consequent results. Thus natural languages are almost certainly weakly incomplete, and quite certainly *at risk* of being weakly incomplete.

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<sup>231</sup>See [Forbes 1989] for a summary of these debates.

<sup>232</sup>If we think of predicates as something like pointers to properties, they will thereby inherit all the variability to which those properties are subject in their instantiation, regardless of whether speakers have any awareness of those areas of variability. Thus the content non-extensionality of a language could quite easily far outstrip the cognitive resources of the speakers of the language.

### **§2.3.4.3 Rigidity**

I now want to move to considering the philosophical applications of the notion of linguistic incompleteness. While the possibility of incompleteness may be of some abstract interest, it's not immediately obvious why we should care if our language is either strongly or weakly incomplete. Of course, there will occasionally be some imprecision in our talk when we want to make very precise claims in the areas where incompleteness is most rampant, but that just means that philosophers will stay employed. Is there any reason to think that the central purposes of language are endangered by incompleteness?

#### **§2.3.4.3.1 Linguistic Incompleteness and the Stability of Reference**

A number of philosophers have explored the idea that consideration of the evolutionary constraints on the development of natural languages can give rise to interesting and substantial conclusions about the types of semantic devices likely to arise in those languages.<sup>233</sup> This line of thought holds that our need to talk about the same objects over time, combined with our lack of perfect knowledge about the current, future, and possible properties of those objects, gives rise to the need for a class of singular terms which refer to those objects directly, without the intervention of a descriptive sense -- enabling us to discuss them without knowing how they are.

The possibility of linguistic incompleteness, I think, provides another argument for the need for singular terms. Even if we had perfect knowledge about the object we wanted to discuss, we now see, we could

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<sup>233</sup>See (e.g.) [Strawson 1959, 1974], [Evans 1973], [Peacocke 1975], [Føllesdal 1986], and [Neale 1993].



not use predicative language to *guarantee* continuity of reference. If I say:

(262) The man in the corner drinking water is a bad philosopher.

and you say:

(263) The man in the corner drinking water is a good philosopher.

there is no guarantee that we are actually disagreeing, because our two uses of the phrase 'the man in the corner drinking water' might not pick out the same individual, by virtue of exploiting different aspects of the (uncontrolled) intensionality of the predicates in question.<sup>234</sup> To avoid such uncontrolled shifts, we introduce proper names, terms with no latent intensionality to be abused.

Not all objects have names, of course, and we often want to talk about the same object or objects repeatedly without introducing a new name for them. We thus also need a system of anaphora and pronominal expression which will enable us to track objects throughout a dialogue, as in:

(264) The man who proved the completeness of modal logic by  
introducing possible worlds helped us understand it.

(265) No, he just opened the door for gratuitously baroque  
metaphysics.

These pronominal devices will, just like their proper name analogs, need to be stable in their reference -- referring directly, rather than by means of predicative material which could be infelicitously affected by linguistic incompleteness.<sup>235</sup>

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<sup>234</sup>Linguistic incompleteness thus gives us another reason to adhere to the Fidelity Principle introduced in §2.2.2.2.3 above.

<sup>235</sup>Note that for exactly this reason, an account of cross-clausal anaphora such as that of [Neale 1990], which analyzes the pronouns in

Of course, pragmatic considerations may often suffice to allow us to converge on our assignments of evaluative indices for the predicates, even in the absence of explicit linguistic markers. But pragmatic determinations are notoriously undependable, especially if we are discussing somewhat obscure entities in the presence of somewhat obscure intensionalities. In some contexts -- doing theoretical science or (worse) philosophy -- the chance for error might become quite great. Speakers may thus face shifts in denotation of descriptive phrases in situations where they least expect it.

If the intensionalities are subtle enough, even the most reflective speakers may risk miscommunication and even false reasoning (if deductions in which they engage presuppose a continuity of reference which does not obtain).<sup>236</sup> It would be much more convenient here if we had some linguistic mechanism which guaranteed that we were talking about the same entities (in the sense of semantic reference rather than speaker's reference, of course. It's not clear how *anything* could *guarantee* that we both had the same entities in mind). That mechanism is the mechanism of the singular term.

#### **§2.3.4.3.2 Two Aspects of Rigidity**

We have seen that linguistic incompleteness highlights the need for stable singular terms in a language, terms whose reference is unaffected by the intensional machinery surrounding them. This stability, of

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terms of definite descriptions, will be inadequate in incomplete languages. In, for example,

(FN 123) Just one man drank rum last night. He was ill later. the 'he' has a stability of reference which the proposed analysis 'the man who drank rum last night' does not, due to the instability inherent in predicative phrases in an incomplete language.

<sup>236</sup>See [Lewis 1982] for a similar observation.

course, is a consequence of what Kripke has called 'rigidity'.<sup>237</sup> The route we have taken to rigid designators, however, brings out two important aspects of rigidity which are not always attended to in the literature:<sup>238</sup>

**(Universal Rigidity)** Rigid designators refer to the same object when embedded in any intensional context, not just when embedded in modal contexts.

**(Transparency of Rigidity)** Not only do rigid designators refer rigidly, but we know that they do, and we know that they will continue to do so (a) no matter what new intensional machinery is added to the language and (b) no matter what we discover about the language's existing intensional operators.<sup>239</sup>

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<sup>237</sup>Kripke originally defines rigidity as follows:

Let's call something a rigid designator if in every possible world it designates the same thing. [Kripke 1980, 48]

Later, in the 1980 preface to *Naming and Necessity*, he makes essentially the same point without appeal to possible worlds:

A proper understanding of [(1) Aristotle was fond of dogs] involves an understanding both of the (extensionally correct) conditions under which it is in fact true, and of the conditions under which a counterfactual course of history, resembling the actual course in some respects but not in others, would be correctly (partially) described by (1). Presumably everyone agrees that there is a certain man -- the philosopher we call 'Aristotle' -- such that, as a matter of fact, (1) is true if and only if he was fond of dogs. The thesis of rigid designation is simply -- subtle points aside -- that the same paradigm applies to the truth conditions of (1) as it describes counterfactual truth conditions. [Kripke 1980, 16]

<sup>238</sup>Universal rigidity is explicitly endorsed by [Neale 1993], and is implicit in the direct reference theorist's position that rigid designators carry a referent as their sole semantic value and thus have no way to be sensitive to intensional contexts.

<sup>239</sup>While this formulation of the transparency of rigidity conveys the core idea succinctly, it is certainly false as it stands. Most people lack the concept of rigidity, and thus do not even know that their terms refer rigidly, let alone that those terms would continue to do so no matter what about the intensional structure of the language was altered or discovered. An ideal statement of the transparency of rigidity would involve settling difficult issues regarding the epistemic status of

Note that universal rigidity is a claim about the semantics of the language, while the transparency of rigidity is a claim about our epistemic relation to that semantics. Universal rigidity tells us that 'Aristotle' picks out Aristotle not only when considering how things might have been, but also (e.g.) how things were or will be. The transparency of rigidity tells us that we need not fear that 'Aristotle' will suddenly start shifting its reference as we expand the resources of our language. Both of these characteristics of rigidity are necessary if rigid designators are to play the stabilizing role described above.

#### **§2.3.4.3.3 Dummett, Kripke, and Rigidity**

There has been some small disagreement about how the rigid designator achieves the stability of reference that it has. While Kripke has introduced rigidity as a primitive property of singular terms (perhaps to be explained as a consequence of the lack of Fregean sense of such terms)<sup>240</sup>, Dummett has claimed that singular terms are to be understood

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semantic principles for speakers. However, the weaker claims that people (a) would not deny the transparency of rigidity, (b) act as if they knew what the transparency of rigidity claims they know, and (c) would be surprised should they come across a newly non-rigid behaviour of a previously rigid term suffice for our purposes.

<sup>240</sup>Kripke makes no explicit pronouncements on why proper names are rigid. Consider, however, the following features of the 'descriptive theory of names' which he rejects:

- (1) To every name or designating expression 'X' there corresponds a cluster of properties, namely the family of properties  $\phi$  such that A believes ' $\phi X$ '.
- (3) If most, or a weighted most, of the  $\phi$ 's are satisfied by one unique object  $\gamma$ , then  $\gamma$  is the referent of X.
- (5) The statement, 'If X exists, then X has most of the  $\phi$ 's' is known a priori by the speaker.
- (6) The statement 'If X exists, then X has most of the  $\phi$ 's' expresses a necessary truth (in the idiolect of the speaker). [Kripke 1980, 71]

What Kripke rejects here is that there is a 'sense' which determines one's epistemic relation to a claim ((5)) and which is reference-determining ((3)) but which also allows the term to shift reference in a modal context ((6)). His insistence that there be no such shift, then, may follow on his rejection of this conception of sense.

as referring in virtue of a descriptive sense, and that the accompanying rigidity of those singular terms is to be explained through a linguistic convention which specifies that singular terms always assume wide scope with respect to modal operators in their linguistic context:

Kripke's doctrine that proper names are rigid designators and definite descriptions non-rigid ones thus provides a mechanism which both has the same effect as scope distinctions and must be explained in terms of them. We could get the same effect by viewing proper names, in natural language, as subject to a convention that they always have wide scope ... Such an explanation would not demonstrate the non-equivalence of a proper name with a definite description in any very strong sense: it would simply show that they behaved differently with respect to ad hoc conventions employed by us for determining scope.  
[Dummett 1973, 128]

Dummett's claim, then, is that a name like 'St. Anne' has associated with it a description like 'the mother of Mary', and it is the tendency of that description to take wide scope in modal contexts which allow us to read:

(266) st. Anne might have had no children.

truly as:

(266-RQ) [the x: mother-of-Mary x]  $\diamond$  (has-no-children x)

A language which is strongly modally incomplete cannot explain the rigidity of its expressions using a Dummettian explanation, since there would be in such languages *no modal operators* for the descriptive sense of the proper name to take wide scope over.<sup>241</sup> Even a weakly modally incomplete language, provided that it is weakly incomplete in the sense of lacking a totalizing operator, would be unable to capture the full

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<sup>241</sup>Of course, a rigid designator would also be equally incapable of (explicitly) showing its rigidity in such a language, since would could never observe it referring to the same object as it was embedded in modal contexts. But its inability to provide this demonstration would not change the fact that it was rigid. See the discussion of Kripke's response to Dummett below for more details on this point.

force of rigidity, since it would lack the resources to guarantee that the singular term adopt the same reference in all possible situations (it could provide such a guarantee only over those possibilities within the range of the modal operators provided by the language).

English, of course, does have a totalizing modal operator ('necessarily'). But Dummett still cannot use his scope-convention explanation to capture our two highlighted features of rigidity: (i) singular terms are rigid with respect to all dimensions of intensionality and (ii) singular terms are *transparently* rigid -- were Dummett's explanation the right one, there would remain a worry that the rigidity of our singular terms *depended* on the functional resources of our language.

The above is, of course, another way of making the very point that Kripke has made in response to Dummett by pointing out that in a sentence like:

(172) Aristotle was fond of dogs.

which contains no modal operators, we can still evoke the rigidity of the name 'Aristotle' by considering the counterfactual situations under which (172) would be true:

The doctrine of rigidity supposes that a painting or picture purporting to represent a situation correctly described by [(172)] must ipso facto purport to describe Aristotle himself as fond of dogs. ... The intuition is about the truth conditions, in counterfactual situations, of (the proposition expressed by) a *simple* sentence. No wide-scope interpretation of certain modal contexts can take its place. [Kripke 1980, 12]

No such interpretation can take its place simply because, when considering the modal behaviour of a simple sentence, there is *no modal operator* to give rise to the desired wide-scope reading. Kripke's

recognition of our ability to consider the modal behaviour of a sentence without modal operators is itself implicitly a recognition of the possibility of linguistic incompleteness.

Dummett claims to have a response to this argument of Kripke's. Dummett distinguishes between the content sense of a sentence -- the meaning it expresses -- and the ingredient sense of that sentence, a mere theoretical construct which is important only in so far as it accounts for how sentences behave semantically when imbedded in further contexts.<sup>242</sup> He then holds that Kripke is appealing here not to the *content* sense of the sentence (172) but to the *ingredient* sense of (172), and that when (172) is imbedded in a modal context, we can there use a wide-scope convention for proper names (a convention which is inert in simple sentences like (172)) to get the right content sense and account for the 'rigidity' of (172).<sup>243</sup> Since a semantic theory is taken to be responsible only to the content senses, the difference between

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<sup>242</sup>Thus:

we must distinguish, as we have seen, between knowing the meaning of a statement in the sense of grasping the content of an assertion of it, and in the sense of knowing the contribution it makes to determining the content of a complex statement in which it is a constituent: let us refer to the former as simply knowing the *content* of the statement, and to the latter as knowing its *ingredient* sense. [Dummett 1973, 446-447]

<sup>243</sup>On the first point, see (e.g.):

Kripke fails to ask for what purpose we need to consider the truth-value of a sentence with respect to a counterfactual situation. The answer is that we need the notion only in order to explain the contribution of that sentence to the content of more complex sentences of which it is a constituent; it serves, on a particular type of semantic theory, to explain the ingredient sense of the sentence. [Dummett 1981, 582]

The second point follows trivially from Dummett's idea that proper names act like descriptions taking wide scope in all modal contexts.

Kripke and Dummett on the ingredient senses provides no grounds for choosing one approach over the other.<sup>244</sup>

Dummett's reason for holding that Kripke is appealing to a theory-internal construct -- an 'ingredient sense' -- rather than a piece of data to which a semantic theory is responsible is that grasp of the counterfactual truth conditions of (172) cannot manifest itself in behaviour beyond grasp of the behaviour of (172) when imbedded in modal contexts. Consider any piece of behaviour in which speakers engage, which would count as a realization that the truth of (172) would depend (in a situation in which Aristotle had not been the last great philosopher of antiquity) not on whoever was the last great philosopher of antiquity but on Aristotle himself. Any such behaviour, Dummett argues, would just be a recognition of the (actual) truth conditions of:

(267) Had Aristotle not been the last great philosopher of antiquity, he would have been fond of dogs.<sup>245</sup>

Lying behind this method of argument is a principle that all semantic facts to which a semantic theory is to be held responsible must be manifest in the behaviour of speakers.

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<sup>244</sup>On the priority of content over ingredient sense as data for a semantic theory, see (e.g.):

The notion of truth-value with respect to a possible world is a technical one, which may or may not admit of a coherent explanation., but belongs to semantic theory rather than to that understanding of our own language which is the datum for such theory. The same holds good for modal status.  
[Dummett 1981, 582]

<sup>245</sup>See Dummett's claim that:

One who has a language containing modal expressions manifests his grade-two understanding of a given statement by his assessment of statements involving such expressions.  
[Dummett 1981, 571]

Dummett's views on the grasp of ingredient sense ('grade-two understanding') by speakers of non-modal languages are discussed below.



Regardless of the correctness of this manifestation principle, however, it does not support the conclusion that Dummett wants. I showed earlier, in the discussion of our hypothetical tenseless but CNE language Pretense, that there are obvious ways in which speakers could manifest in behaviour their understanding of the way in which the truth conditions of utterances depended on the time with respect to which those utterances are to be evaluated, *even if* they have no linguistic contexts to use in stating that understanding. Dummett's assumption that speaker's semantic understanding is exhausted by what he calls content sense is based on an assumption that functional and contentual intensionality always go hand in hand; that there is never even weak incompleteness in our languages -- surely not a tenable assumption.

Dummett makes this assumption explicit, claiming:

Someone who has a language which lacks subjunctive conditionals and modal operators cannot express [judgements concerning counterfactual conditionals and other modal statements], and may be supposed incapable of the thought of counterfactual courses of history. [Dummett 1981, 571]

Once we see that linguistic incompleteness is a real possibility, however, Dummett's reliance on content sense as the sole empirical touchpoint for a linguistic theory crumbles.<sup>246</sup>

#### **§2.3.4.4 Deictics**

Finally, I want to consider the implications of linguistic incompleteness for our understanding of the semantics of context-sensitive terms such as indexicals and demonstratives (I will use

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<sup>246</sup>Lying behind Dummett's preference for content over ingredient sense is his acceptance of the Priority Thesis. Taking the meanings of entire sentences as primary, and -- via further acceptance of the Correlation Principle -- taking those meanings to be propositional in nature, he is able to relegate the non-propositional ingredient sense possessed by individual words to theory-internal status.

'deictics' as an umbrella term for context-sensitive syntactically simple noun phrases).<sup>247</sup> In doing so, I will focus on what I take to be two relatively uncontroversial features of such terms:<sup>248</sup>

(A) The referents of deictics depend on the context in which such terms are used, and it is part (indeed, the central part) of the meaning of these terms that there is a particular rule which determines what referent is assumed in a given context.<sup>249</sup> Thus:

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<sup>247</sup>Alternatively, one can take deictics to be those context-sensitive noun phrases which contain no predicative components. Complex demonstratives, which I take to be deictics subject to the comments made here, are an apparent exception to this definition. Complex demonstratives are discussed further in §2.3.3.2 above. I leave as an open question how much of what I say in this section can be transferred over to context-sensitive lexical items in general.

<sup>248</sup>These two features have obvious connections with Kaplan's 'two obvious principles':

Principle 1: The referent of a pure indexical depends on the context, and the referent of a demonstrative depends on the associated demonstration.

Principle 2: Indexicals, pure and demonstrative alike, are directly referential. [Kaplan 1977, 492]

Principle 2, some subtle points aside, corresponds exactly to my feature (B); my feature (A) is a strengthening of Principle 1 to the effect that the dependencies there cited are semantic in origin. Setting aside Kaplan's subtle worries, I will use 'rigid' and 'directly referential' interchangeably in the subsequent discussion.

<sup>249</sup>In Kaplanesque terms, this rule provides the *character* of the term. Although direct reference theorists like Kaplan take the contribution of a term to the proposition expressed to be the content of that term -- in the case of deictics, their referent-in-a-context -- they will nonetheless agree that it is a semantic fact about a term that it has the character that it does. While Kaplan inveighs against holding that a deictic term is synonymous with the description which fixes its referent, he does hold that 'the meaning of a word or phrase is what I have called its character.' [Kaplan 1977, 521] Similarly, Salmon introduces character:

Indexical expressions ... generate a higher-level non-relativized *semantic value* ... which David Kaplan calls the character of the expression. [Salmon 1986, 14 (emphasis added)]

Since I am concerned here only with directly referential terms, I overlook Salmon's distinction between *character* and *contour*.

(i) It is part of the meaning of the word 'I' that it refers, in a context, to the producer of the word 'I'.<sup>250</sup>

(ii) It is part of the meaning of the word 'that' that it refers, in a context, to the object demonstrated by the producer of the word 'that'.

(B) Deictics are rigid referring expressions.<sup>251</sup> Thus when I embed the word 'I' in the scope of any sort of intensional

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<sup>250</sup>I use 'producer' here as a generalization of 'speaker' meant to accommodate those utterances (broadly construed) not in spoken form.

<sup>251</sup>Feature (B) may seem more controversial to those impressed by recent work on supposed 'attributive' readings of deictics (see, e.g., [Nunberg 1993] and [Récanati 1993]). On such readings, certain deictics are to be interpreted as definite descriptions or other quantificational noun phrases, and thus are not rigid in behaviour. I am skeptical of the purported attributive readings, and am inclined to think that a number of distinct issues are being run together in discussions of such readings. Considerations of length prohibit a full discussion of these issues here, but briefly:

First, attempts to show that deictics in extensional contexts often pick out objects other than those distinguished by the standard semantic rules strike me as gratuitously reading pragmatic phenomena into the semantics. Thus examples like:

(FN 124) I'm in the alley behind the restaurant.

used to give the location of the speaker's car, I am inclined to see as examples of metonymy along the lines of:

(FN 125) The White House today released a statement about Iran-Contra.

We might also understand (FN 124) as being asserted within the context of a game in which people identify themselves with their automobiles, as in an utterance in the context of a Monopoly game of:

(FN 126) I'm the hat.

Such metonymy and games should be explained through pragmatic means, rather than being read into the semantics.

Note two considerations favoring a pragmatic metonymy-based explanation over a semantic explanation interpreting 'I' as 'my car'. AS proponents of the attributive readings themselves note, similar (apparent) shifts in reference can be found in numerous non-deictic contexts. Thus a waiter in a diner might assert any of the following:

(FN 127) The ham sandwich left a small tip.

(FN 128) Some ham sandwich left a generous tip.

(FN 129) Most ham sandwiches are good tippers.

We thus have a choice between universal semantic ambiguity or an unambiguous semantics coupled with the emergence of pragmatic readings. Considerations of economy clearly favor the latter. Also, consideration of the counterfactual truth conditions of (FN 124) yield at best highly confused results, and certainly not straightforwardly the results predicted by the attributive theory (had I owned the red BMW instead of

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the blue Saturn, and had that BMW been parked in the parking garage, what would have been the truth value of the proposition expressed by (FN 124)?).

Second, attempts to find attributive uses of indexicals in non-extensional contexts seem to me (a) to ignore the prior difficulties introduced by such contexts and (b) to place too much weight on the particular form of words used. Consider the case of the man who, hearing a knock at his door, opens it to find a friend who says:

(FN 130) You should look before you open the door. I might have been a thief.

The claim here is that 'I' is to be understood as 'the man knocking at the door'. But why should we think this? If the second half of (FN 130) is understood as:

(FN 131) It was epistemically possible for you that I was a thief. then what we have here is yet another version of those attitude problems in which the same individual is apprehended in multiple ways. I am skeptical that much philosophical mileage can be gotten out of such difficult cases. Also, were one to understand the second half of (FN 130) as:

(FN 132) 'I am a thief' might have been truly asserted. then it can be unproblematically true with the normal semantics for 'I'.

Third, claims that some use of deictics, such as that in the road sign reading:

(FN 133) You are now entering San Francisco. must be interpreted as 'anyone reading this sign' in order to account for their ability to address different people at different times seems to me to place undue weight on our metaphysics of signs. If, for example, one were to take the utterance to be not the sign itself but the event of a viewing of the sign, then each utterance could be given the proper interpretation using the normal semantics for 'you'. (The case is similar to that discussed in [Kaplan 1977] of answering machine messages.) Note, however, that the context-insensitive theory of deictics given in §2.3.4.4.4 below is well-suited to yielding an appropriate interpretation of (FN 133) even when the sign itself is taken as the sole utterance. I leave the details of this proposal for the reader to work out.

Finally, supposed 'generic' interpretations of deictics, as in the proverbial:

(FN 134) You can't make an omelet without breaking some eggs. here to be understood as:

(FN 135) No one can make an omelet without breaking some eggs. are insufficiently sensitive to the possibility that we may have multiple homophonous terms in English -- some deictic and some not. The usual translation tests for ambiguity, for example, will indicate that the 'you' in (FN 135) is not the usual second-person pronoun 'you', since it translates into (e.g.) the German 'man' rather than 'du', or the French 'on' rather than 'tu'. Also, again the semantically empty interpretation of deictics of §2.3.4.4.4 below offers, I think, hope of obtaining through normal pragmatic devices an understanding of these generic uses of 'deictics'.

In short, then, I see no convincing evidence that we need to add to our semantics an attributive reading of deictics or weaken in any way the semantic assumption that deictics are rigid referring expressions.

operator, it always refers to me, regardless of what sorts of properties I or others might have at the other indices of evaluation considered by the intensional operator.

If anything, the rigidity of deictics is even more self-evident than that of proper names (especially that of 'I' -- for how could I be anyone other than who I actually am?).

Although (A) and (B) appear to be core features of deictics, we will now see that the phenomena of linguistic incompleteness we have been discussing thus far show that they cannot both be true.

In short, my worry is that there is a submerged conflict between (a) assuming that rigid referring expressions do not receive their reference in virtue of an accompanying sense and (b) assuming that there is some 'sense-like' semantic rule which determines the reference of deictics. By utilizing the arguments from linguistic incompleteness meant to bolster (a), we can draw this conflict to the surface.

Since deictics are, like proper names, rigid referring expressions, it can't be quite right that these singular terms get their reference from the sorts of descriptions used above:

(268) the producer of the word 'I'

(269) the object demonstrated by the producer of the word 'that'

because they would then not function properly in modal and other intensional contexts.<sup>252</sup> We've seen that we can't use Dummett-style scope conventions to enforce the rigidity, so there must be some other

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<sup>252</sup>That is, if 'that' were synonymous with 'the object demonstrated by the producer of the word *that*', then (e.g.) the following sentence would be analytically false (in any context):

(FN 136) I might not have been demonstrating that.  
since it would reduce to:

(FN 137) I might not have been demonstrating the object that I am demonstrating.

way to make 'I', 'this', and others rigid. I will discuss two common such methods and show that neither succeeds.

#### §2.3.4.4.1 Deictics and Homing Operators

If one attempts to extend a deictically-suited Davidson-style truth theory (which assigns truth to utterances rather than sentence) to a modal language, one can easily get the wrong truth conditions.<sup>253</sup> If, for example, one has the following axioms in the system:

(AX21)  $(\forall u)$  [Ref('I',u) = the utterer of u]

(AX22)  $(\forall \alpha)(\forall u)$  (an utterance u of ' $\alpha$  snores' is true iff  
Ref( $\alpha$ ,u) snores)

(AX23)  $(\forall \phi)(\forall u)$  (an utterance u of ' $\phi$ ' is true iff ( $\phi$  is  
true))

(overlooking some technical niceties), then one will get the result:

(TH1)  $(\forall u)$  (an utterance u of 'I snore' is true iff (the  
utterer of u snores))<sup>254,255</sup>

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<sup>253</sup>There are, of course, well-known problems with extending truth theories to modal languages. Problems of deictics aside, one will need to introduce axioms such as:

(AX FN 4) Ref('Socrates' = Socrates)

which, if the semantic properties of the language are (as they appear) contingent, are simply false. See [Davies 1978], [Peacocke 1978], and [Gupta 1978] for details on dealing with this problem. Of course, as the truth theory is extended to cover languages containing other dimensions of intensionality, the axioms will need to be similarly stabilized over all such dimensions.

<sup>254</sup>Proof (in a background logic of S5, although any modal logic in which the accessibility relation is transitive will support the proof):

- |   |  |                |
|---|--|----------------|
| 1 | (1) $(\forall \phi)(\forall u)$ (an utterance u of ' $\phi$ ' is true<br>iff ( $\phi$ is true))                  | AX23           |
| 1 | (2) $(\forall \phi)(\forall u)$ (an utterance u of ' $\phi$ ' is true<br>iff ( $\phi$ is true))                  | 1, E           |
| 1 | (3) $(\forall u)$ (an utterance u of 'I snore' is true<br>iff ('I snore' is true))                               | 2, $\forall E$ |
| 4 | (4) $(\forall \alpha)(\forall u)$ (an utterance u of ' $\alpha$ snores' is true<br>iff Ref( $\alpha$ ,u) snores) | AX22           |
| 4 | (5) $(\forall \alpha)(\forall u)$ (an utterance u of ' $\alpha$ snores' is true<br>iff Ref( $\alpha$ ,u) snores) | 4, E           |

which gives the wrong truth conditions, since the utterer of *u* may not, in some possible worlds, be me.<sup>256</sup>

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4	(6) $(\forall u)$ (an utterance <i>u</i> of 'I snores' is true iff $\text{Ref}('I',u)$ snores)	4, $\forall E$
1	(7) (utterance <i>U</i> of ' I snore' is true iff ('I snore' is true))	3, $\forall E$
4	(8) (utterance <i>U</i> of 'I snores' is true iff $\text{Ref}('I',u)$ snores)	6, $\forall E$
1,4	(9) (utterance <i>U</i> of ' I snores' is true iff ( $\text{Ref}('I',u)$ snores))	7,8 T
10	(10) $(\forall u)$ [ $\text{Ref}('I',u)$ = the utterer of <i>u</i> ]	AX1
10	(11) $(\forall u)$ [ $\text{Ref}('I',u)$ = the utterer of <i>u</i> ]	10, E
10	(12) [ $\text{Ref}('I',U)$ = the utterer of <i>U</i> ]	11, $\forall E$
1,4,10	(13) (utterance <i>U</i> of ' I snores' is true iff (the utterer of <i>U</i> snores))	9,12, 14.15
1,4,10	(14) $(\forall u)$ (utterance <i>u</i> of ' I snores' is true iff (the utterer of <i>u</i> snores))	13, $\forall I$
1,4,10	(15) $(\forall u)$ (utterance <i>u</i> of ' I snores' is true iff (the utterer of <i>u</i> snores))	14, I

where T is the rule allowing the inference from ' P' and ' Q' to ' R' where R is a tautological consequence of P and Q, and 14.15 is the appropriately modalized version of [Whitehead & Russell 1925]'s extensional 14.15, and thus allows replacement, in any modal context, of a singular term with a *necessarily* codenoting description:

$$\alpha = (\iota x)\varphi(x)$$

$$\Sigma(\alpha)$$

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$$\Sigma((\iota x)\varphi(x))$$

<sup>255</sup>Note that the definite description 'the utterer of *u*' takes small scope with respect to the necessity operator. The proof in the previous footnote shows that only this small scope reading can be derived.

<sup>256</sup>Even if one takes it to be an essential property of an utterance that it is produced by its actual producer (a difficult position to maintain, if one thinks about, say, typed or written as well as spoken utterances), TH1 will not yield the correct truth conditions simply because there may well be worlds in which there is no utterance and hence no utterer -- worlds which are, nonetheless, worlds in which I exist and worlds which are relevant to the truth value of 'Necessarily I snore'. Demonstratives provide additional problems for the advocate of the simple theory sketched by AX1 - AX3. In the case of that, the appropriate axiom seems to be something like:

$$(AX\ FN\ 5)\ \text{Ref}('that',u) = \text{the object demonstrated by the producer of } u$$

Even if I am essentially the producer of all my actual utterances, I clearly could have demonstrated things other than the things I actually did.

Standardly this problem is fixed by actualizing the descriptions associated with the indexical (or, *mutatis mutandis*, with the demonstrative) to obtain:

(AX21\*)  $(\forall u)$  [Ref('I',u) = the actual utterer of u]

We would then derive:

(TH2)  $(\forall u)$  (an utterance u of 'I snore' is true iff (the actual utterer of u snores))

which will give the correct truth conditions and provide the desired modal rigidity. Of course, 'I' should be rigid through all dimensions of intensionality, so we need to add appropriate homing operators for each dimension ('now', etc.). But here incompleteness rears its head. If the language in question is incomplete to the extent of lacking a homing operator for one of its dimensions of intensionality, then it cannot construct a descriptive axiom for the indexicals which will maintain the appropriate rigidity.<sup>257</sup> Even if the language does have all the necessary homing operators, it still cannot ensure *transparent* rigidity, since the epistemic possibility of new intensionalities or new structures to existing intensionalities cannot be ruled out.

#### **§2.3.4.4.2 Deictics and Truth-in-a-context**

The second standard approach to handling deictics is that pioneered by [Kaplan 1977]. Here truth *simpliciter* is replaced by truth with respect to a context, where one element of the context is the speaker of the

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<sup>257</sup>Assuming that the linguistic resources employed in the truth theory for the object language are limited to those possessed by the object language itself. I discuss below the motivation for this assumption. (Of course, the truth theory for a language L cannot be entirely so limited, since it must make use of the concept of 'truth-in-L', which we know to be undefinable in L.)



utterance, and another is the object demonstrated. In obtaining the correct behaviour of the deictic in:

(270) I snore

then, we proceed in two stages. First, we define a context (in which the sentence is to be evaluated) as an ordered quadruple of agents, times, positions, and worlds. Thus for a context  $c$ , we have the following:

$c_A$  = the agent in  $c$

$c_T$  = the time of  $c$

$c_P$  = the position of  $c$

$c_W$  = the world of  $c$ <sup>258</sup>

We then perform semantic evaluation relative to a context:

$\text{Val}(\text{'Here'})_c = c_P$

$\text{Val}(\text{'I'})_c = c_A$

$\text{Val}(\text{'I snore'})_c = T$  iff  $\text{Val}(\text{'I'})_c$  snores

Since we can use a rigid referring expression  $c_A$  in the context for specifying the speaker, we avoid the problem of the shifting denotation of the description 'the producer of  $u$ ' within modal contexts.  $c_A$  refers rigidly to me, and thus continues to pick me out even when imbedded in a modal context.<sup>259</sup>

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<sup>258</sup>This approach to contexts is drawn from [Kaplan 1977, 543], and is designed for the analysis of what Kaplan calls 'pure indexicals'. As Kaplan notes, our notion of a context may have to be expanded considerably to accommodate demonstratives and other indexical elements of the grammar.

<sup>259</sup>I have altered the technical details of Kaplan's presentation slightly here. In particular, Kaplan defines truth relative not just to a context, but also to a world (and time) of evaluation. He then uses the following clause for evaluating modal operators:

$\text{Val}(\varphi)_{c_W} = \text{true}$  iff  $\forall w', \text{Val}(\varphi)_{c_W'} = \text{true}$

When modal operators are implemented in this method, it is the fact that the value of deictics is defined as constant over choice of world (i.e.,  $\text{Val}(\text{'I'})_{c_W} = c_A$  for all  $w$ ) which provides their rigidity, not the rigidity of  $c_A$ . However, Kaplan elsewhere expresses a reluctance to take the referent of a rigid term as a function of worlds:

However, all we have really done here is, in a sneaky technical way, reintroduced Dummett's move by pulling the scope of that description outside the scope of any intensional operators within the sentence. In order to construct the context needed to evaluate the sentence, we still have to pick out the relevant objects, and we will (presumably) use descriptions like 'the producer of u' or 'the object demonstrated by the producer of u' in order to do so. It's just that by constructing the context before turning to the semantic evaluation of the sentences, we place these descriptions outside the scope of any operators in the sentence itself. But, as we saw in our discussion of Dummett, such a move fails to capture rigidity in any language which is weakly incomplete in the sense of lacking a totalizing operator, and always fails to capture the 'transparent' character of rigidity.

#### **§2.3.4.4.3 Object Language, Metalanguage**

Once linguistic incompleteness is taken into account, then, neither the use of actualized descriptions nor the move to truth-in-a-context suffices to capture the full force of the rigidity of deictics.

Defeating two examples, of course, is no proof of the generic impossibility of the project, but I would suggest that we now have good reason to think that no semantic theory of deictics which is faithful to

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In actual fact, the referent, in a circumstance, of a directly referential term is simply independent of the circumstance and is no more a function (constant or otherwise) of circumstance, than my action is a function of your desires when I decide to do it whether you like it or not. [Kaplan 1977, 497]

If the semantic value of the deictic is not to be given as a function of worlds, it is then crucial that it be given in some world-indifferent manner (e.g., via a proper name) which can then be freely moved in and out of modal contexts.

(A) and (B) will emerge unsullied from the quagmires of linguistic incompleteness.

One might suspect, however, that I have already crucially misrepresented the goals of both of the semantic approaches I discuss above. Why should the linguistic incompleteness of the object language pose any threat to our ability to describe, in a metalanguage, the semantic behaviour of such terms? In particular, why should the fact that the object language lacks (or is not (to its own speaker) definitively known not to lack) certain 'homing' operators stop us from using homing operators we do have to ensure the appropriate behaviour of the object language deictics? Similarly, why should the lack of object language totalizing operators stop us from using our metalanguage totalizing operators in semantic analysis?<sup>260</sup> It is always open to us, as theorists about an object language, to make the observation that these people use the word 'I' to refer to the speaker of the utterance containing the word 'I'.

If we go this route, however, we concede important methodological ground. In some important sense we have abandoned feature (A) of deictics. For feature (A) is meant to say more than just that these descriptions -- the producer of 'I', the object demonstrated by the producer of 'that' -- are accurate guides to the referents of the deictics. We want to say more than this: it is a fact *about the object language*, about what words mean *in this language*, that deictics function

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<sup>260</sup>To take this line, of course, is implicitly to assume that English is not an incomplete language -- a position I argue against above. But even if English were incomplete, all that would follow would be that we would need to improve our own language before we could successfully give semantic analyses, not that such analyses were impossible. (That only a complete metalanguage could support analysis, not that only a complete object language could be analyzed.)

according to these rules. If there is to be such a fact about their language, it should be implemented in such a way as to be compatible with the intensional apparatus available within that language, and it is *this* task which there seems to be no way to perform.

A methodological quandary is thus imposed on the semanticist. We can obtain the right referents for demonstratives and indexicals -- and hence the right truth conditions for (utterances of) sentences containing such terms -- but only at the price of weakening the overall goals of our semantic theory. The semantic theory, we might say, will no longer be a semantic theory recognizable to the speakers of the object language. It will no longer explain how, within the object language, deictics obtain the referents they do. Given this, the claim that the *semantics shows us* that the indexical 'I' refers to the speaker of the utterance must be abandoned: we have to rest content with the weaker claim that we are shown, via the semantics, that 'I' refers to the speaker of the utterance *however described*. Thus we could replace that description with any other coextensive description, or even with a list of names for distinct occurrences of the word 'I'.

Such a methodological shift should not be made lightly. Should we make it, we lose the very ability to claim that there is a *single word* 'I'. Since all that is required of the semantic theory is that it assign the appropriate referent to each occurrence of 'I', we are perfectly free to take the phonetic form 'I' to stand for a series of homophonous proper names, each of which refers to one particular person. It would then be a (semantically uninteresting) fact that any given speaker will use only one of these many homophonous names. If we accept that the project of the linguistic theory is merely to chart the idiolect of a

particular speaker, rather than the language of a community at large, then we see that only one word 'I' is necessary, and we face not even the slightest pragmatic hurdle to analyzing 'I' straightforwardly as a name (a name shared with no other speaker). But surely to treat 'I' in this manner is to ignore what, *prima facie*, is the central *semantic* fact about 'I'. To weaken one's methodological goals in this way, then, is to concede that (A) must be abandoned.<sup>261</sup>

#### **§2.3.4.4.4 A Return to Truth Simpliciter**

I am not happy with making it a brute, rather than a (psychologically?) explanatory semantic fact that deictics take on the referents they do where they do. My suggestion, then, is that the general approach to singular terms which I developed in §2.3.2 above can profitably be applied to deictics. Thus, rejecting the Correlation Principle, we will hold that the sentence:

(271) I snore

employs a free variable and is to be formalized as:

(271') x snores

which, due to the presence of the free variable, receives no truth value and expresses no proposition. Following this route, we abandon the idea of truth-in-a-context and return to the idea of truth simpliciter.

We want it to be a semantic fact that deictics refer as they do. The phenomenon of linguistic incompleteness shows us that we cannot have this fact. One approach to this problem -- the standard one -- is to keep the reference but abandon the meaning behind it. To do so is, in

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<sup>261</sup>One could, I suppose, retain (A) and a robust semantic theory simply by giving up (B). But to give up (B) is to empirically falsify one's theory.

essence, to give up the very category of deictics and even to give up the very word 'I'. Another approach, however, is to insist that the connection between rule and reference remain, but abandon the idea that the connection lies in the semantics. On this approach -- the one I am advocating here -- we empty the semantics of deictics, and hope to reimpose on some other level of linguistic understanding (perhaps in our pragmatics) the thought behind feature (A).<sup>262</sup>

#### **§2.4 Summary of a New Taxonomy of Noun Phrases**

I want to close this chapter by indicating how the work done here gives rise to a new taxonomy of noun phrases, one in which, as desired, syntax and semantics go hand in hand. The central idea of this taxonomy is that at the core of every noun phrase is a variable. This variable will then manifest itself in surface syntax in various ways depending on what binding apparatus is brought to bear on it. At the level of logical form, however, one can think of the core sentence as containing some verbal structure with the argument positions all filled by noun phrases in the guise of pure variables. Further adorning this core structure will be any number of additional operators. Some of these operators will be sentential operators like negation or modal operators, but other will be variable binding operators, whether anaphoric binders such as N' constructions or variable distributors such as determiners.

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<sup>262</sup>The work of [Evans 1981] and [Evans 1982] on accounting for the features of our thought which enable us to entertain thoughts about demonstratively and self-referentially located objects provides one avenue for spelling out all the details about how a pragmatic mind-based (as opposed to a semantic language-based) account of reference functions. Unlike Evans, however, I am reluctant to take the kinds of recognitional capacities and dispositions to judgement, many of which seem essentially private and inaccessible to other speakers, as appropriate for incorporation into a theory of *semantics*.

At the level of logical form, then, the syntactic taxonomy of noun phrases has but a single category -- that of the variable. Semantic variations within that category are to be explained by differences in the operator superstructure surrounding the variables. As we move to surface structure, however, the syntactic taxonomy becomes more complicated. At least in English, the various variable binding lexical items move down in the phrase tree to occupy syntactic positions held by the variables in logical form. Depending on both the nature of the binding operators and the nature of the binding relations, a number of possibilities result. Variables which are wholly unbound survive into surface structure and manifest themselves phonetically, as pronouns, proper names, demonstratives, indexicals, wh-words, etc. Where there are binding operators, we get either quantified noun phrases (when both anaphorically binding N' and distributing determiner are present) or bare plural noun phrases (when anaphoric binder but not determiner is present). As a further complication, when one binding operator binds multiple variables, it moves into only one syntactic position, leaving the other bound variables the ability to manifest in surface syntax. Thus we find that pronouns in surface syntax span the full semantic behaviour of noun phrases, because (when multiple variables are bound by a single operator) they can result from logical form variables under any binding configuration.

Given a sufficiently phonetically rich initial stock of variables, these remarks suffice to account for the bulk of the (surface structure) noun phrases adduced at the beginning of this chapter. Some difficult cases require additional epicycles, some of which -- e.g., the multi-headed syntactic structures lying behind complex demonstratives -- have

already been provided and some of which -- e.g., accounts of infinitival noun phrases or of that-clauses -- are left as open projects. It is worth noting that this syntactic taxonomy of noun phrases departs substantially from the X-bar schema which lies at the heart of Chomskyan syntax. In particular, by allowing binding configurations of variables to drive the syntax of noun phrases, I give up the idea that there is a single underlying syntactic strategy which lies behind the construction of all phrasal types.<sup>263</sup> The hope is that the unity provided in the Chomskyan framework by X-bar theory can be recaptured here by developing a verb-driven account of syntax in which verbs, by virtue of their semantic properties, introduce certain frames which form sentential cores and then allow supplementation by various operators, but considerable work remains to see if such an account can be adequately general as a theory of natural language syntax.<sup>264</sup>

The variable-centered syntactic taxonomy of noun phrases also gives rise naturally to an account of the semantic behaviour of NPs. That some noun phrases express singular propositions and refer ('refer') rigidly while others express general propositions, denote non-rigidly, and exhibit world-centered context sensitivity falls out of the distinction between variables whose semantic behaviour is governed by predicate material, which is correlated with properties rather than objects and which thus varies in extension as the character of the world alters, and variables whose semantic character is empty and which thus act as placeholders for objects, whose singularity and necessary self-

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<sup>263</sup>This claim relies on the assumption, defended in §3.2.2.2.1 below, that first-order quantification is the only form of quantification which occurs in natural language.

<sup>264</sup>I draw heavily here on [Neale (forthcoming-a)].



identity give rise to the singularity and rigidity of the resulting pragmatically conveyed proposition.

The work here provides only an indication of how the new taxonomy might appear, and much work remains to be done to see that it will indeed be fully adequate to the needs of natural language syntax and semantics. One major chore in this direction will occupy much of the next chapter, as we consider whether the behaviour of bound variables as predicted by the anaphoric account of variable binding is of the right breadth to match the behaviour of quantified noun phrases in natural languages. Other portions of the work will have to remain open questions. Nevertheless, hopefully enough has been done here to suggest (a) that there are good prospects for a more satisfactory taxonomy of noun phrases arising out of the new paradigm provided by the anaphoric account of variable binding and (b) that these prospects in turn give us good reason to believe that the anaphoric account, rather than more traditional understandings of variables and variable binding, is the right way to think about quantification.

## Chapter 3

### Mechanisms of Variable Restriction and Distribution

#### §3.1 Some Unanswered Questions

In the previous chapter, I attempted to show how an anaphoric theory of variable binding of the sort sketched at the end of chapter 1 could be used to produce a substantially more unified and elegant account of the natural language category of noun phrases -- an account which treated this category as a genuine syntactic and semantic category, with a single mode of semantic operation lying behind it. While this explanatory payoff serves, I suggest, as support for the appropriateness of the anaphoric theory, the work of the previous chapter largely presupposed a workable version of the anaphoric account sketched at the end of the first chapter and did little to answer some of the fundamental questions about the nature of quantification and variable binding that I raised in the first chapter. A few issues, such as the reasons for the syntactic limitation on the semantic range of quantifiers and the nature of the semantic values passed from variable restrictors to variables, were touched on and clarified, and details concerning the connection between a formal language of anaphoric binding and the structure of natural language, such as the nature and distribution of variables in natural language and the connection between proper names and variables, were investigated in detail, but the larger and more fundamental questions about quantification and variable binding proper, such as the reasons that quantifiers are sentential operators transforming nonpropositional entities into propositional entities or

the sources of the order-dependence of iterated quantifiers or the proper arity of the variable binding relation, remain unaddressed. Also open are the technical challenges set out early in the first chapter. We have yet to see whether the anaphoric account can serve as a core notion of quantification uniting a sufficiently broad class of putatively quantificational phenomena. In this final chapter, then, I turn to detailed investigations of the two components of the anaphoric account -- the process of variable restriction and the process of variable distribution. These detailed investigations will answer several technical questions which thus far have been relegated to the background, and will indicate from time to time where the anaphoric theory has additional utility in evaluating philosophical issues which border on the philosophy of language.

Recall that the anaphoric theory of variable binding makes a substantial break from the classical tradition in taking quantification to be essentially restricted. On the anaphoric theory, a sentence like:

(271) All F's are G's.

in its classical regimentation:

(271-C)  $(\forall x)(Fx \rightarrow Gx)$

is to be understood as *first* involving an assignment of content to the variables -- an assignment of content which is surreptitiously accomplished metasemantically through the specification of a domain of quantification -- and then only secondarily as involving the application of the distributor ' $\forall$ ' to the now-contentful variables. Again, the claim is that classical logic, by focusing on the universal and existential quantifiers as the paradigms of variable binding, has overlooked the real process of variable binding -- the restriction which

provides semantic content to the variables. The formal paradigm of restricted quantification, which regimented (271) as:

(271-RQ) [all x: Fx] Gx

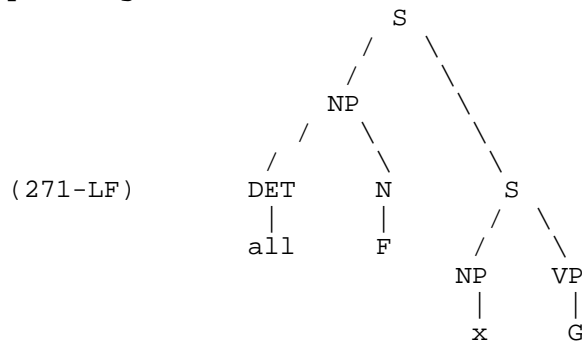
more explicitly draws attention to the two components of quantification identified by the anaphoric account, and we can also use syntactic forms such as:

(271-AA) [Fx]<sub>x</sub> (∀x)Gx<sup>265</sup>

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<sup>265</sup>Note that the anaphoric account is *not* a new formal language, but a proposal for how to understand the semantic mechanisms underlying concepts employed in languages we already use. Thus there is no distinctive syntactic form for anaphoric variable binding, and expressions like (271-AA) are used purely heuristically in order to bring attention to certain semantic features posited under the anaphoric account. Any language, in principle, can be understood in accordance with the dictates of the anaphoric account. Of course, syntactic issues cannot be evaded under a discussion of the anaphoric account. For example, the discussion of partial binding in intensional contexts in §2.2.2.2.1.3 reveals the need for a syntactic indexing of partial binding relations which is not provided by, say, classical or restricted quantificational formal languages. We may thus discover that the syntactic resources of some languages are inadequate for full control over the semantic properties undergirding those languages (a lesson familiar from the discussion of §2.3.4) In §3.3 below, syntactic issues will rear their heads again as we discuss partially ordered quantification and the distinction between simple and complex distribution.

I have, of course, argued at great length in the second chapter that the way in which syntax and semantics interact in natural languages is best suited to explanation via the anaphoric account of variable binding. It would be nice, then, to have a fully developed account of natural language syntax which reflected that semantic account. The primary difficulty here is that the dominant Chomskyan understanding of natural language syntax, even after reconstrued in the ways I suggest in §2.1.3.1, continue to treat in a sentence like (271) above 'all Fs' as a unit, positing a structure at the level of logical form something like:



Preferably, we would have a semantics which, at logical form, treated common-noun level expressions as extra-sentential operators and

which fully distinguish the restricting from the distributing components of variable binding.

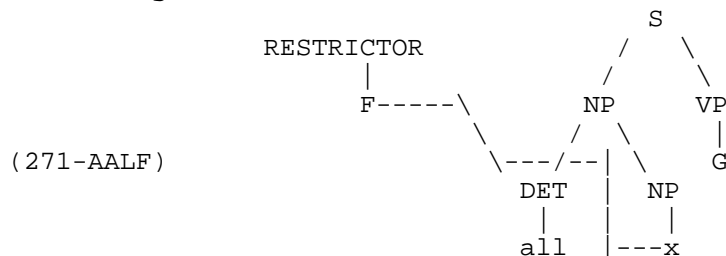
A full defense of the anaphoric account, then, requires detailed investigations of these two key concepts: variable restriction and variable distribution. In this chapter we take up such a defense, examining first restriction and then distribution. The chapter then closes with a discussion of some residual worries about the compositionality of semantics under the anaphoric account.

### §3.2 Variable Restriction

All quantification is restricted quantification. In this section, we will explore both the meaning and the consequences of this fact. We will begin by focusing on the first-order case. Here our task will be two-fold: first to settle questions about how variable restriction works in the first-order case, and second to explore the importance of the fact that it works in this way. While the bulk of the remarks on variable restriction will focus on the first-order case, we will close this section with a tentative examination of lessons of the anaphoric account

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determiners as operators on (chains of) variables. Thus (271) would become something like:



(The picture is even more complicated when the determiner needs to modify a non-trivial chain). Some further story would then be needed about the transition to surface form, in which free-floating extrasentential restrictors move into term positions and determiners move from chain-modifying to restrictor modifying positions. Obviously it is a not insubstantial defect of my position that I have no such fully developed syntactic story to tell.

for 'higher-order' quantification.<sup>266</sup> The thrust of this discussion will be less exploratory and more therapeutic. After situating the anaphoric account within the traditional objectual/substitutional dichotomy, I will argue that there is an important conceptual confusion at the heart of discussion of higher-order logics, and will attempt show how my account's unusual position in the objectual/substitutional dichotomy allows it better to avoid this confusion. Despite this conceptual success, however, I remain doubtful of the ultimate coherence of higher-order logics, and close the discussion of variable restriction by sketching some reasons why we might be wary of such.

### **§3.2.1 Restriction of First-Order Variables**

In the first-order case, variables are restricted by predicate-like material (open formulae, in most formal languages, and N' expressions, in natural languages). The relevant semantic feature of these predicate-like materials is their ability to be satisfied by objects. Think of predicates as filters, which pass certain objects through and reject others. By transmitting that filtering potentiality to the variables, these predicates then enable those variables to refer to those objects which satisfy the predicates. The first point to note here is that there will, in general, be more than one object which satisfies a given predicate. Variables restricted by these predicates, then, will in general be plural referring expressions. Our first task, then, will be to make some observations about the nature, function, and ontology of plural reference. We will also comment briefly on the behaviour of

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<sup>266</sup>I will suggest below that the traditional appellation 'higher-order' is misleading.

variable restriction in those cases in which the restrictor is an open formula with more than one free variable.

Having thus laid the foundations for the process of variable restrictions, we will then move to examine two interesting consequences of a view which takes restricted quantification to be conceptually prior to unrestricted quantification. First, we will demonstrate that not every conception of free logic can be successfully transcribed from unrestricted to restricted quantificational notation, and thus will derive some constraints on what form a true free logic would have to take. Second, we will show that certain recent conceptions of logic rely on logical properties closely allied with unrestricted quantification, and thus that these conceptions are ill-suited to underlie our general understanding of quantification.

#### **§3.2.1.1 Plural Reference**

In my account of variable binding, variables<sup>267</sup> become, by virtue of the process of restriction, referring expressions referring to whatever satisfied the predicate (more generally, well-formed formula) which restricts the variable. Since quite often more than one object satisfies a single predicate, it will also often happen that a variable suitably restricted refers to more than one object. I thus want to begin by saying a few words about the nature of plural reference. These remarks will serve both to clarify some of the semantic and ontological assumptions which underlie the notion of variable restriction and to lay the basis for some later conclusions about the explanation of branching and cumulative quantification within my system.

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<sup>267</sup>In the first-order case.

There is a vast and rapidly expanding literature on the topic of plural reference, one which derives largely from the seminal work of [Link 1983]. While I won't discuss the details of the opposition views here, I want to begin by noting that my account of plurals differs from the mainstream view by rejecting a certain pervasive *singularist bias*. Accounts of plural reference in the Link tradition tend to eliminate the plurality of plurals in one of two ways:

- By making plural reference into a relation between a word and a single object, usually a set, group, collection, or mereological sum of individuals.
- By reducing facts of plural reference to underlying prior facts of singular reference.

Both of these eliminativist tendencies seem to be driven by an assumption that the appropriate language of logical and semantic analysis is a singular one, and thus that claims about plurals must be regimented into or understood in terms of talk about individuals.

However, there is no basis for this bias.<sup>268</sup> Natural language contains

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<sup>268</sup>A full defense of pluralism would need to discuss and reject singularist doctrines of metaphysics and mind which undergird semantic singularism. The metaphysical singularist assumption is that properties and relations are, in the ultimate constitution of reality, properties of and relations among single individuals, not plurals (where, again, 'plurals' here is not meant to pick out some singular entity which the many compose). Were metaphysical singularism true, then we would expect that, since the facts being described by the language were ultimately singular, the plural linguistic claims themselves would be eventually reducible to singular claims. The singularist philosophy of mind assumption is that aboutness or object-orientation of mental states is always directed at single objects (note, thus, that attempts to spell out the necessary conditions on object-dependent thought, such as those of [Evans 1982], always presuppose that the thought is dependent on a single object (even if, as in the case of a thought relating, e.g., Russell to Whitehead, the thought is of two individual objects). Again, should the singularist doctrine hold, we would be justified in assuming that, since linguistic facts must be facts we can grasp, that linguistic facts too are ultimately singular. A defense of metaphysical or mental pluralism, or even a rebuttal of such singularisms, lies beyond the



fundamentally plural constructions, and it is a mistake to try to understand this plurality in terms of the singular. I will thus develop what I call a *genuine pluralist* account of plural reference, which begins with the rejection of the singularist bias.

### §3.2.1.1.1 The Genuine Pluralist Account of Plurals

The main thrust of my discussion will be that plural reference is a far less troubling and complex matter than it is generally taken to be. There are plenty of sentences in even the most common-place English which make use of plurally referring expressions:

(272) Russell and Whitehead wrote *Principia Mathematica*.

(273) Ajax and Odysseus quarreled over Achilles' armor.

(274) We are going to see *Vampyr* tonight.

(275) I saw them at the bookstore today.

The genuine pluralist suggestion is that the appropriate explanations of the referential facts in the above claims are:<sup>269</sup>

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scope of this work. In the case of mental pluralism, we might begin by asking how a condition such as Russell's Principle:

(RP) In order to have a thought about an object, you must know which object you are thinking about.

might be rephrased and understood in the plural case. Consider in this light [Evans 1982]'s example of the two indistinguishable spinning metal balls. Even if we accept Evans' conclusion that we cannot have thoughts about either ball, might we still hold that we can have thoughts about the two balls?

<sup>269</sup>Strictly speaking, I (due to my position on singular terms developed in §2.3) reject all of these claims as part of my general rejection of the claim that singular terms ever refer. These names (variables), however, are the kinds of things which potentially plurally refer -- that is, which create satisfaction conditions involving plural reference. As mentioned above (§2.2.2.1.2) my view is that syntactic number is without semantic import, and thus that all variables, even (syntactically) singular pronouns and simple proper names, are fundamentally plural, although grammatical number creates a pragmatic preference for a singular reference. On my view, the only *genuine* plural referring expressions are *bound* variables.

In the discussion that follows, however, I speak always as if proper names refer, and in particular as if certain proper names refer plurally. All strictly false claims about plurally referential names in the main text can, with the disadvantage of added verbiage, be

(R-272) 'Russell and Whitehead' refers to Russell and Whitehead.

(R-273) 'Ajax and Odysseus' refers to Ajax and Odysseus.

(R-274) 'We' refers to [P1 and P2 and P3 and ...].<sup>270</sup>

(R-275) 'They' refers to [Q1 and Q2 and ...].<sup>271</sup>

These reference claims are distinguished by simply accepting that the reference relation is a one-many relation -- one which holds between a single word and many objects -- rather than a one-one relation which holds between, say, a single word and a set, group, or mereological sum of objects.<sup>272</sup> While in *some* cases facts of plural reference seem to be

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transformed into true claims about the plural satisfiability of sentences containing certain names.

<sup>270</sup>Note that an occurrence of 'we' may refer to infinitely many objects. In general, plural reference need not be *finitely* plural reference.

<sup>271</sup>We thus see here two types of plural referring expression. Some simple lexical items, such as the plurally numbered pronouns 'we', 'you' ('y'all'), and 'they', are plural referring expressions. Presumably we could also introduce proper names which refer to several individuals. 'Camper Van Beethoven' might serve as such a name, although there are delicate issues here about the distinction between referring (plurally) to individuals and referring (singularly) to an organization with individuals as members.

We can also form *complex* plural referring expressions by linking together (singular) plural referring expressions with a conjunctive term, as in 'Russell and Whitehead'. It is important to note that the 'and' here cannot be (in any obvious way) reduced to the Boolean *sentential* connective '&' (see [Partee & Rooth 1983] for discussion of the relation between the apparently irreducibly NP-joining conjunction and the Boolean sentential conjunction). It's also interesting to note that, while we can use other (apparently) Boolean connectives to join NPs -- as in 'Russell or Whitehead' or 'If Russell then Whitehead' -- such constructions do not appear to give rise to plural referring expressions. I admit to some dissatisfaction with this state of affairs. The connectives 'and' and 'or' are much more puzzling than might be supposed.

<sup>272</sup>The claim that plural reference is a one-many relation, unfortunately, does not suffice as a rejection of the singularist bias. Mathematically, a one-many relation is understood as a collection of ordered pairs in which more than one ordered pair can have the same first element in common (thus preventing the relation from being a function). Such an understanding of a one-many relation, however, is not a plural understanding, since it rejects the plurality of the many related to the one in favor of a reductive singularist analysis into many facts about singular relations between the one and other ones. The deeper problem here is that the singularist bias is encoded into the very language of mathematics, which accepts only reference to singular objects and which accommodates talk of plurals by ontological accumulation, introducing (singular) sets of individuals as proxies for

supervenient on facts of singular reference -- as the reference of 'Russell and Whitehead' to Russell and Whitehead is on the reference of 'Russell' to Russell and the reference of 'Whitehead' to Whitehead -- in general plural reference should not be seen as deriving from underlying facts about singular reference.<sup>273</sup> We thus want to resist all of the following formulations of the facts of reference:

(\*R272-1) 'Russell and Whitehead' refers to Russell and refers to Whitehead

(\*R272-2) 'Russell and Whitehead' refers to each of Russell and Whitehead

(\*R272-3) 'Russell and Whitehead' refers to the set {Russell, Whitehead}

(\*R272-4) 'Russell and Whitehead' refers to the group (whose members are) Russell and Whitehead

'Russell and Whitehead' simply refers to Russell and Whitehead.<sup>274</sup>

Corresponding to this genuine pluralist account of plural reference is a genuine pluralist account of truth conditions for sentences containing plurals. Given a sentence of the form:

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true pluralities (where a plurality is not a (singular) thing, but is many things). For an interesting critique of the singularist bias of mathematics and a suggestion that this bias leads to many of the foundational problems in set theory, see [Lewis 1991]. It is perhaps the singularist bias of mathematics which tempts so many formal semanticists into singularism.

<sup>273</sup>It is perhaps best to think of singular reference as the limiting case of plural reference. The reference of 'Russell' to Russell, then, is just like the reference of 'Russell and Whitehead' to Russell and Whitehead, except that in the former case the 'plurality' referred to has but a single member.

<sup>274</sup>We might wonder whether, given that 'Russell and Whitehead' refers to Russell and Whitehead, it is also proper to say that 'Russell and Whitehead' refers to Russell, or that 'Russell and Whitehead' refers to Whitehead. I have no strong views on such matter, but see no harm in such locutions so long as it is kept in mind that the reference of 'Russell and Whitehead' to Russell and Whitehead is not to be *reduced* to reference to Russell and reference to Whitehead.

(272) Russell and Whitehead wrote *Principia Mathematica*.

we want truth conditions of the form:

(T272) 'Russell and Whitehead wrote *Principia Mathematica*' is true

iff Russell and Whitehead wrote *Principia*

*Mathematica*.<sup>275,276</sup>

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<sup>275</sup>The appropriateness of (T272) is determined not by a general principle that metalinguistic ascriptions of truth conditions, when the metalanguage and object language are identical, ought to be homophonous. I see no reason to suppose that any semantically useful notion of truth condition or of interpretive T-sentence will impose a homophony constraint (and, of course, in any language containing token-reflexive constructions, pure homophony will be *impossible*). Instead, the thought is that (T272) is appropriate in that it respects the plurality of the object language construction by endorsing the acceptability of metalanguage plural constructions for analysis and thus avoiding the analysis of the plural into the singular. Any statement of truth conditions preserving this respect for plurality would share with (T272) the relevant superiority of the competing (\*T272-1) and (\*T272-2). Similar remarks apply to the superiority of (R272) over the competing (\*R272-1) through (\*R272-4).

<sup>276</sup>The derivation of truth conditions like (T272) will require, in addition to plural reference axioms of the form (R-272), satisfaction axioms for the predicate 'wrote *Principia Mathematica*' which allows for that predicate to be satisfied by plurals in addition to individuals. If we countenance quantification over plurals (as I suggest we do), such an axiom can be phrased simply as:

(AX FN 6)  $(\forall x)(x \text{ satisfied 'wrote } \textit{Principia Mathematica} iff x wrote *Principia Mathematica*)$

The simplicity of this axiom, however, masks important changes in the logical structure of predicates. Considered model-theoretically, predicates will no longer take simple sets of individuals as extensions. In order to accommodate the ability of plurals to satisfy predicates (in a way not reducible to the satisfaction of the predicates by individuals), we will need to incorporate those plurals in the extensions of the predicates. Within the (singularly biased) language of mathematics, perhaps the best we can do in this direction is to take the extension of a (monadic) predicate to be not a subset of the domain but rather a subset of the power set of the domain (here associating a plurality with the set of all those in the plurality, and a taking a single individual as the minimal case of a plurality).

Changing the semantics of predicates in this way, however, introduces difficulties for the anaphoric account of variable binding. The process of variable restriction, on this account, amounts to passing on to the variable the semantic potential of the predicate, and thus allowing the variable to refer to all those objects which satisfy the predicate. When the predicate is wholly singular (i.e., not satisfied by an pluralities), then in essence a collection of individuals is passed on to the variable, and the variable comes to refer plurally to those individuals. When, however, the predicate is not wholly singular, matters are more complicated. In such cases the predicate will pass on some pluralities, in addition to a number of individuals.

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The difficulty here lies in how we are to understand the induced plural reference of the bound variable. Is it simply to refer to all those objects which satisfy the predicate, whether directly or by way of being part of a plurality? Or is it somehow to refer plurally both to individuals and to pluralities? This second option would seem, from the point of view of the genuine pluralist, best avoided, since it involves an undesirable reification of pluralities as distinct (singular) entities. The worry, however, is that if we follow the first option, we will lose the ability to distinguish those individuals who by themselves satisfy the restriction from those who only in cooperative efforts satisfy the predicate.

Consider a concrete example:

(FN 138) Everyone who pushed a car up the hill was tired afterward.

regimented as:

(FN 139) [every x: x is a person & x pushed a car up the hill](x was tired afterward)

How are we to understand the plural reference of the bound x? In particular, if John and Fred cooperate in pushing a car up a hill, while Albert pushes a car on his own, is (FN 138) to be understood as saying that all three were tired? In this case, the first option (lumping the plural referents into the mass of objects referred to by the bound variable) seems to generate the better reading. Other cases, however, are less clear:

(FN 140) All those who pushed a car up the hill exerted 1000 newtons of force.

(FN 141) Everyone who pushed a car up the hill was paid \$100 for their efforts.

In (FN 140), it may look as if it is to John and Fred together that we attribute having exerted 1000 newtons of force, while in (FN 141) we might even be inclined to think that the quantification ranges only over those who by themselves pushed a car up the hill.

Matters are further complicated by considering common nouns which are satisfied only by plurals and not by individuals. Thus the noun 'lovers' is satisfied not by single individuals but by two individuals together (not, on the genuine pluralist account, by pairs of individuals (understood set theoretically as a single entity)). This satisfaction by plurals gives rise to sentences such as:

(FN 142) Most lovers come to hate each other eventually.

where the hatred holds not between any two people who are lovers of someone (but not necessarily of each other) but only between every two lovers (of each other). Here the variable binding seems to favor a reading on which the various pluralities are held distinct in the reference of the bound variable, rather than being lumped together. In the end, I have no complete satisfactory resolution of the difficulties arising due to the interaction of plural satisfaction of predicates and the restrictive role of predicates in variable binding.

(Note, incidentally, that sentences like (FN 142) should not be taken, as [Van Bentham 1989] takes them, as evidence of plural quantification in natural language. Van Bentham proposes that a sentence like (FN 142) be understood as involving a single quantifier binding two variables (one for each lover):

(FN 143) [most x,y: x and y are lovers](x and y come to hate each other)

But we can easily construct similar examples using common nouns requiring satisfaction by pluralities in which the cardinality of the plurality is not, as it is will 'lover', predetermined. Thus consider:

(FN 144) Most team members come to hate each other eventually.

Here again we remain committed to the basic legitimacy of the plural construction, and thus avoid giving truth conditions in terms of singular constructions and facts, such as:

(\*T272-1) 'Russell and Whitehead wrote *Principia Mathematica*' is true iff Russell wrote *Principia Mathematica* and Whitehead wrote *Principia Mathematica*.

(\*T272-2) 'Russell and Whitehead wrote *Principia Mathematica*' is true iff Russell wrote *Principia Mathematica* with Whitehead and Whitehead wrote *Principia Mathematica* with Russell.<sup>277</sup>

### §3.2.1.1.2 Genuine Pluralism and the Ambiguity of Plurals

In this section I want to discuss one line of objection to the genuine pluralist account, both in order to see how that line can be rejected and to uncover certain facts about the behaviour of plurals which will prove useful later. This objection holds that by ascribing simple reference axioms and truth conditions to plurals, we miss the fact that claims containing plurals are typically ambiguous.<sup>278</sup> Traditionally,

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(FN 144) allows a reading on which what is being declared is that, for most teams, the members of that team come to hate each other eventually. But this reading cannot be captured using polyadic quantification, because we do not know how many members the various teams will have and thus how polyadic our polyadic quantifier needs to be. Even in the case of (FN 142), it is merely contingent biological and cultural facts about human sexuality which allow a dyadic quantification to get the right truth conditions; presumably such contingencies should not determine the underlying logical form of the sentence.)

<sup>277</sup>Perhaps, in the end, it will be possible to analyze all claims about the actions of plurals into claims about the actions of individuals [see the literature on 'we-intentionality' for an exercise in providing such an analysis]. I take no stand on this issue here, but merely hold that it isn't the business of the semantic theory to provide such an analysis.

<sup>278</sup>The following discussion of the potential ambiguity of sentences containing plurals is heavily indebted to [Gillion 1987].

collective and distributive readings of plurals have been distinguished.

Given a sentence such as:

(272) Russell and Whitehead wrote *Principia Mathematica*.

we will have a collective reading on which they collaborated in the writing, and a distributive reading on which they (coincidentally enough) independently wrote the same book.<sup>279</sup> The genuine pluralist, the objection will then proceed, lacks the tools to deal with these ambiguities, and thus his account must be rejected.<sup>280</sup> I want to resist

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<sup>279</sup>Obviously some sentences are less amenable to one reading than the other. The sentence:

(FN 145) Ajax and Odysseus quarreled over Achilles' armor. for example, strongly prefers a collective reading (although it will allow a distributive reading on which they, e.g., both quarrel with Diomedes over the armor). Similarly, the sentence:

(FN 146) Babe Ruth and Lou Gehrig hit a home run. strongly prefers a distributive reading (but can admit a collective one). Whether there are any sentences which *require* one reading or the other I leave an open question here.

<sup>280</sup>I ignore here the possibility that the genuine pluralist might accept these ambiguities and simply claim that the ambiguity of the object language sentence is mirrored by the ambiguity of the metalanguage sentence. The proposal would thus be that an axiom like:

(T272) 'Russell and Whitehead wrote *Principia Mathematica*' is true iff Russell and Whitehead wrote *Principia Mathematica*. would be acceptable, since there would be collective and distributive readings of the metalanguage analysis, and these readings would induce the collective and distributive readings of the object language sentence.

In general, however, the accommodation of ambiguity in a truth theory by this sort of object language/metalanguage mirroring strikes me as a poor strategy. If we assume that the ambiguous sentences are ambiguous at least to the extent that they can have readings differing in truth value (i.e., the 'ambiguous' sentence can in some situations be true on one reading and false on another) then the approach is an unambiguous failure. Consider, for example, the ambiguous sentence:

(FN 147) John keeps his money in the bank. along with the 'mirroring' ambiguous T-sentence:

(FN 148) 'John keeps his money in the bank' is true iff John keeps his money in the bank.

Now suppose we have a situation S1 in which one reading of (FN 147) is true and the other reading false. We will then want to say that in S1 (FN 148) also has a true reading and a false reading. But of course we don't want false T-sentences in our truth theory, so we will need to make clear that it is not the false reading of (FN 148). (FN 148) will then have an unambiguous understanding, and when we now consider another situation S2 in which the reading of (FN 147) true in S1 is false, and vice versa, we will find that our unambiguous (FN 148) gives the wrong result. (I have some suspicion that attempts to accommodate vagueness in

this line of argument and suggest that the minimal truth conditions given by the genuine pluralist account are in fact adequate, but before doing so we must understand more fully the range and type of putative ambiguity generated by plurals.

### §3.2.1.1.2.1 Monadic Plurals

We begin our examination on the array of readings generated by the presence of plural referring expressions by considering what I will call *monadic plurally referring sentences* (MPR sentences). MPR sentences are sentences containing only a single plural referring expression, and thus of the form:

(276)  $R \varphi$ 's

for some property  $\varphi$  and some plural referring expression  $R$  which refers to some  $r_1, \dots, r_n$ .<sup>281</sup>

We have so far identified collective and distributive readings of MPR sentences. One might suspect that the collective reading is the distinctively plural reading, while the distributive reading is to be understood as a (possibly infinite) conjunction of singular readings. this suspicion, however, needs (from the point of view of the genuine pluralist) to be resisted, both because it furthers the ambiguity thesis and because it manifests a resurgence of singularism, by insisting that

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a truth theory by a similar mirroring of object language and metalanguage vagueness is subject to a similar worry, although I will not attempt to develop this thought here).

Instead of capturing mirroring through ambiguities, then, truth theories ought to accommodate them by introducing distinct syntactic structures (and hence distinct object language sentences) corresponding to distinct readings. In the case of (FN147), this would amount to introducing two words 'bank' into the lexicon. In general, such considerations provide a further argument for the incorporation of a level of logical form into the syntax. In the particular case of plural reference, we would need some syntactic markers of collectivity and distributiveness.

<sup>281</sup>As noted above, the things referred to need not be finite in number.



at least some uses of plurals ought to be understood in terms of *prior facts* about the behaviour of individuals.

### §3.2.1.1.2.1.1 Beyond Collective and Distributive: Partitions

As a first step toward resisting the privileged plural status of the collective reading, note that the collective and distributive readings do not exhaust the range of available readings of an MPR sentence.

Consider the following example from [Gillion 1987]:

(277) Mozart, Haydn, Gilbert, and Sullivan wrote operas.<sup>282</sup>

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<sup>282</sup>I have here altered the sentence from [Gillion 1987] slightly. The original read:

(FN 149) The men wrote operas  
where 'the men' is taken to denote Mozart, Haydn, Gilbert, and Sullivan. In general, I want to separate two issues too often confused: the readings created by the presence of plural referring expressions, and the quantificational devices through which plurally referring expressions are or are not introduced. Thus, if we have the sentence:

(FN 150) Two men wrote *Principia Mathematica*.  
there are two problems facing us in the semantic analysis. We must (a) explain the collective and distributive readings of the plural referring expression governed by 'two men', and (b) explain the quantificational devices through which an appropriate plural referring expression is introduced into the sentence. Thus note that on the standard quantificational analysis, we get:

(FN 151) [2x: man x](x wrote *Principia Mathematica*)  
which does not introduce a plural referring expression and *a fortiori* does not allow for the collective reading.

It might look as if the standard quantificational apparatus will then always get us the distributive reading. The fact that the collective and distributive readings already arise in cases like:

(272) Russell and Whitehead wrote *Principia Mathematica*.  
in which there is no quantificational apparatus in play, should show that the two issues are indeed orthogonal. However, consideration of more complex cases further illustrates the orthogonality. Consider the sentence:

(FN 152) Two men gave five women eight books.  
There is one reading of this sentence in which there are some two men, some five women, and some eight books such that each of the two men gave the five women (collectively) the eight books (collectively). There is another (distributive) reading on which the two men (each) gave each of the five women the same eight books. Neither of these readings, however, are available through the standard quantificational apparatus, which forces us to choose among:

(FN152-RQ1) [2x: man x][5y: woman y][8z: book z]gave x,y,z  
(FN152-RQ2) [5y: woman y][2x: man x][8z: book z]gave x,y,z  
(FN152-RQ3) [5y: woman y][8z: book z][2x: man x]gave x,y,z  
(FN152-RQ4) [2x: man x][8z: book z][5y: woman y]gave x,y,z  
(FN152-RQ5) [8z: book z][2x: man x][5y: woman y]gave x,y,z  
(FN152-RQ6) [8z: book z][5y: woman y][2x: man x]gave x,y,z

Neither of the following is true:

(278) Gilbert wrote operas

(279) Sullivan wrote operas

so the distributive reading of the sentence does not obtain.<sup>283</sup> But there is no opera on which all four of these men collaborated, so the collective reading of the sentence also fails to obtain. Nevertheless, clearly the sentence in some way correctly describes the world -- in virtue of the following facts:

(280) Mozart wrote operas and Haydn wrote operas and Gilbert and Sullivan wrote operas.<sup>284</sup>

In general, if we an MPR sentence:

(281) R  $\phi$ s

containing a plurally referring expression R referring to some  $r_1, \dots, r_n$ , then there will distinct readings of (281) for every way of *partitioning*  $r_1, \dots, r_n$  into groups. Here we understand *partition* in the following manner:

**(Def. 14)**  $\{P_1, \dots, P_m\}$  is a *partition* of  $\{r_1, \dots, r_n\}$  iff (a)  
for every  $P_i$  in  $\{P_1, \dots, P_m\}$  and every  $x \in P_i$ ,  $x$

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Of these, all but (FN152-RQ1) and (FN152-RQ4) involve either 10, 16, or 80 men -- rather than two -- and (FN152-RQ1) and (FN152-RQ4) involve more than the desired number of women and books. So the desired distributive reading is unavailable on all six analyses, and the standard quantificational apparatus cannot, even when present, be equated with the presence of a distributive reading. In the end, the array of possible readings of (FN 152) will be quite impressive in volume.

<sup>283</sup>Note here that I carefully refrain from saying that the distributive reading of the sentence is not true. See §3.2.1.1.2.1.2 for discussion of how I propose to understand such readings.

<sup>284</sup>There's an additional complication introduced here by the plural 'operas' -- clearly the sentence could be true if each of Mozart, Haydn, and the Gilbert-and-Sullivan team wrote just one opera. I will take up this issue again shortly when I consider relational claims involving multiple plural referring terms.

$$\in \{r_1, \dots, r_n\}, \text{ (b) } \bigoplus_{i=1}^m P_i = \{r_1, \dots, r_n\}, \text{ and (c) } P_i \cap P_j = \emptyset \text{ for all } i, j \in \{1, \dots, m\}.^{285}$$

So for each distinct partition  $P = \{P_1, \dots, P_m\}$ , there a distinct reading of (281) claiming that  $P_i \varphi s$  for each  $P_i$  in  $P$ . Here the collective reading will correspond to the maximally coarse partition  $R_1 = \{r_1, \dots, r_n\}$ , and the distributive reading will correspond to the maximally fine partition in which  $R_i = \{r_i\}$  for  $i = 1, \dots, n$ .

### §3.2.1.1.2.1.2 The Status of MPRS Readings

I say that there will be distinct *readings* of 'R  $\varphi s$ ' for each partition of  $\{r_1, \dots, r_n\}$ . What is meant here by a reading of a sentence? One answer to this question is given by [Gillion 1987], which takes MPR sentences to be systematically ambiguous and the choice of a partition to provide a particular disambiguation of the sentence. Thus on this view the sentence (277) is multiply ambiguous, and two possible readings of it correspond to the partitions:

(P1)  $\{\{\text{Mozart}\}, \{\text{Haydn}\}, \{\text{Gilbert, Sullivan}\}\}$

(P2)  $\{\{\text{Mozart, Haydn, Gilbert, Sullivan}\}\}$

We can thus distinguish between the *true* (277-1) -- that reading of (277) in which the plural subject has the partition P1 imposed on it -- and the *false* (277-2) -- the reading derived from P2.

In general, on this view, given an MPR sentence 'R  $\varphi s$ ' and partitions  $P^1, \dots, P^m$  of the reference of R, we can give the following (cluster of) truth definition(s):

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<sup>285</sup>Again, this definition extends in the obvious way when the plural reference of R is infinitely plural.

(\*T281) 'R  $\phi$ s'<sup>i</sup> is true iff for partition  $P^i = \{P_1^i, \dots, P_q^i\}$  of  $\{r_1, \dots, r_n\}$ ,  $(\forall j \in \{1, \dots, q\}) ('P_j^i \phi$ s' is true).

(where 'R  $\phi$ s'<sup>i</sup> is that disambiguation of 'R  $\phi$ s' associated with partition  $P^i$ ) I find such a truth definition unhelpful, however, because it makes use of the notion of the truth of sentences of the form ' $P_j^i \phi$ s', where ' $P_j^i$ ' will, in general, be a plural referring expression. It thus relies on a prior understanding of truth conditions for MPR sentences in order to give truth conditions for MPR sentences -- not a very satisfying situation. Furthermore, it's easy to see that in order to get the desired results out of this truth definition, the sentences ' $P_j^i \phi$ s' must all be given a collective reading. This approach to truth for MPR sentences thus gives the collective reading a distinguished status, makes it into the 'real' plural reading from which all other readings are derived. Not only does acknowledging such a genuinely plural reading undermine the singularism which undergirds the opposition to the genuine pluralist account, it also, as will become clear when we come to discuss polyadic plural-referring sentences, creates later difficulties in producing a unified account of the semantic behaviour of plurals.

The genuine pluralist account, on the other hand, takes as the canonical truth conditions for MPR sentences the simple analog of those for singularly referring sentences:

(T281) 'R  $\phi$ s' is true iff R  $\phi$ s.

To do so, of course, is to reject the ambiguity theory favored by [Gillion 1987]. I must then hold that (277) is either straightforwardly true or straightforwardly false; the only plausible option seems to be

that it is true. Given this genuine pluralist analysis, along with the prior observations about partitions, it will be true that:

(T281-1) 'R  $\phi$ s' is true iff for *some* partition  $P^i = \{P_1^i, \dots, P_q^i\}$  of  $\{r_1, \dots, r_n\}$ ,  $(\forall j \in \{1, \dots, q\}) ('P_j^i \phi$ s' is true).

where each ' $P_j^i \phi$ s' is given the collective reading, but this should be seen not as an *analysis* of the truth conditions for 'R  $\phi$ s' but as an interesting relation between 'R  $\phi$ s' and some other (antecedently understood) MPR sentences.

This is not to say that there is nothing to the distinction between collective and distributive readings -- or, more generally, among the readings imposed by various partitions. What we are distinguishing here are ways in which a given MPR sentence can be true. Thus there are many ways Mozart, Haydn, Gilbert, and Sullivan can write operas: by each writing an opera individually, by all collaborating on an opera, or by Mozart and Haydn each individually writing operas while Gilbert and Sullivan collaborate -- but in each case, the four write operas. I will use the term 'readings' to distinguish these various ways in which an MPR sentence can be true.

This sense of a reading of a sentence in terms of a way in which it can be true should be familiar from contexts other than plurals. Thus consider the sentence:

(282) Francis saw a movie by Hitchcock.

We can say here that (282) is made true by Francis having seen *Frenzy*, by Francis having seen *Jamaica Inn*, etc. In saying this we are *not* claiming that (282) is ambiguous or that its truth conditions need to make reference to *Frenzy*, *Jamaica Inn*, or any particular film.

Nevertheless we can sensibly talk here about the various readings of (282) corresponding to various films, in some suitably attenuated sense of a reading. Similarly, and perhaps more closely analogous to the plural case, a sentence such as:

(283) Cary Grant appears either in *Notorious* or in *Rebecca*.

can be true in one way by having Cary Grant appear in *Notorious*, and true in another way by having him appear in *Rebecca*. In general, sets of truth-supporting circumstances can be taken as ways of making a sentence true, and if the sets are prominent or isolated enough, speakers may identify distinct readings of the sentence correlated with those sets.<sup>286</sup>

Obviously there's quite a bit of looseness in my use of 'reading' here. After all, we might equally well say that there are many ways that Mozart, Haydn, Gilbert, and Sullivan can write operas: using a fountain pen, while in Venice, quickly, poorly, etc. It will turn out, however, that there is a theoretical utility to making certain distinctions among the ways an MPR sentence can be true that deal with the groupings of the plural reference.

### **§3.2.1.1.2.1.3 Beyond Partitions: Covers**

While we now have an account of what is meant by a reading of an MPR sentence, we do not yet have an adequate story about what the range of such readings will be (continuing to keep in mind, of course, that what the range of readings is is largely a function of our broader theoretic

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<sup>286</sup>This tendency to take distinct sets of truth-supporting situations as underlying distinct readings of a sentence, coupled with a (better resisted) reflex tendency to take such readings as representing truth-conditionally distinct ambiguities of the original sentence, helps explain [Donnellan 1966]'s desire to isolate distinct referential and attributive readings of claims with definite descriptions, depending on whether the grounds for the claim are singular or general.

and pragmatic interests, and certainly is not a function of the truth conditions of MPR sentences). Partitions alone will not capture all the readings we want to capture. Assume Gilbert had another opera-writing partner Pembleton, and consider the sentence:

(284) Gilbert, Sullivan, and Pembleton wrote operas.

Again, there is some desire to say that this sentence is true. However, no partition of {Gilbert, Sullivan, Pembleton} makes it so. The available partitions are:

(284-P1) {Gilbert}, {Sullivan}, {Pembleton}

(284-P2) {Gilbert, Sullivan}, {Pembleton}

(284-P3) {Gilbert, Pembleton}, {Sullivan}

(284-P4) {Sullivan, Pembleton}, {Gilbert}

(284-P5) {Gilbert, Sullivan, Pembleton}

Since none of the three ever wrote operas individually, (284-P1) through (284-P4) fail to explain the truth of (284). Since all three never collaborated on an opera, (284-P5) is also inadequate. Clearly, what we want is a reading which sees (284) as equivalent to/derived from:

(285) Gilbert and Sullivan wrote operas and Gilbert and Pembleton wrote operas.

In order to capture this reading, we need to drop condition (c) of the definition of a partition, allowing the elements to be pairwise non-disjoint. What we then have is the definition of a cover:

**(Def. 15)**  $\{C_1, \dots, C_m\}$  is a cover of  $\{r_1, \dots, r_n\}$  iff:

(a) for every  $C_i$  in  $\{C_1, \dots, C_m\}$  and every  $x \in C_i$ ,

$x \in \{r_1, \dots, r_n\}$ , and

(b)  $\bigcup_{i=1}^m C_i = \{r_1, \dots, r_n\}$

We then similarly hold that for each cover  $C = \{C_1, \dots, C_m\}$  of  $\{r_1, \dots, r_m\}$ , there is a reading of 'R  $\phi$ s' claiming that 'C<sub>i</sub>  $\phi$ s' is true for each C<sub>i</sub> in C. The desired reading of (284) would then be associated with the following cover:

(284-C1)  $\{\{Gilbert, Sullivan\}, \{Gilbert, Pembleton\}\}$

Clearly the cover condition set out in definition 14 is as weak as can be permitted. Clause (a) is an immediate consequence of what we might call the Sufficiency Principle:

**(Sufficiency)** In order for a claim of the form 'R  $\phi$ s' to be true, the actions or conditions of the referents of 'R' must be sufficient to guarantee or bring about that  $\phi$ .

The Sufficiency Principle thus rules out the truth of:

(286) Gilbert wrote operas

on the grounds that the actions of Gilbert (alone) are insufficient to bring about the writing of an opera. Without clause (a), the Sufficiency Principle will be violated, for when the individuals actually given by the cover do not in themselves act in such a way as to guarantee the truth of the MPR sentence, we will be able to choose some expanded pseudo-cover, violating clause (a), which would, under a definition lacking this clause, be sufficient for the truth of the MPR sentence. Thus, for example, the existence of the pseudo-cover consisting of Gilbert and Sullivan would suffice, in the absence of the Sufficiency Principle and clause (a), to make (286) true.

Weakening or rejecting clause (b), on the other hand, would be to allow the truth of:

(287) Gilbert, Sullivan, and Socrates wrote operas



on the grounds that that same pseudo-cover (now failing to cover the given plural reference) wrote operas. Here we would have a violation of an intuitively appealing principle of Involvement:

**(Involvement)** If R is a plural referring expression such that 'R  $\phi$ s' is true, then all of the objects to which R refers must be involved in  $\phi$ ing.

This principle is, of course, as vague as the notion of involvement to which it appeals, but I hope that it's clear that it's onto something correct. Thus:

(287) John, Albert, and Fred pushed a truck up the hill.

can't be true unless each of John, Albert, and Fred were involved in the pushing. Note that, because of the possibility of non-distributive readings, we cannot require that each object referred to by R actually  $\phi$ .

We can then state the following formal analog to the Involvement Principle:

**(Formal Involvement)** If R is a plural referring expression such that 'R  $\phi$ s' is true, then any reading of 'R  $\phi$ s' which views it as a conjunction of smaller MPR sentences must be such that, for any object x in the reference of R, there is some conjunct in that conjunction such that x is in the reference of the subject of that conjunct.

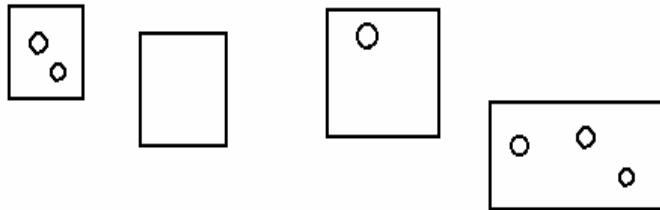
Condition (b) in the definition of a cover is then a consequence of the Formal Involvement Principle. The Formal Involvement Principle, of course, depends on the successful implementation of the (plain) Involvement Principle on the level of the (collectively read) conjuncts,

and thus cannot serve to eliminate the vagueness of the notion of involvement.

While the Involvement Principle may have considerable intuitive appeal, there are some difficult cases. [Scha 1984] offers a putative counterexample to the principle. Consider the sentence:

(288) The rectangles contain the circles.

in conjunction with the following diagram:



There is, I think, an intuitive plausibility to the claim that (288) is true of this diagram. However, it clearly is not true that each of the rectangles is involved in the containing of the circles. Do we here have a violation of Involvement?

Certainly if we use the maximally fine minimal cover on the rectangles we have a problem, since the empty rectangle will then get its own element in that cover, and there will be no circle or circles that it contains. But what if we use the maximally coarse cover, and simply hold that the rectangles (collectively) contain the circles (either distributively or collectively)?

The question here is whether the empty rectangle is really involved in the containing of the circles which is performed by the rectangles en masse. I suspect that what we see here is an idiosyncrasy of the verb 'contain'. Containment claims seem to be evaluated by considering all of the containing objects as forming a (spatially

disjoint) container, which either does or does not hold the relevant objects. Thus the emptiness of one rectangle is no more relevant to the involvement of that rectangle in the containing than the emptiness of the lower half of the third rectangle is to its involvement in the containing.

Note that some other claims about isomorphically structured situations are more sensitive to the 'uninvolved' object. Thus the following appear false:

(289) Homer and Shakespeare wrote the Iliad and the Odyssey.

(290) Clinton, Reagan, and Perot won presidential elections.

In these cases, there is no plausible sense in which the writing or the winning can be expanded to include the extraneous objects. Compare these cases with:

(291) The U.S.A. and Nigeria are wealthier than Egypt and Peru.

(292) Kripke and I can outwit any three linguists.

which can be true even if Nigeria contributes no wealth to the collective subject and even if Kripke does all the outwitting. The states or processes of being wealthier than and outwitting seem more open to a purely additive reading. Other verbs are somewhat ambivalent in their willingness to take on freeloaders. Consider:

(293) The rocks crushed the goats.

when half the rocks missed the goats entirely. True or false?

### **§3.2.1.1.2.1.3.1 Truth-Conditional Considerations on Minimal Covers**

We might wonder (as does [Gillion 1987]) if covers are too plentiful to provide the right account of the available readings of plural sentences.

For example, consider the following two covers of Gilbert, Sullivan, and Pembleton:

(284-C1) {{Gilbert, Sullivan}, {Gilbert, Pembleton}}

(284-C2) {{Gilbert, Sullivan}, {Gilbert, Pembleton}, {Sullivan, Pembleton}}

and modify our mock history so that Sullivan and Pembleton also collaborated on an opera. Now, do (284-C1) and (284-C2) lead to different readings of (284)? Much here depends on what we mean by a 'reading'.

If, like [Gillion 1987], we take ourselves to be providing truth conditions for different senses of an ambiguous sentence when analyzing a sentence like (284), we must decide whether (284-C1) and (284-C2) isolate truth-conditionally differing (homophonous) sentences. Formally, the two covers do provide distinct truth conditions:

(285) Gilbert and Sullivan wrote operas and Gilbert and Pembleton wrote operas. [from (284-C1)]

(294) Gilbert and Sullivan wrote operas and Gilbert and Pembleton wrote operas and Pembleton and Sullivan wrote operas. [from (284-C2)]

-- (285) can be true while (294) is false. There is, however, a simple logical relation between them -- (294) implies (285). Gillion thus holds that (284-C2) does not induce a genuinely *accessible* distinct reading of (284), and introduces the notion of a minimal cover to capture the range of distinct readings:

**(Def. 16)**  $\{C_1, \dots, C_m\}$  is a *minimal cover* of  $\{r_1, \dots, r_n\}$  iff:

- (a) for every  $C_i$  in  $\{C_1, \dots, C_m\}$  and every  $x \in C_i$ ,  
 $x \in \{r_1, \dots, r_n\}$ ,

$$(b) \bigoplus_{i=1}^m C_i = \{r_1, \dots, r_n\}, \text{ and}$$

$$(c) (\neg \exists C \subseteq \{C_1, \dots, C_m\}) \text{ (C is a cover of } \\ \{r_1, \dots, r_n\})$$

Distinct readings of a plural reference sentence are then given by the minimal covers of the reference of the plurally referential term in the sentence.<sup>287</sup> In the case we are considering, cover (284-C2) is not minimal, since it contains (284-C1) as a proper subset, and thus does not capture a distinct reading of (284).

Why should we believe that the putative readings corresponding to non-minimal covers are not actually accessible? Gillion is regrettably incomplete on this issue. He seems to take it as sufficient to illustrate this inaccessibility to show that (284), under the cover (284-C2), is equivalent to the conjunction of (284) under the covers (284-C1) and:

$$(284-C3) \{\{\text{Gilbert, Sullivan}\}, \{\text{Sullivan, Pembleton}\}\}$$

But surely this is insufficient -- there is nothing to prevent us from having more readings for a sentence than are necessary to cover the logically available space. As a simple example, note that there are two readings available of:

$$(295) \text{ Every fan of cinema has seen every Hitchcock film.}$$

corresponding to the two scope arrangements for the universal quantifiers, even though the two readings are logically equivalent.

What we need to show is that, in fact, people don't see a distinct reading of (284) corresponding to the cover (284-C2). This strikes me as

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<sup>287</sup>Note that, on the minimal cover condition for distinct readings, any two readings of an MPR sentence are logically independent.

not completely implausible. For some evidence of it, consider the negation of (284):

(296) Gilbert, Sullivan, and Pembleton did not write operas.<sup>288</sup>

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<sup>288</sup>The interpretation and status of negated MPR sentences are tricky matters. On the genuine pluralist account, although there are many ways in which an MPR sentence can be true, there is only one way that it can be false -- namely, by all of the ways in which it can be true failing to obtain. But there does seem to be some intuitive force to saying that a sentence like:

(FN 153) Mozart and Haydn didn't write operas.

while false in one sense, is true in another sense, since they never wrote an opera *together*. I find that the temptation to acknowledge this reading is stronger than the corresponding temptation to acknowledge that there is a sense in which:

(FN 154) Mozart and Haydn wrote operas.

is false because they never collaborated, leading me to suspect that it is the presence of the negation which creates the difficulty here. Consider also that some writers (see, e.g., [Loebner 1987], [Lapin 1989], [Lappin and Francez 1994]) take the truth conditions for the negation of an MPR sentence 'R  $\phi$ s', at least on the distributive reading, to be given by:

(FN T(-281)) ' $\neg$  (R  $\phi$ s)' is true iff  $r_i$  does not  $\phi$ , for each  $r_i$  in the reference of R.

They would thus take (FN 153) to be true iff:

(FN 155) Mozart did not write operas and Haydn did not write operas.

Again, it is tempting to hear (FN 153) in this way, but this approach to negating MPR sentences has the disadvantage of falsifying the law of the excluded middle, since we can easily have:

(FN 156) Mozart and Haydn wrote operas or Mozart and Haydn didn't write operas.

just in case Mozart but not Haydn wrote operas. Also again, there is no temptation to hear (FN 154) as true iff Mozart wrote operas or Haydn wrote operas -- indicating further that it is the negation which creates the difficulties.

I offer two diagnostic suggestions. One is that this is a pragmatic, rather than semantic, interpretation. People are simply more attuned to ways in which a sentence can be true than ways in which it can be false. A desire to read (FN 153) as true, combined with an ability to distinguish ways in which (FN 154) can be true, leads them to find a way to read it as true -- a way which is not actually endorsed by the semantics. On this view, the reading of (FN 153) suggested above corresponds closely to people's ability to 'deny':

(FN 157) There's a man in the corner drinking wine.

(accompanied with a gesture toward one particular individual) with:

(FN 158) No, Saul's drinking water.

even though there may be another man in the corner drinking wine. There are many ways for the quantified sentence to be true, and if one of these ways is made particularly prominent, denial of the sentence may be equated with denial of that way. It is for this reason that examination of negated MPR sentences is a useful tool for determining what readings people tend to see of the corresponding unnegated MPR sentence.

I can also provide some hints at a semantic solution to the problem. My later remarks on  $\lambda$ -abstraction and complex distribution

According to Gillion, (296), like (284), will be multiply ambiguous, with one (false) reading for each (true) reading of (284). If, for example, Gilbert, Sullivan, and Pembleton had done all of their opera writing as a group, then there would be one sense in which (296) was true -- the sense corresponding to the false (fully) distributive reading of (284) associated with the minimal cover  $\{\{\text{Gilbert}\}, \{\text{Sullivan}\}, \{\text{Pembleton}\}\}$  -- and another sense in which (296) was false -- the sense corresponding to the true (fully) collective reading of (284). The relevant question, then, is whether we can isolate, corresponding to the covers (284-C1) and (284-C2), false senses of (296).

Making these distinctions soon becomes difficult at best. If Gilbert, Sullivan, and Pembleton all did their opera writing alone, one can perhaps see a sense in which (296) is true because they never collectively wrote an opera. To see as well a sense in which it is true because there were no two operas, one written by Gilbert and Sullivan and one written by Gilbert and Pembleton, is much harder. But surely to see yet another sense in which (296) would be true, this time because, while those two operas did exist, there was no third opera written by Sullivan and Pembleton, is too much to ask. But if that reading is unavailable in (296), then (284) in turn has no distinct reading corresponding to (284-C2). Considerations like these, I take it, are

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(see §3.3.2.2.3.3) implicitly provide one method for capturing this reading of 'Mozart and Haydn didn't write operas' while allowing for another reading which satisfies the law of the excluded middle. Here the presence of a negation operator is crucial, since it allows for the distinction between  $\lambda$ -abstraction of, and distribution over, the core predicate and the negated formula.

behind [Gillion 1987]'s endorsement of the minimal cover condition on distinct readings.

Note, in closing, that to deny that there is a distinct reading of (284) corresponding to the non-minimal cover (284-C2) is not (on Gillion's view or mine) to hold that (284) is not true should the opera-writing facts be as described by (284-C2). The existence of a reading corresponding to the minimal cover (284-C1) implies (for Gillion)<sup>289</sup> that (284) is true if Gilbert and Sullivan (collectively) wrote an opera and Gilbert and Pembleton (collectively) wrote an opera, and thus a fortiori that (284) is still true if, in addition, Sullivan and Pembleton (collectively) wrote an opera. What is denied (again, by Gillion) is that (284) is true (on any reading) *if and only if* the facts are as described by (284-C2).

### **§3.2.1.1.2.1.3.2 A Qualified Endorsement of Minimal Covers**

On the genuine pluralist account, the question of whether the minimal cover or the mere cover condition provides the right array of readings for MPR sentences is much less momentous than for Gillion (or for ambiguity theorists in general). If one believes that one is providing the truth conditions for possible disambiguations of a sentence, then one presumably believes that there is some fairly definite answer about how many ways there really are of disambiguating the sentence. If, however, one takes 'readings' of a sentence in the weak sense I set out above, then, since we have already acknowledged that 'ways in which a

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<sup>289</sup>Recall again that for me, (284) is true simply if Gilbert, Sullivan, and Pembleton wrote operas. I recognize no ambiguity in (284); so as long as (284-C2) describes a way in which the three can write operas -- and surely it does, although it may be a redundant or theoretically uninteresting way -- the claim is true.



sentence can be true' multiply quite rapidly, there's no need to draw a firm line.

Since any non-minimal cover can be represented as the union of minimal covers, it follows from the preservation of truth over conjunction that, whether one takes covers or minimal covers to provide 'ways in which' a sentence can be true, one ends up with the same ultimate truth conditions for the sentence. In choosing between the cover condition and the minimal cover condition, then, we are not, as Gillion, is, making a decision which affects the final semantic interpretation of the language. We are merely making an empirical observation about the way in which people tend to distinguish ways for a sentence to be true.

For the most part, it seems correct to claim that people are not attentive to the non-minimal covers as providing distinct readings of MPR sentences. The particular difficulties in seeing a way in which (296) can be true corresponding to the non-minimal cover (284-C2) seems good evidence for the (generic) unavailability of (284-C2) as a (distinct) way in which (284) can be true. However, there are at least two reasons for refusing to be overly dogmatic in our endorsement of the minimal cover condition on distinct readings of MPR sentences. One is that we can explicitly call attention to non-minimal readings of such sentences. Consider a sentence of the form:

(297) Gilbert, Sullivan, and Pembleton wrote operas *in every combination*.

The force of this added clause is to bring to prominence/single out the reading derived from the non-minimal cover:

(284-C5) {{Gilbert, Sullivan, Pembleton}, {Gilbert, Sullivan},  
{Gilbert, Pembleton}, {Sullivan, Pembleton}, {Gilbert},  
{Sullivan}, {Pembleton}}

The other reason not to limit ourselves to minimal covers is that, when we turn to consider sentences containing multiple plural referring expressions, we will find that covers, rather than minimal covers, lead to the most natural account of the range of available readings.

### **§3.2.1.1.2.2 Polyadic Plurals**

While the behaviour of monadic plurally referring sentences has been extensively charted in the literature, there has been relatively little attention given to the further difficulties introduced by sentences which contain more than one plurally referring expression -- sentences which I will call polyadic plurally referring sentences (PPR sentences). While the investigation of PPR sentences will serve to bolster my claim above that the purported ambiguities induced by plural referring expressions should be understood in terms of ways of satisfying univocal truth conditions rather than in terms of a multiplicity of distinct truth conditions, the main reason for investigating the distinct case of polyadic plurals here is to draw out certain features of the interaction of plurals which will prove useful below when we consider questions of the linear ordering of quantifiers.

Consider an arbitrary PPR sentence such as:

(298) John and Albert wrote to Sarah and Mary.

(298) contains two plurally referring expressions: 'John and Albert' and 'Sarah and Mary'. As a start on seeing how these two plurally referring

expressions interact, we will attempt to chart the available readings of (298). At least the following eighteen (logically independent) ways:

(298-1) John wrote to Sarah; Albert wrote to Mary.

(298-2) John wrote to Mary; Albert wrote to Sarah.

(298-3) John wrote to Sarah and Mary (collectively); Albert wrote to Sarah.

(298-4) John wrote to Sarah and Mary (collectively); Albert wrote to Mary.

(298-5) John wrote to Sarah and Mary (collectively); Albert wrote to Sarah and Mary (collectively).

(298-6) John wrote to Sarah; Albert wrote to Sarah and to Mary (individually).

(298-7) John wrote to Sarah; Albert wrote to Sarah and Mary (collectively).

(298-8) John wrote to Mary; Albert wrote to Sarah and Mary (collectively).

(298-9) John and Albert (collectively) wrote to Sarah; John wrote to Mary.

(298-10) John and Albert (collectively) wrote to Sarah; John wrote to Sarah and Mary (collectively).

(298-11) John and Albert (collectively) wrote to Sarah; Albert wrote to Mary.

(298-12) John and Albert (collectively) wrote to Sarah; Albert wrote to Sarah and Mary (collectively).

(298-13) John and Albert (collectively) wrote to Mary; John wrote to Sarah.

(298-14) John and Albert (collectively) wrote to Mary; John wrote to Sarah and Mary (collectively).

(298-15) John and Albert (collectively) wrote to Mary; Albert wrote to Sarah.

(298-16) John and Albert (collectively) wrote to Mary; Albert wrote to Sarah and Mary (collectively).

(298-17) John and Albert (collectively) wrote to Sarah and to Mary (individually).

(298-18) John and Albert (collectively) wrote to Sarah and Mary (collectively).

#### **§3.2.1.1.2.2.1 Principles for Individuating Readings of Polyadic Plurals**

It's not entirely clear that this list is exhaustive. The reader should consider whether the following also represent distinct readings of (298):

(298-19) John wrote to Sarah and to Mary (individually); Albert wrote to Sarah and to Mary (individually).

(298-20) John wrote to Sarah and to Mary (individually); Albert wrote to Mary.

(298-21) John wrote to Sarah and to Mary (individually); Albert wrote to Sarah and Mary (collectively).

(298-22) John and Albert (collectively) wrote to Sarah and Mary (collectively); John wrote to Sarah, Albert wrote to Mary.

Each of these readings is equivalent to some conjunction of (298-1) through (298-18). Nevertheless, at least some of these strike me as natural ways to hear (298). (298-19), if no other, is an available

reading -- what we might call the 'all-to-all' reading. To see this, consider the *prima facie* acceptability of:

(299) It's not true that John and Albert wrote to Sarah and Mary,  
because Albert didn't write to Mary.<sup>290</sup>

### §3.2.1.1.2.2.1.1 Polyadic Plurals and Minimality

We will return to readings (298-19) through (298-22) shortly. First let's determine what sort of algorithm could deliver to us just the readings (298-1) through (298-18).

One would assume that the readings available for a PPRS of the form:

(300)  $R_1 \ \varphi s \ R_2$

would supervene on the readings available for the two plural referring expressions  $R_1$  and  $R_2$ , as those readings are exhibited in the behaviour of the MPR sentences ' $R_1 \ \psi s$ ' and ' $R_2 \ \psi s$ '. We tentatively adopted above the thesis that an MPR sentence has readings corresponding to each minimal cover of the subject's plural reference. Can we then use the minimal covers of {John, Albert} and {Sarah, Mary} to get readings (298-1) through (298-18)? The available minimal covers are:

For {John, Albert}:	(MC1) = {{John, Albert}}
	(MC2) = {{John}, {Albert}}
For {Sarah, Mary}:	(MC3) = {{Sarah, Mary}}
	(MC4) = {{Sarah}, {Mary}}

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<sup>290</sup>Note again that the genuine pluralist account does not actually accept the truth of claims like (299). The pragmatic acceptability of such claims, however, is evidence for the relative prominence of certain readings of PPR sentences. See footnote 287 above for more detailed discussion of negations of sentences containing plurals.

Consider the following first pass at a standard for available readings of a PPRS:

**(n-Minimal Condition)** Given a PPRS ' $P^n (R_1 R_2 \dots R_n)$ ', the sentence:

$$\bigwedge_{mc_1^1, mc_2^2, \dots, mc_n^n | R(i_1, \dots, i_n)} P^n(\overline{mc_1^1}, \dots, \overline{mc_n^n})$$

specifies a reading of the PPRS iff:

- (a)  $MC_1, \dots, MC_n$  are minimal covers of  $\text{ref}(R_1), \dots, \text{ref}(R_n)$  respectively,
- (b)  $mc_j^i \in MC_i$  for all  $i, j$ ,
- (c)  $\overline{mc_j^i}$  is a plural referring expression referring to the elements of  $mc_j^i$ , and
- (d)  $R$  is an  $n$ -total relation on  $\langle \text{ref}(R_1), \dots, \text{ref}(R_n) \rangle$

where an  $n$ -total relation is defined as follows:

**(Def. 17)** A relation  $R$  on  $\langle X_1, \dots, X_n \rangle$  is *n-total* iff, for any  $i \in \{1, \dots, n\}$  and any element  $x$  of  $X_i$ , there exist elements  $x_j \in X_j$  for all  $j = 1, \dots, n, j \neq i$ , such that  $R$  holds of  $\langle x_1, x_2, \dots, x, \dots, x_n \rangle$

Put more informally, the  $n$ -minimal condition for readings of PPR sentences tells one to pick some minimal cover for each plural referring expression in the sentence, and then form a large conjunction of formulae, each of which picks, for each argument place, some element of the minimal cover for the plural referring expression originally occupying that argument place -- and to form it *in such a way that each element of each minimal cover appears in some conjunct*. This last requirement is the  $n$ -total requirement on the relation  $R$ . It is

necessary in order to satisfy the intuitive requirement that every object referred to in a PPRS be involved in the relevant predication.<sup>291</sup>

The n-minimal condition then gives us the following readings of 'John and Albert wrote to Sarah and Mary': (The initial pair of numbers indicates which minimal covers were used to generate the reading)

(1,3-1) John and Albert (collectively) wrote to Sarah and Mary  
(collectively)

(1,4-1) John and Albert (collectively) wrote to Sarah and to Mary  
(individually).

(2,3-1) John wrote to Sarah and Mary (collectively); Albert wrote  
to Sarah and Mary (collectively).

(2,4-1) John wrote to Sarah; Albert wrote to Mary.

(2,4-2) John wrote to Mary; Albert wrote to Sarah.

(2,4-3) John wrote to Mary and to Sarah (individually); Albert  
wrote to Sarah.

(2,4-4) John wrote to Mary and to Sarah (individually); Albert  
wrote to Mary.

(2,4-5) John wrote to Mary; Albert wrote to Sarah and to Mary  
(individually).

(2,4-6) John wrote to Sarah; Albert wrote to Sarah and to Mary  
(individually).

(2,4-7) John wrote to Sarah and to Mary (individually); Albert  
wrote to Sarah and to Mary (individually).

Two observations should be immediate about this list of readings. First, it is richer than the readings (298-1) through (298-18) given above.

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<sup>291</sup>A requirement following from the Involvement Principle.

Readings (2,4-3) through (2,4-7) are not among the original eighteen readings (although (2,4-4) and (2,4-7) correspond to (298-20) and (298-19), respectively). Second, the list of readings is also less rich than the list (298-1) through (298-18). We have in fact captured only five of the eighteen readings given there.

### §3.2.1.1.2.2.1.2 Minimal Minimality

The excess readings (2,4-3) through (2,4-7) are generated due to the lack of a further minimality condition. While we have required that the various plural referring expressions be interpreted via minimal covers, we have not required that the correlations among the elements of those minimal covers themselves be minimal. Thus, for example, when we take the minimal covers:

(MC2) {{John}, {Albert}}

(MC4) {{Sarah}, {Mary}}

we allowed correlations which mapped John to Sarah, Albert to Mary, and Albert to Sarah -- even though this final mapping is not necessary to meet the n-totality condition.<sup>292</sup> What we need, then, is the notion of a *minimally* n-total relation:

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<sup>292</sup>Note that the problem here is *not* that we have mapped one element of the first minimal cover to multiple elements of the second minimal cover. In many cases, this kind of multiplicity will be necessary in order to generate any readings. Thus consider:

(FN 159) John and Albert wrote to Sarah, Mary, and Francine. with the fully discrete minimal covers. We must then find some n-total relation on:

{{John}, {Albert}} X {{Sarah}, {Francine}, {Mary}}

But only a relation which correlates either {John} or {Albert} with multiple elements of the second cover can *possibly* meet the n-totality condition. (Note further that since there are only such correlations, some such correlation must be minimally n-total in the sense defined below.)



**(Def. 18)** A relation  $R$  on  $\langle X_1, \dots, X_n \rangle$  is *minimally n-total*

if: (a)  $R$  is n-total, and (b) for any  $R' \subseteq R$ , if

$R'$  is n-total on  $\langle X_1, \dots, X_n \rangle$ , then  $R' = R$ .

We can then define a 'minimally n-minimal' condition on PPR sentence readings, which modifies the n-minimal condition by requiring a minimally n-total relation among the elements of the minimal covers.

### §3.2.1.1.2.2.1.3 Minimal Maximality

We thus eliminate the extraneous readings, but we are still missing the majority of our original eighteen readings. Consider one of the readings not captured under the n-minimal condition:

(298-11) John and Albert (collectively) wrote to Sarah; Albert  
wrote to Mary.

In order to get this reading, we need to map one element of the minimal cover MC1 to Sarah and one element of the minimal cover MC2 to Mary. There's no particular reason that we stick with one minimal cover for one argument position as we construct a reading. To get the right array of readings, then, we need to make use not of minimal covers of the argument position references, but of (mere) covers. We can then state the following condition of readings:

**Minimally Correlated Cover (MCC) Condition:** Given a PPR

sentence ' $P^n (R_1 R_2 \dots R_n)$ ', the sentence:

$$\bigwedge_{mc_1^1, mc_2^2, \dots, mc_n^n | R(i_1, \dots, i_n)} P^n(\overline{mc_1^1}, \dots, \overline{mc_n^n})$$

specifies a reading of the PPRS iff:

(a)  $MC_1, \dots, MC_n$  are covers of  $\text{ref}(R_1), \dots, \text{ref}(R_n)$

respectively,

(b)  $mc_j^i \in MC_i$  for all  $i, j$ ,

- (c)  $\overline{mc}_j^i$  is a plural referring expression referring to the elements of  $mc_j^i$ , and
- (d) R is a minimally n-total relation on  $\langle \text{ref}(R_1), \dots, \text{ref}(R_n) \rangle$

The MCC condition will then generate exactly the eighteen canonical readings of (16) given above.<sup>293</sup>

The MCC condition thus seems the best candidate for predicting the range of readings available in PPR sentences. It is because the MCC condition makes use of covers of plural referents, rather than minimal covers, that I refrained from fully endorsing the minimal cover analysis of MPR sentences. Again, it seems reasonable that the readings available in PPR sentences should be a function of the readings generated by the same plural referring expressions when appearing monadically. I thus conclude that we are at least potentially sensitive to the full range of covers, but that in general we tend to focus on minimal covers (at least in part because readings expressible using full covers can always be generated using minimal covers). However, we can always explicitly draw attention to some nonminimal reading, as was done above in the MPR case with:

- (297) Gilbert, Sullivan, and Pembleton wrote operas *in every combination*.

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<sup>293</sup>The MCC condition could also be formulated using, rather than select covers of the reference of the argument terms, either (a) always the maximal cover  $\wp(\text{ref}(R_i))$  for each  $i$  (plus a slight weakening of the totality requirement on R), or (b) unions of minimal covers. The cover formulation seems most natural, however, in light of (a) the inclusion of irrelevant cover elements in the use of maximal covers, and (b) the violation of the intuitive unity of the referential term in the use of multiple minimal covers.

Similarly, in the PPR case we can explicitly draw attention to non-minimally correlated readings, as in:

(301) John and Albert *each* wrote letters to *each of* Sarah and Mary.

(which is equivalent to the marginal reading (298-19)). This tendency potentially to recognize the full range of non-minimal readings coupled with a further tendency generally to prefer the minimal readings helps, I think, bolster the view that the range of readings is a pragmatic matter of which sorts of truth-supporting circumstances we are particularly attentive to, rather than a semantic or syntactic matter of which truth conditions are actually available.

### **§3.2.1.1.2.2.2 Some Important Readings of Polyadic Plurals**

Having determined what sorts of readings are made available by the presence of multiple plural referring expressions in a sentence, I now want to isolate a few types of readings which will prove particularly useful in later discussion of cumulative and branching quantifiers.

Given an n-ary PPRS of the form:

(302)  $P^n (R_1 R_2 \dots R_n)$

where each  $R_i$  refers plurally to some  $\{r_i^1, \dots, r_i^{n_i}\}$ , we can distinguish among the many readings of the sentence what I will call the *fundamentally singular* readings of the sentence. These are the readings which are derived from the maximally fine covers (partitions) of all of the plural referring expressions involved -- those covers, that is, which divide the plural reference of each  $R_i$  into single objects:

$\{\{r_i^1\}, \dots, \{r_i^{n_i}\}\}$ . Thus, for example, the reading of:

(298) John and Albert wrote Sarah and Mary

which sees it as:

(298-9) John wrote to Sarah and to Mary (individually); Albert  
wrote to Sarah and to Mary (individually).

is fundamentally singular. Among the fundamentally singular readings, we can then point out some interesting available readings:

- (1) The all-all-...-all readings: on which for each  $j_1 \in \{1, \dots, n_1\}$ , each  $j_2 \in \{1, \dots, n_2\}$ , ..., each  $j_n \in \{1, \dots, n_n\}$ ,  $P^n$  holds of the n-tuple  $\langle r_{j_1}^1, r_{j_2}^2, \dots, r_{j_n}^n \rangle$ . In the binary case, this reduces to those readings in which each object in the reference of the subject bears the relevant relation to every object in the reference of the object.
- (2) The 1-1-...-1 readings: on which for each  $i \in \{1, \dots, n\}$  and each  $j \in \{1, \dots, n_i\}$ , there is exactly one n-tuple of the form  $\langle x_1, \dots, r_i^j, \dots, x_n \rangle$  such that  $P^n$  holds of  $\langle x_1, \dots, r_i^j, \dots, x_n \rangle$  and  $x_k \in \{r_1^k, \dots, r_{n_i}^k\}$  for  $k = 1, \dots, n$ . In the binary case, this reduces to those readings in which the relation imposes a 1-1 mapping from its (relativised)<sup>294</sup> domain to its (relativised) range.
- (3) The each - two-or-more - two-or-more - two-or-more - ... - two-or-more readings: on which for each  $i \in \{1, \dots, n_1\}$ , there are at least two  $f_1^2, f_2^2 \in \{1, \dots, n_2\}$ , at least two  $f_1^3, f_2^3 \in \{1, \dots, n_3\}$ , ..., at least two  $f_1^n, f_2^n \in \{1, \dots, n_n\}$  such that  $P^n$  holds of  $\langle r_i^1, r_{f_{j_1}^2}^2, \dots, r_{f_{j_n}^n}^n \rangle$  for any

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<sup>294</sup>'Relativised' in the sense that P2 here imposes a 1-1 mapping only from  $\text{Domain}(P2) \cap \{r_1^1, \dots, r_{n_1}^1\}$  to  $\text{Range}(P2) \cap \{r_1^2, \dots, r_{n_2}^2\}$ . If, for example, my sentence is:

(FN 160) Albert, Barry, and Charles wrote to Diane, Elizabeth, and Francine

then I am guaranteed that the writing relation imposes a 1-1 mapping from  $\{\text{Albert, Barry, Charles}\}$  to  $\{\text{Diane, Elizabeth, Francine}\}$ , but know nothing about the behaviour of that relation outside these two sets. As this relativization condition is obvious, I omit mention of it in further discussion.

$k_2, \dots, k_n \in \{1, 2\}$ . In the binary case, this reduces to those readings on which, for each object in the domain of the relation, the relation holds between that object and at least two objects in the range of the relation.

- (4)  $p(1)$ -each -  $p(2)$ -more-than- $k_{p(2)}$  -  $p(3)$ -more-than- $k_{p(3)}$  - ... -  $p(n)$ -more-than- $k_{p(n)}$  readings, for arbitrary permutation  $p$  of  $\{1, \dots, n\}$ : on which, for each  $i \in \{1, \dots, n_{p(1)}\}$ , there are at least  $(k_{p(2)} + 1)$   $x_{p(2)} \in \{1, \dots, n_{p(2)}\}$ , at least  $(k_{p(3)} + 1)$   $x_{p(3)} \in \{1, \dots, n_{p(3)}\}$ , ..., at least  $(k_{p(n)} + 1)$   $x_{p(n)} \in \{1, \dots, n_{p(n)}\}$  such that  $P^n$  holds of  $\langle r_{x_1}^1, r_{x_2}^2, \dots, r_i^{p(1)}, \dots, r_{x_n}^n \rangle$ .

- (5)  $\sum_{i,j=1}^n$   $i$ -each -  $j$ -some readings ( $i \neq j$ ): on which, for each  $i, j \in \{1, \dots, n\}$ ,  $i \neq j$ , each  $x \in \{1, \dots, n_i\}$ , there is some  $y \in \{1, \dots, n_j\}$  such that, for all  $k \in \{1, \dots, n\}$ ,  $k \neq i$ ,  $k \neq j$ , there is some  $x_k \in \{1, \dots, n_k\}$  such that  $P^n$  holds of  $\langle r_{x_1}, r_{x_2}, \dots, r_x, \dots, r_y, \dots, r_{x_n} \rangle$ . In the binary case, this reduces to those readings on which each object in the reference of the subject bears the relation to some object in the reference of the object, and each object in the reference of the object bears the relation to some object in the reference of the subject. Note that all fundamentally singular readings are  $\sum_{i,j=1}^n$   $i$ -

each -  $j$ -some readings.

- (6)  $\sum_{i,j=1}^n$   $i$ -each -  $j$ -at-least-half readings ( $i \neq j$ ): on which, for each  $i, j \in \{1, \dots, n\}$ ,  $i \neq j$ , each  $x \in \{1, \dots, n_i\}$ , at least half of the  $y \in \{1, \dots, n_j\}$  are such that for each  $k \in \{1, \dots, n\}$ ,  $k \neq i$ ,  $k \neq j$ , there is some  $x_k \in \{1, \dots, n_k\}$  such that  $P^n$  holds of  $\langle r_{x_1}^1, r_{x_2}^2, \dots, r_x^i, \dots, r_y^j, \dots, r_{x_n}^n \rangle$ . In the binary case, this reduces to those

readings on which each object in the reference of the subject bears the relation to at least half of the objects in the reference of the subject, and *vice versa*.

(7) p(1)-most - p(2)-most - ... - p(n)-most readings, for arbitrary permutation p of  $\{1, \dots, n\}$ : on which most  $x_{p(1)} \in \{1, \dots, n_{p(1)}\}$  are such that most  $x_{p(2)} \in \{1, \dots, n_{p(2)}\}$  are such that ... are such that most  $x_{p(n)} \in \{1, \dots, n_{p(n)}\}$  are such that  $P^n$  holds of  $\langle r_{x_1}^1, r_{x_2}^2, \dots, r_{x_n}^n \rangle$ .

In the binary case, this reduces to those readings on which most of the objects in the reference of the subject bear the relation to most of the objects in the reference of the subject, or *vice versa*.<sup>295</sup>

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<sup>295</sup>Note that, although a p(1)-most - p(2)-most - ... - p(n)-most reading places constraints only on what happens with most of the objects in the reference of the various terms, the fact that any such reading is also a

$\sum_{i,j=1}^n$  i-each - j-some reading means that there are further constraints on what happens with those objects in the minority. Thus, for example, given the sentence:

(FN 161) John, Albert, and Louis wrote to Sarah, Mary, and Francine.

we could satisfy the most(subject)-most(object) constraint on a reading simply by having:

- (a) John write to Sarah and to Mary (individually)
- (b) Albert write to Sarah and to Mary (individually)

and not have Louis write to or Francine be written by anyone. But this situation would not provide a reading of (FN 161), because we already know, due to general features of referential terms, that all of John, Albert, and Louis must be involved in the writing, and all of Sarah, Mary, and Francine must be involved in the being written to. The important observation here is that we can design these sorts of cardinality constraints singling out interesting classes of readings without worrying about whether the conditions built into the constraints are adequate to make these readings intuitively true ones, since we already have a general theory of plural reference which guarantees this.

The constraints, then, act in a way on additional filters on the  $\sum_{i,j=1}^n$  i-each - j-some readings (in the fundamentally singular case).

There are lexical clues which can be added to PPR sentences to prejudice the reading in favor of one or another of these types of reading.<sup>296</sup>

Thus we have sentences such as:

(303) John and Albert (on the one hand), and Sarah and Mary (on the other hand), are *all* writing letters to each other.

(304) John and Albert *each* wrote letters to *at least two of* Mary, Sarah, and Francine.

(305) Cary Grant and Grace Kelly *each* appeared in *at least half of* *To Catch a Thief*, *Rear Window*, and *Suspicion*.

(306) Andrei Tarkovsky and Dusan Makavejev directed *Stalker* and *Sweet Movie* (*respectively*).

which are marked to be read in accordance with (1), (3), (4), and (2) above, respectively. Whether these lexical markers are semantic operators isolating a given reading or terms of conventional implicature raising the pragmatic salience of a reading I leave an open question.

In addition to this dazzling array of fundamentally singular readings, of course, there will be an even broader range of readings which involve (loosely speaking) some collectives. Some of these readings are quite easily accessible, as in:

(307) Clinton and Gore beat Bush and Quayle in 1992

which is made true by virtue of the 'beating' relation holding between Clinton and Gore (collectively) and Bush and Quayle (collectively), or in:

(308) Russell and Whitehead mention Socrates, Plato, and Napoleon in the *Principia Mathematica*.

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<sup>296</sup>As observed by [Sher 1990]. The following sample sentences are derived from examples given there.

which is made true by Russell and Whitehead (collectively) mentioning each of Socrates, Plato, and Napoleon. I will, in later discussion, be less concerned with these collective-type readings than with the fundamentally singular reasons, not because these readings are any less interesting or important, but simply because the literature on partially ordered quantifier prefixes has evolved in such a way that it is the fundamentally singular readings which are important for addressing that literature in the way in which I want to address it. There is a large (here untouched) project of extending the discussion of these topics to include these collective readings as well.

#### **§3.2.1.1.3 The Ontology of Plural Reference**

In the above, I have deployed a substantial array of mathematical tools in order to describe the patterns which MPR and PPR sentences can assume in their predicative behaviour. It is easy, when confronted with such formalism, to assume that the mere use of plural reference commits one to or presupposes a substantial set theory, or the use of a background logic (such as second-order logic) which mirrors the expressive capacities of the language of set theory. However, to make this assumption would be a mistake. When I say:

(272) Russell and Whitehead wrote *Principia Mathematica*.

I commit myself only to the existence of Russell and Whitehead and of *Principia Mathematica*.<sup>297</sup> That I make use of substantial mathematical apparatus in order to clarify what is going on when Russell and

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<sup>297</sup>Plus any other ontological commitments already present in the similar:

(FN 162) Russell wrote *Principia Mathematica*.  
if any. I assume that, in general, the ontological commitment to Russell and Whitehead will amount to a commitment to Russell and to Whitehead, although I don't presuppose this in the metaphysical dogma.



Whitehead write the *Principia* is at least in part a function of the pervading singularist bias of our mathematics, which (in its canonical languages) contains no devices for plural reference and which thus uses the resources of set theory to mimic plurality.

[Lewis 1991] argues forcefully against the assumption that plural quantification is ontologically promiscuous in the manner described above. Lewis suggests that the attribution of laden ontology to plural quantification is not just incorrect but also incoherent:

It is customary to take for granted that plural quantification must really be singular. Plurals, so it is said, are the means whereby ordinal language talks about classes. According to this dogma, he who says that there are the cats can only mean, no matter that he professes nominalism, that there *is* the class of cats. And if you say that there are the non-self-membered classes, you can only mean, never mind that you know better, that there is the class of non-self-membered classes.

If that's what you mean, what you say cannot be true: the supposed class is a member of itself iff it isn't, so there can be no such class. (Likewise there cannot be the set of all non-self-membered sets; and if there are class-like what-nots that are not exactly classes or sets, there cannot be the what-not of all non-self-membered what-nots.) To translate your seemingly true plural quantification into a contradictory singular quantification is to impute error - - grave and hidden error. [65]

The worry here is that the 'ontological lifting' of plural talk into singular talk of a 'plural' entity like a set is doomed to failure because there are logical limitations imposed by Russell's Paradox on the types of such liftings we can perform. We can talk (plurally) about the sets which do not contain themselves as members, but we cannot lift this talk to (singular) talk of a set of all such sets, because we know there can be no such set.

The usual move in response to the combination of (a) our desire to talk about all the non-self-membered sets and (b) our logically-dictated inability to introduce an appropriate set to talk about is to add a new

layer to our ontology -- proper classes, things which are set-like but (roughly speaking) too big to be sets. While we might have worries about the comprehensibility of the notion of a proper class, Lewis grants this notion and pushes a different line of objection. Given the notion of a proper class, we can talk about those proper classes which do not contain themselves as members. But, of course, there can be no class of all non-self-membered classes. So the singularist is forced to make a further extension to his ontology: introducing, say, super-classes which are even larger than classes. Now we talk about all those super-classes which do not contain themselves as members, and so on. It's not just that the singularist is forced into an infinite regress of ontological promiscuity, however:

Let's cut a long story short. *Whatever* class-like things there may be altogether, holding none in reserve, it seems we can truly say that there are those of them that are non-self-members. Maybe the singularist replies that some mystical censor stops us from quantifying over absolutely everything without restriction. Lo, he violates his own stricture in the very act of proclaiming it! [68]<sup>298</sup>

Since the ontological lifting cannot be carried out universally, it should be resisted at the first stage. Quantifying or referring plurally to several individuals is an irreducibly plural act to simple individuals, not a disguised singular act toward a single lifted set-theoretic individual.<sup>299</sup>

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<sup>298</sup>Lewis places this objection within the (from my point of view) unacceptable framework of restricted quantification. According to the anaphoric account, there is some (non-mystical) censor preventing us from quantifying without restriction: namely, the very nature of quantification (as motivated, in part, by the need to meet restrictions imposed by the Generalized Russell's Principle). (We can, of course, still quantify over every *thing*.) Lewis's objection, however, can easily be reconstructed within a framework of restricted quantification. It will suffice, for example, to quantify over everything containing the empty set in its transitive closure.

<sup>299</sup>While I use Lewis's argument here in defense of genuine pluralism, I admit that I would feel more comfortable with his line of defense if it

### §3.2.2 Relativized Reference

The way in which the anaphoric account of variable binding conceives of the restriction of variables can be thought of as a rejection of the Fregean innovation in the *Begriffsschrift* of treating quantifiers as sentential operators and a return to the older logics of Aristotle or Boole and their treatment of quantifiers as noun phrases.<sup>300</sup> While this retreat from Frege allows a greater fidelity to the structure of natural language, it inevitably reintroduces the problems which prompted the move away from the Aristotelian and Boolean systems.

The central problem which Frege solves in the *Begriffsschrift* is the problem of multiply quantified sentences. While the older Aristotelian and Boolean logics were well-equipped to handle single quantified noun phrases, they foundered when it came to the analysis of sentences containing more than one quantifier -- to the analysis of what, from our post-Fregean perspective, we would call non-unary quantifier prefixes. My formal system, in returning to pre-Fregean days, reinherits these difficulties. One of the problems for my system -- the difficulty in explaining the semantic ordering properties of quantifiers imposed by scope relations -- is, I think, a problem revolving around the notion of variable distribution, and will be taken up in great

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were clearer why the attempt to make a singular collection of certain pluralities lead to contradiction (why in the sense of what feature of single objecthood prevented this assimilation, not (of course) why in the sense of what was contradictory about doing so).

<sup>300</sup>It is not entirely correct to say that my approach rejects the idea that quantifiers are sentential operators. The notion of variable restriction is entirely an operation on noun phrases and referring expressions (in the first-order case), and thus is entirely free of the Fregean innovation. Even the notion of variable distribution is in large part to be understood as an operation on noun phrases (more properly, as we will note in footnote 344 below, on chains), but certain elaborations on the theory of variable distribution discussed in §3.3.2.2.3.3 will reveal a sense in which my quantifiers are also sentential operators.

detail below in §3.3.2.4. There is, however, a second manifestation of this class of difficulties which is pertinent to the notion of variable restriction.

The difficulty for variable restriction arises when we consider a sentence with multiple quantified noun phrases such that one noun phrase contains variables bound by the other. Thus consider:

(309) Every film critic owns some movie he admires.

This sentence contains two quantified noun phrases: 'every film critic' and 'some movie he admires'. The second of these contains a variable bound by the first. In traditional restricted quantifier notation, we would capture this fact through a regimentation such as:

(309-RQ) [every x: film critic x][some y: movie y & x admires y](x owns y)

Within the framework of anaphoric binding, however, the variable restriction and the variable distribution must be distinguished. Thus we obtain:

(309-AB) [film critic]<sub>x</sub> [movie x admires]<sub>y</sub>  $\forall x\exists y(x \text{ owns } y)$

Setting aside until §3.3 below details on how to understand the two distributors ' $\forall$ ' and ' $\exists$ ' here, focus on the variable restrictors. It is clear enough what effect the first restrictor '[film critic]<sub>x</sub>' is to have. This restrictor causes the later occurrence of x in 'x owns y' to refer (plurally) to all film critics.<sup>301</sup> The behaviour of the second restrictor, however, is less clear. What does the later occurrence of y come to carry as its semantic value once restricted by '[movie x admires]<sub>y</sub>'?

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<sup>301</sup>A reference which, of course, will then be acted on by the universal distributor.

The difficulty, of course, is that the free 'x' in '[movie x admires]<sub>y</sub>' prevents us from simply identifying some objects which satisfy the restriction and passing those objects on to the restricted variable. What we need instead is a new notion of *relativized reference*. The idea is that a restrictor like '[movie x admires]<sub>y</sub>' creates, through restriction, a new referring expression which refers to different objects relative to the referential behaviour of x. Thus, for example, when in:

(310) x owns y

x refers to Pauline Kael, then y, when restricted by '[movie x admires]<sub>y</sub>', refers to those movies admired by Pauline Kael. When x refers to P. Adams Sitney, then y refers to those movies admired by P. Adams Sitney. As variable restriction and distribution cause x to take on various referents, then, the relativized nature of the reference of y will cause it to adopt the appropriate corresponding referent.

Relativized reference will play a crucial role in the analysis of sentences with cross-clausal anaphora. Consider a sentence like:

(97) Every man who owns a donkey vaccinates it.

Such a sentence will be analyzed as something like:

(97-AB) [man who [donkey]<sub>y</sub>  $\exists y$ ([x owns]<sub>y</sub> y)]<sub>x</sub>  $\forall x$ (x vaccinates y)<sup>302</sup>

The final y will thus have a relativized reference, referring, relative to any choice of x, to those donkeys owned by x. This relativization then allows us to make sense of constructions such as:

(311) Every man who owns a donkey vaccinates it. If it's John, he vaccinates it twice for good measure.

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<sup>302</sup>Where the final y is doubly restricted, both by 'donkey' and by 'x owns y'. It is the second of these two restrictors which creates the relativized reference.

Here we want the final 'it' to refer to those donkeys owned by John. If that 'it' starts off as a term with relativized reference, referring, as above, to those donkeys owned by  $x$  for any choice of  $x$ , then the particular choice of John in the second sentence will relativize the reference appropriately.

I admit to some uncertainty about the notion of relativized reference. Nevertheless, I see no particularly convincing argument against the notion. A certain degree of noncompositionality is introduced into the semantics by relativized reference, since the semantic behaviour of a term which refers in a relativized way will depend on semantic facts coming from outside the scope of the relativized term. However, I will suggest in §3.4.2 below that (a) such noncompositionality is inherent in any quantified language and (b) such noncompositionality is harmless anyway. In part, the worry about relativized reference may derive from a sense that it is hard to see how the semantic behaviour of one term could be in this way dependent on facts from the linguistic context. It's not clear, however, that in the end having one's reference depend on the referential behaviour of terms in the lexical context is any more disturbing than having one's reference depend (as those of indexicals do) on the conversational context in which the singular term is produced. A third source of worry is that these terms of relativized reference ultimately need to be understood as referring to, or by means of, functions, and thus that our quantificational theory is here illicitly importing set-theoretic notions. It's not clear to me, however, that terms with relativized reference must be understood in functional terms. We might instead simply understand them as referring in a relative manner. Of course such

relativity can be described using functional language, but that in itself ought not determine that the semantic explanation of the relativity is via functions. In any case, it's worth noting that while we need terms with relativized reference, we never need to quantify over terms with relativized reference. Thus even if set-theoretic commitments do come in through the notion of relativized reference, the anaphoric account does not immediately collapse to a disguised form of second-order logic.

Relativized reference is connected with restrictive wh-phrases. Relativized reference occurs when the open formula restricting a variable has more than one free variable in it. When we limit variable restrictors just to common nouns, this situation will never arise, since common nouns are always expressions in a single free variable.<sup>303</sup> Restrictors with multiple free variables occur when these common nouns are combined with restrictive wh-phrases, as in:

(312) man who read a book written by him/x

I suspect, therefore, that a completely satisfactory understanding of relativized reference will come hand-in-hand with a completely satisfactory theory of restrictive wh-clauses. I am, however, not convinced that we have such a theory. The best-known work on restrictive wh-clauses is that of [Evans 1977, 1977a], which treats them as lambda-extracted predicates. I worry, though, that Evans' account does not adequately explain the apparent similarity between the

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<sup>303</sup>Apparent counterexamples to this claim, mostly centering around common nouns like 'lovers' or 'neighbors' which appear to express a relationship among objects rather a simple property of an object, should be understood as expressions of a single free variable which require that the satisfiers of the expression be plural referring expressions. See footnote 275 above for further discussion of this issue.

wh-phrases of relative clauses and (a) the wh-phrases of questions and (b) the analogous demonstrative th-phrases.<sup>304</sup> Providing a superior replacement for Evans' account, however, lies well beyond the scope of this work. Relativized reference will, for now, have to remain a slight sore spot in my side.

### §3.2.1.3 The Conceptual Priority of Restricted Quantification

*Prima facie*, quantification can be seen either as restricted or unrestricted quantification. If quantification is restricted, it is not *in the nature* of the classical quantifiers ' $\forall$ ' and ' $\exists$ ' -- instances of what I have been calling determiners or distributors -- to determine what is quantified over. Instead, the range of quantification is provided independently of the determiner. If, on the other hand, quantification is unrestricted, then the determiner (which, on the unrestricted view, are the entirety of the quantifier) carries with it a notion of existence which allows it itself to provide the range of quantification.

The anaphoric account of variable binding endorses restricted quantification as the conceptually fundamental notion of quantification. Unrestricted quantification is available, if at all, as a special case of restricted quantification.<sup>305</sup> The purpose of this section is to

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<sup>304</sup>See footnote 196 above for more detailed discussion of these similarities.

<sup>305</sup>If there is some restrictor available which imposes an otiose restriction, then we can imitate unrestricted quantification through the devices of that restrictor. Two plausible candidates are (a) the use of some noun which by its nature holds of everything, such as 'thing' or 'existent', and (b) the use of tautological complex restrictors, such as those of the form ' $Fx \vee \neg Fx$ '.

However, even if unrestricted quantification can thus be reintroduced, it is introduced as a special case of restricted quantification, not as a logical mode of its own. Moreover, it is not entirely obvious that unrestricted quantification can be reintroduced. We can, I think, quite plausibly wonder whether there is any notion of



develop two consequences of the conceptual priority of restricted over unrestricted quantification. First, we will examine the role of quantifiers in a certain conception of metaphysical methodology, arguing that the promotion of restricted quantification forces us to rethink traditional connections between existence and quantification. As a test case for these issues, we will consider how coherently a free logic can be constructed in a system of restricted quantification. Second, we will consider a rather more technical argument suggesting that the essentially restricted nature of quantification shows that certain recent proposals for understanding the nature of quantification are inherently flawed.

Before beginning these two investigations, however, I want to note a technical curiosity of a logic making use of restricted quantifiers, and draw a couple of minor morals from that curiosity. When quantification is taken always to be restricted quantification, there is no longer any need to specify a domain of quantification in the model

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'thing' or 'existent' which is sufficiently broad to play the role we want it to play in reintroducing unrestricted quantification. There are a number of unusual cases (abstract objects; past, future, or merely possible objects; gerrymandered mereological sums; statue-clay type coincidents; Meinongian mere subsistents) which will call for explicit decisions about the range of 'thing' or 'existent'; we will no longer be able to count on our (unrestricted) quantifiers to go out and range over everything independent of our decisions about what everything is to include. (The importance of this shift will depend on the uses to which we put our quantifiers, but it will be particularly pronounced in the use of quantifiers as a tool of ontological investigation; see §3.2.1.3.1 below.)

The use of tautological complex restrictors may evade these worries, but even here the route to unrestricted quantification is not entirely straightforward. If we have anything like a type theory of objects, on which certain predicates can only even potentially be satisfied by certain types of objects, then in the presence of a 'category mistake' neither 'Fx' nor '¬Fx' may be satisfied by a given object. So tautological restrictors can give rise to (simulated) unrestricted quantification only if either (a) we reject the notion of category mistakes or (b) we find some predicate which applies across all categories.

theory. All the semantics requires is extensions (or other appropriate semantic values) for all predicates. These extensions then determine what objects can become values of the restricted variables -- that is, what objects can be quantified over. If there are objects which are not in the extension of any predicate, then those objects simply cannot be talked about in that language. In the absence of a notion of quantification which is independent of any explicit specification of what is to be quantified over, we need not have a ready-to-hand domain to control the behaviour of unrestricted quantifiers.

One consequence of dropping the notion of a domain of quantification from one's formal system is that one can no longer appeal to tacit restrictions of the domain of quantification as an explanation of certain natural language phenomena. Thus, for example, it is common to defend the Russellian analysis of definite descriptions against worries from incomplete descriptions as in:

(313) The book is on the table.

where it might appear that the Russellian analysis would return an unwanted falsity due to the existence of multiple books and tables, by holding that we here tacitly restrict the domain of quantification to things in the room (or some other conversationally salient set of objects), and that in this restricted domain, the definite descriptions can be given the standard Russellian analysis and will pick out, e.g., the unique book and the unique table *in that domain*.<sup>306</sup>

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<sup>306</sup>[Barwise & Etchemendy 1989] use a similar notion of implicit restriction in the domain of quantification in their neo-Austinian analysis of Liar sentences. If my proposal is right, their solution will also have to be abandoned.

In the absence of a notion of domain of quantification, this solution to the problem of incomplete definite descriptions (and underspecified quantificational noun phrases in general) will have to be abandoned.<sup>307</sup> While there are a number of other routes which can be explored in defense of Russell on this issue, it is not my purpose here to canvas them.<sup>308</sup> I want merely to notice that the decision to treat quantification as fundamentally restricted quantification immediately imposes a constraint on the acceptable solutions to the problem.

A second, and more positive, side effect of elimination the notion of a domain of quantification is that a potential objection raised against the Tarskian notion of logical equivalence by [Etchemendy 1990] loses all force. Etchemendy claims that traditional model-theoretic semantics appeals to unmotivated assumptions, which he calls 'cross-term restrictions' that (a) all constants are assigned referents in the domain of quantification, (b) all predicates are assigned extensions in the domain of quantification, and (c) all quantifiers range over the domain of quantification. On my semantics, no such assumptions are

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<sup>307</sup>One might think that it is not such a bad thing anyway that it be abandoned, given sentences of the form:

(FN 163) Take that book from the table and put it on the other table.

which, under a Russellian analysis, come out to be logical falsehoods no matter how the domain of quantification is tacitly restricted.

<sup>308</sup>Among the possible solutions are (a) treating the incomplete definite descriptions as elliptical for complete descriptions (perhaps appealing to demonstrative completions to avoid worries about the choice of appropriate completion raised by [Wettstein 1981]), (b) treating the sentences with incomplete descriptions as themselves (semantically) false but as convenient tools for implicating some further (typically singular) true proposition, and (c) treating such sentences as imbedded within an implicit pretense that there exists (anywhere) but a single table and a single book (I am indebted to Mark Crimmins for discussion on this last option).

necessary, so Etchemendy's objection loses whatever dubious force it originally possessed.<sup>309</sup>

### §3.2.1.3.1 Metaontology and Free Logic

[Quine 1950] and [Davidson 1976] have each suggested that we can root out the ontological commitments of our beliefs and theories by subjecting the language in which those beliefs and theories are stated to rigorous analysis in some privileged logical system, and then treating quantifiers in that system as the markers of ontological commitment and determining what objects must be included in the domain of quantification in order to make the analyzed sentences true.

Regardless of what one thinks of the general plausibility of this metaphysical methodology<sup>310</sup>, it is clear that the details must be

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<sup>309</sup>Note, however, that on my system, inferences such as:

(FN 164) Abraham Lincoln had a beard. Therefore, something had a beard.

(FN 165) Some dog is barking. Therefore, something is barking. will not be (structurally) valid in the absence of further specifications that Abraham Lincoln is a thing and that all dogs are things. Presumably it will be a constitutive fact about the notion of thinghood that these two claims hold true.

<sup>310</sup>Obviously one's choice of privileged logical system will greatly influence the ontological conclusions one reaches from Quine's standard of commitment. Thus, for example, the commitments to be incurred from:

(FN 166) Necessarily, I am human.

will depend on whether the privileged logic can contain modal operators:

(FN 167) (I am tired)

in which case the commitment will be only to myself (via, in Quine's case, regimentation of proper names into appropriate 'Socratizer'-type predicates coupled with existential quantifiers) or whether the privileged logic must be wholly extensional:

(FN 168)  $(\forall x)(\text{World } x \rightarrow \text{I am tired at } x)$

in which case commitment also runs to possible worlds. Similarly the commitments to be incurred from:

(FN 169) Most philosophers know logic.

will depend on whether the privileged logic can contain generalized quantifiers:

(FN 170) [most  $x$ : philosopher  $x$ ]  $x$  knows logic

in which case commitment only extends to philosophers, or whether only the classical quantifiers are to be permitted:

(FN 171)  $(\exists f)(\forall x)(\neg \text{philosopher } x \rightarrow (\exists y)(\forall z)((\text{philosopher } z \ \& \ f(x) = z) \leftrightarrow z=y)) \ \& \ \neg(\exists f)(\forall x)(\text{philosopher } x \rightarrow (\exists y)(\forall z)((\neg \text{philosopher } z \ \& \ f(x) = z) \leftrightarrow z=y))$

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in which case there is also commitment to functions.

One can in fact see that with sufficient strengthening of the privileged logic, all ontological commitments can be avoided. The general strategy here will be to add sentential operators wherever objects seem to be called for, replacing (say):

(FN 172) Nixon resigned over Watergate.

with:

(FN 173) It resigned over Watergate nixononly.

and:

(FN 174) Someone knows what the answer is.

with:

(FN 175) It is known what the answer is existentially.

Thus any success which Quine's standard of commitment can have is dependent on there being a clear choice of privileged logic, and it is clearly incumbent on Quine to motivate some particular choice. While Quine does *have* a particular choice -- classical logic -- it is not at all clear that he has adequate motivation for that choice.

Davidson's standard of ontological commitment is somewhat more subtle than Quine's, although similar in spirit. The idea here is to look at what sorts of entities are needed in stating a truth theory for an object language. Here there is at least the hope that constraints from elsewhere in the theory on what counts as a good truth theory -- requirements that, say, the theory be compositional, follow object language syntax, or issue in homophonic T-sentences -- may block certain undesirable logics from the permissible tools of ontological investigation.

Nevertheless, there are serious worries about the Davidsonian account. On one understanding of the proposal, it is hard to see how we could get useful information out of the method without begging the question. Let's say that we are attempting to determine whether we ought to countenance facts and electrons in our ontology. We know that our T-theory will give rise to the following theorems:

(FN 176) 'There are facts' is true iff there are facts.

(FN 177) 'There are electrons' is true iff there are electrons.

But it seems that any conclusions about whether we need to mention electrons or facts in our theory depends on first knowing whether there are electrons (in which case both sides of the biconditional of (FN 177) are true and we need to mention electrons) and there are facts (in which case both sides of the biconditional of (FN 176) are true and we need to mention facts) or whether, on the other hand, there are no electrons (in which case both sides of the biconditional (FN 177) are false and we do not need to mention electrons) and there are no facts. Clearly a methodology which tells us that we are committed to the existence of facts if and only if we believe that there are facts is not very useful.

We might more sympathetically interpret Davidson as holding that T-theory construction reveals ontological commitments in a coarser sense. On this reading, the view is that since satisfaction is the only world-word relation required by a T-theory (or at least Davidson claims that it will be the only needed such relationship) and since open sentences are satisfied by objects (as opposed to, say, facts or states of affairs) we conclude that we are ontologically committed only to objects (actually, under a standard Tarskian construction we will be committed to infinite sequences of objects). Of course, this method will not tell us which particular objects or types of objects we are committed to, but it will at least narrow down the range of entities needed.

The worry here, however, is that this approach depends on an untenable type-hierarchy of entities. The coarser interpretation of Davidson will yield useful results only if, say, objects and facts are

revised once we come to accept that quantification is primarily restricted quantification. Any pretense that it is the classical symbols ' $\forall$ ' and ' $\exists$ ' themselves which are markers of existence must be abandoned once we acknowledge that these quantifiers are merely ranging over objects already provided to them by the restrictors. It is, then, the restrictors -- in the first-order case, predicates -- which provide the mark of ontological commitment, not the quantifiers. While a thorough attempt to recast a neo-Quinean account of ontological commitment within the framework of restricted quantification lies beyond the scope of this work, I want here to give at least some evidence that some such recasting will be necessary by showing that the attempt to separate quantification and ontological commitment which lies at the heart of free logic falters when we move to restricted quantification.

#### **§3.2.1.3.1.1 Three or Four Grades of Free Logic**

The field of free logic was sparked by [Leonard 1956], but subsequent work in the field has gone off in a number of directions. I want here to identify four increasingly strong versions of the thesis of free logic, and show that not all of these versions are compatible with the assumption that quantification is essentially restricted quantification.

The fundamental idea in free logic is that certain existence assumptions are denied. However, there are a number of such assumptions which can be denied, and a number of ways in which they can be denied. The first and weakest brand of free logic which I will identify is what I will call the logic of Minimal Freedom. Minimal Freedom drops the

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of fundamentally different types and if thus facts cannot stand in the satisfaction relation to open sentences. But it is hard to see how this could be true, given that we can talk about and quantify over facts. See §3.2.2.2.2 for further discussion of 'higher-order entities'.

classical assumption that the domain of quantification is always non-empty. Thus in a logic of minimal freedom the following classical inference pattern will fail:

$$(314) (\forall x)\phi \models (\exists x)\phi$$

I take it that few would object to Minimal Freedom. Allowing empty domains of quantification seems to introduce no semantic oddities, and the classical assumption that the domain of quantification is always non-empty is probably best seen as a simplifying assumption allowing more straightforward statement of the rules of universal instantiation and existential generalization.<sup>311</sup> Certainly no interesting consequences for ontology or ontological investigation seem to follow from Minimal Freedom.

The next strongest version of free logic I will identify is what I will call Fregean Freedom. On Fregean Freedom, we drop the classical assumption that all names have referents. Thus the interpretation function associated with a model is allowed to be a partial function, and certain terms can fail to be assigned a referent.

Given Fregean Freedom, there is a further decision to be made about the truth value of claims involving empty names. [Lambert 1991] distinguishes negative free logics, positive free logics, and neuter free logics. A *negative free logic* holds that any sentence which contains an empty name is false. A *positive free logic* holds that at

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<sup>311</sup>Of course, if one implements Minimal Freedom in a language containing constants, then one will be forced into a system at least as strong as Fregean Freedom, as described below, since in those interpretations in which the domain of quantification is empty, the constants cannot be assigned a referent. Nonetheless, Minimal Freedom remains an option distinct from Fregean Freedom for languages which contain no constants.

least some sentences which contain empty proper names are true.<sup>312</sup>

Finally, a *neuter free logic* holds that no sentence with an empty name receives a truth value at all.<sup>313</sup> My tendency is to think that only a neuter free logic is truly compatible with Fregean Freedom, given an underlying compositionality-driven assumption that all the parts of a sentence must be meaningful in order for that sentence to be meaningful, but this tendency will not bear on the conclusions drawn here.

Lambert also distinguishes between what he calls logics based on a Russellian world picture and logics based on a Meinongian world picture. Under the Russellian world picture, empty names are empty because they fail to have a referent. The logic of Fregean Freedom, then, is based on the Russellian world picture.<sup>314</sup> Under the Meinongian world picture, on the other hand, empty names refer, but they refer (in Meinongian terminology) to objects which subsist rather than exist. Technically,

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<sup>312</sup>The most likely candidates here are negations of atomic sentences with empty names. Thus a proponent of a positive free logic might wish to hold that the sentence:

(FN 178)  $Fa$

in an interpretation in which 'a' is assigned no referent, is false, and then (in order to preserve the classical behaviour of negation) hold that:

(FN 179)  $\neg Fa$

is true with respect to the same interpretation. Note that the fan of a negative free logic will be committed to the (absurd) view that  $Fa$  and  $\neg Fa$  are both false in such an interpretation.

Another candidate for true sentences with empty names under a positive free logic are structural analogs of tautologies in classical logic. Thus fans of supervenience (e.g., [Van Fraassen 1966]) will hold that a sentence like:

(FN 180)  $Fa \vee \neg Fa$

is true with respect to an interpretation in which 'a' is assigned no referent, even though neither  $Fa$  nor  $\neg Fa$  receive a truth value in such an interpretation.

<sup>313</sup>Lambert explicitly allows that some sentences, such as negative existential statements, can be granted a truth value even under a neuter free logic. I would be inclined not to allow these exceptions.

<sup>314</sup>Following the discussion of [Evans 1981], [Evans 1982, ch.1], I take it that this particular feature of the Russellian world picture, as embodied in the Russellian notion of a singular term, was plausibly also part of the Fregean conception of a singular term.



the Meinongian world picture is implemented by distinguishing an outer domain of subsistents from an inner domain of existents, and then taking the quantifiers to range over the inner domain while allowing names to refer in the outer domain.

A logic which allows names to refer to subsistents as well as existents I will call a logic of Meinongian Freedom. However, we can further distinguish mere Meinongian Freedom from what I will call Full Meinongianism. Under Meinongian Freedom, there is<sup>315</sup> an outer domain of objects to which the names in the logic can refer, but no other part of the logic is allowed access to the outer domain. In particular, predicates are not allowed to have extensions in the outer domain. Atomic sentences containing constants referring to subsistents in the outer domain thus either are false or lack a truth value.

In a logic of Full Meinongianism, on the other hand, predicates can have extensions in the outer domain. Subsistents, that is, are permitted to have properties. Thus in a Fully Meinongian logic, atomic sentences with empty names can express true claims. Full Meinongianism is perhaps the dominant position among free logicians. Certainly it is this sort of logic which is best suited for tasks such as making true our everyday utterances about fictional entities, such as:

(315) Sherlock Holmes lives at 221B Baker Street.

(316) Roger O'Thornton climbed down Mount Rushmore.

In a Fully Meinongian logic, we simply add to the outer domain subsistent entities Sherlock Holmes and Roger O'Thornton, and have the predicates 'lives at 221B Baker Street' and 'climbed down Mount

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<sup>315</sup>In a non-existential sense of 'is'.

Rushmore' hold appropriately among those subsistents. Full Meinongianism is also well-suited for those who want to treat definite descriptions as singular terms and who want the referents of definite descriptions of the form ' $(\iota x)\varphi$ ' to be  $\varphi$  even when the description is empty. Thus those who want to hold true:

(317) The largest prime number is prime.

can take the description 'the largest prime number' to refer to a subsistent in the outer domain and place that subsistent in the extension of 'is prime'.

### **§3.2.1.3.1.1.1 The Incompatibility of Full Meinongianism and Restricted Quantification**

We thus have, in increasing order of strength, Minimal Freedom, Fregean Freedom, Meinongian Freedom, and Full Meinongianism. However, it turns out that if we accept that quantification is essentially restricted quantification, then we cannot have freedom so strong as Full Meinongianism.

Recall that in Full Meinongianism we allow constants to refer to subsistents and predicates to be satisfied by (ordered  $n$ -tuples of) subsistents. Technically, then, a model for a Fully Meinongian logic will have an inner domain  $D_I$  and an outer domain  $D_O$ . The interpretation function for a model will map the set of constants into  $D_I \cup D_O$  and the set of  $n$ -place predicates into  $\wp((D_I \cup D_O)^n)$ . However, the goal is to allow quantifiers to range only over the inner ('existent') domain, in order to match the idea that quantification is a mark of existence. When quantification is unrestricted, this goal is easily accomplished. We simply include in our definition of truth-in-a-model clauses such as:

(318) Sequence  $\sigma$  satisfies ' $(\forall\chi)\varphi$ ' if and only if every object  $\alpha$  in  $D_I$  is such that the sequence  $\sigma'$  which differs from  $\sigma$  only in that  $\sigma'(\chi) = \alpha$  satisfies  $\varphi$ .

(319) Sequence  $\sigma$  satisfies ' $(\exists\chi)\varphi$ ' if and only if some object  $\alpha$  in  $D_I$  is such that the sequence  $\sigma'$  which differs from  $\sigma$  only in that  $\sigma'(\chi) = \alpha$  satisfies  $\varphi$ .

Here the range of quantification is explicitly stated to be the inner domain, which will be narrower than the range of potential referents for constants.

However, when quantification is restricted quantification, achieving the free logical divergence between what can be quantified over and what can be talked about is more difficult. In restricted quantification, there need be no independent notion of a domain of quantification. Instead, the restrictors -- in the first-order case, predicates -- provide the objects that the determiners act on. Thus if predicates are allowed to have extensions which extend into the outer domain, quantification will automatically also follow into the outer domain. Thus it will no longer be possible for:

(320)  $Fa \ \& \ Ga$

to be true while:

(321)  $[\text{some } x: Fx] Gx$

is false, since the restrictor ' $Fx$ ' will cause the quantifier to range over all things which satisfy the predicate  $F$ , which will include the referent of ' $a$ ', even if that referent is a mere subsistent.

One can, of course, force the quantification back into the inner domain, either by explicitly altering the rules governing restriction:

(322) A sequence  $\sigma$  satisfies ' $[\forall\chi: \varphi(\chi)]\psi(\chi)$ ' if and only if every sequence  $\sigma'$  differing from  $\sigma$  in at most the  $\chi$  position, satisfying  $\varphi(\chi)$ , and containing an object from  $D_I$  in the  $\chi$  position satisfies  $\psi(\chi)$ .

or by reading all sentences as containing implicit additional restriction by a logical existence predicate to be interpreted as picking out all and only objects in the inner domain:

(323) A sequence  $\sigma$  satisfies ' $[\forall\chi: \varphi(\chi)]\psi(\chi)$ ' if and only if  $\sigma$  satisfies ' $[\forall\chi: \varphi(\chi) \ \& \ E(\chi)]\psi(\chi)$ ', where  $\sigma$  satisfies  $E(\chi)$  if and only if  $\sigma(\chi) \in D_I$ .<sup>316</sup>

Neither of these moves, however, is very satisfying. The first seems extraordinarily ad hoc. The second, even if it is successful, concedes that the notion of existence is not to be found in the notion of quantification per se, but in this existence predicate which inevitably accompanies quantification. However, it is unclear why, once we have a notion of restricted quantification in which predicates provide objects for quantifiers to range over, we should be blocked from using predicates to so provide objects in such a way that subsistents as well as existents are provided.

### **§3.2.1.3.1.1.2 The Instability of Meinongian Freedom and the Familiarity of Fregean Freedom**

I conclude, then, that if quantification is understood as restricted quantification, as it is on my account and as I have argued it ought to be, then a logic of Full Meinongianism is inconsistent. This leaves Meinongian Freedom as the strongest brand of freedom available to the

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<sup>316</sup>Clause (322) will then combine with a standard clause for truth-in-a-model for restricted quantification to give the desired results.

free logician. However, I want now to suggest that mere Meinongian Freedom is an unstable position, and thus that it is not clear that there is any position stronger than Fregean Freedom available for the free logician to retreat to once driven out of Full Meinongianism.

The difficulty with mere Meinongian Freedom is that, since predicates are not allowed to have extensions in the outer domain, the subsistent objects are left unable to have any properties at all.<sup>317</sup> But it is simply unclear how we are to understand the idea that some objects lack all properties. Are we to assume that subsistent objects lack even the property of self-identity? And how are we to individuate subsistents, given that they will lack the property of distinctness from one another? What we have here is a particularly stark version of the Quinean thesis that in the absence of a notion of individuation, there is no notion of individual.<sup>318</sup>

If mere Meinongian Freedom is an untenable position, and if Full Meinongianism is ruled out by the choice of restricted quantification, then the only available brands of free logic are Minimal Freedom and Fregean Freedom. I take it that Minimal Freedom in itself is of little interest, so the only interesting available free logical thesis is that names can be empty -- not by referring to subsistent objects, but by failing to refer at all. Note, however, that Fregean Freedom is a

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<sup>317</sup>This is somewhat less straightforward than it might appear. While it cannot be true for any predicate  $F$  and any constant  $a$  naming a subsistent that  $Fa$ , we might still have it true that  $\neg Fa$  (depending on whether our free logic was positive or neuter). I find the idea of a positive free logic dubious to begin with, and the notion of objects which can have negative but not positive properties little more comprehensible than the notion of objects which can have no properties at all, but most of what I say below depends only on the assumption that subsistents lack positive properties.

<sup>318</sup>To my knowledge, no one has ever endorsed a logic of mere Meinongian Freedom.

trivial consequence of my own semantic system (especially as developed in §2.3.3), and that there is depends not on any peculiar assumptions about the range of semantic options available to constants, but rather on the assumption that constants are universally to be supplanted in favor of free variables, for which the doctrine of Fregean Freedom holds trivially.

### **§3.2.1.3.2 Are Classical Quantifiers Special?**

Frege's *Begriffsschrift*, and other subsequent foundational work in modern logic, enshrined the universal and existential quantifiers as the paradigm, if not the sole, devices of quantification. Starting with [Mostowski 1957], however, this hegemony has been challenged. More recently, the seminal [Barwise & Cooper 1981] has launched a healthy cottage industry in the examination of quantifiers ranging far beyond the classical ' $\forall$ ' and ' $\exists$ ', and it has been a fundamental assumption of this work that these two classical quantifiers do not exhaust the concept of quantification. Despite -- or perhaps because of -- this explosion in quantifiers, there remains a muted worry that classical quantifiers captured some crucial notion of logicity that generalized quantifiers have stretched too far. Although impressive work has been done in using the notion of permutation invariance to ground the logicity of generalized quantifiers,<sup>319</sup> I suspect that anyone who has worked in this area has at some point experienced a momentary worry on seeing ' $\forall$ ' and ' $\exists$ ' teamed up yet again. While I think that generalized quantifiers represent a legitimate extension of the notion of a logical

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<sup>319</sup>Starting with [Lindenbaum & Tarski 1935] and continuing in [Mostowski 1957] and [Lindström 1966]. More recent work pursuing this line includes [Sher 1991] and [Van Bentham 1986].

quantifier, I am not immune to these worries. In this section, I take one special property of the classical quantifiers -- their ability to combine with Boolean sentential connectives to capture the expressive force of certain natural language noun phrases, as in:

(324) All men are tall.

(324-UQ)  $(\forall x)(\text{man } x \rightarrow \text{tall } x)$

and:

(325) Some men are tall.

(325-UQ)  $(\exists x)(\text{man } x \wedge \text{tall } x)$

This ability of classical quantifiers, in addition to distinguishing them from the host of generalized quantifiers, will also prove to threaten the very notion of a genuinely restricted understanding of quantification. We will see, however, that this ability does not herald any deep property distinguishing classical quantifiers. Having seen this, we will proceed to suggest that three recent accounts of quantification -- game-theoretic semantics, discourse representation semantics, and predicate logic with flexibly binding operators -- which crucially exploit this fortuitous ability of the classical quantifiers are called into question as general explanations of the nature of quantification.

#### **§3.2.1.3.2.1 Collapsing to a Connective**

Classical first-order logic employs unrestricted quantification. The quantifier in:

(326)  $(\forall x)(Fx \rightarrow Gx)$

that is, ranges over all the objects in the domain of the model: what is required for the truth of this sentence is that all of those objects be

G if F. An alternative framework for quantification has in recent years come into increasing favor. In this model, quantification is restricted. We thus have formulae such as:

$$(327) [\forall x: Fx] Gx$$

in which the quantifier ranges not over all the objects in the domain, but only over those objects which meet a certain condition -- here the condition of *being F*. What is required for the truth of the sentence is still that all of the objects ranged over -- here all the F objects -- be G.

At first blush, the move from unrestricted to restricted quantification may seem like nothing more than a notation change. After all, there is a simple canonical translation scheme between the two, using the following two rules:

$$(R1) (\forall x)(\phi(x) \rightarrow \psi(x)) \leftarrow \text{translates to } \rightarrow [\forall x: \phi(x)]\psi(x)$$

$$(R2) (\exists x)(\phi(x) \wedge \psi(x)) \leftarrow \text{translates to } \rightarrow [\exists x: \phi(x)]\psi(x)$$

However, it is by now well known that once one adds generalized quantifiers to one's logical system, this neat intertranslatability disappears. The sentence:

$$(328) \text{Most men are tall.}$$

which can easily be expressed in restricted notation once a 'most' quantifier is added to the language:

$$(328\text{-RQ}) [\text{most } x: \text{man } x] \text{tall } x$$

cannot be expressed in a similar unrestricted manner without the use of substantial set-theoretic apparatus.

Call a quantifier  $Q$  a collapsible quantifier if there is some truth functional connective  $\oplus$  such that the following restricted and unrestricted formulae are provably equivalent:



(329-RQ)  $[Qx: \varphi(x)]\psi(x)$

(329-UQ)  $(Qx)(\varphi(x) \oplus \psi(x))$

for any open formulae  $\varphi, \psi$ . As we will make precise and see shortly, it turns out that almost all quantifiers are non-collapsible.

If almost all quantifiers are non-collapsible, some explanation is required for the remarkable coincidence that our initial choices of quantifiers in classical logic turned out to be collapsible. If there is some interesting and distinctive property which those quantifiers have in virtue of which they are collapsible, we might suspect that this property is in fact partially constitutive of being a quantifier. The coherence of generalized quantifiers such as 'most', which would then lack this feature, would thus be called into question. And, of course, if restricted quantification helps in this way to undermine generalized quantifiers, it also helps undermine itself, since in the presence only of the classical quantifiers, there is no point in distinguishing restricted from unrestricted quantification.

On the other hand, if we were to discover that the collapsibility of the classical quantifiers was the consequence of some uninteresting or overly parochial property of these quantifiers, we might suspect that the full class of collapsible and non-collapsible quantifiers provides a better realization of the notion of a quantifier, and consequently that unrestricted representability, rather than being a genuine type of logic, is an amusing if insignificant technical side-effect of certain quantifiers.

I thus want to take a look at what properties of a quantifier make it collapsible, and see what explains the convenient collapsibility of the classical quantifiers. It's worth noting before beginning on this

project that collapsibility is neither necessary nor sufficient for first-order expressibility. Since collapsibility requires that the very same quantifier be used in both the restricted and unrestricted formulae, with only the addition of a suitable sentential connective to the unrestricted formula, there are some quantifiers which create formulae which have first-order equivalents, but none of the appropriate form. Take for example the quantifier 'some but not all'. The sentence:

(330) Some but not all men are tall.

can be given the restricted formalization:

(330-RQ) [some-but-not-all x: man x] tall x

and the unrestricted formalization:

(330-UQ)  $(\exists x)(\text{man } x \wedge \text{tall } x) \wedge (\exists y)(\text{man } y \wedge \neg \text{tall } y)$

but there is no truth-functional connective  $\oplus$  which will give the unrestricted (but non-classical) sentence:

(331) (some-but-not-all x)(man x  $\oplus$  tall x)

the appropriate truth conditions. On the flip side, there are ready examples of quantifiers which, although not expressible using a first-order apparatus, allow collapsing. Both of:

(332-RQ) [many x: Man(x)] Tall(x)

(333-RQ) [prime-number-of x: Man(x)] Tall(x)

translate readily into the unrestricted formulae:

(332-UQ) (many x)(Man(x)  $\wedge$  Tall(x))

(333-UQ) (prime-number-of x)(Man(x)  $\wedge$  Tall(x))

but neither can be expressed using only first-order resources.

### §3.2.1.3.2.2 A Formal Characterization of Collapsibility

We begin by making our terms more precise. Take a quantifier to be a syntactic object  $Q$ , with which is associated a characteristic function  $Q$ . This characteristic function maps from pairs of cardinals to the set  $\{0,1\}$ . Intuitively, the first cardinal marks the number of relevant objects which do satisfy the appropriate condition and the second cardinal marks the number of relevant objects which do not satisfy the condition, and the characteristic function  $Q$  returns 1 if these cardinals have the right properties.<sup>320</sup> We then implement this characteristic function in the truth definition in the following way:

$$(AX24) [Qx: \varphi(x)]\psi(x) \text{ is true in } M \text{ iff } Q(|\underline{\varphi(x)} \cap \underline{\psi(x)}|, |\underline{\varphi(x)} - \underline{\psi(x)}|)=1$$

$$(AX25) (Qx)\varphi(x) \text{ is true in } M \text{ iff } Q(|\underline{\varphi(x)}|, |D-\underline{\varphi(x)}|)=1$$

where underlining indicates the set of all objects satisfying a formula of one free variable, magnitude lines indicate the function from sets to their cardinality, and  $D$  is the domain of the model  $M$ .

Given a two-place truth functional connective  $\oplus$ , we can associate with it a function  $\underline{\oplus}$  from pairs of sets to sets in such a way that:

$$\underline{\oplus}(\underline{\varphi(x)}, \underline{\psi(x)}) = \underline{(\varphi(x) \oplus \psi(x))}$$

Using this notation, we can easily specify the conditions under which a quantifier is collapsible:

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<sup>320</sup>In order to compare the logical properties of restricted and unrestricted quantifiers, I step back here from the anaphoric account of variable binding for a more traditional notion of a quantifier. I also, in defining quantifiers in this manner, place some substantial constraints on the admissible class of quantifiers. I assume, in particular, that quantifiers are all (a) extensional, (b) conservative, and (c) invariant under isomorphisms of the domain (all of these in the sense of [Keenan & Stavi 1983]).

**(Collapsing Condition)** A quantifier  $Q$  is collapsible if there is some connective  $\oplus$  such that for all sets  $X, Y, D$ ,

$$Q(|X \cap Y|, |X - Y|) = Q(|\oplus(X, Y)|, |\oplus(X, (D - Y))|)$$

Unfortunately, I don't see that any immediate insight into the nature of collapsible quantifiers is gained through this condition. It's easy to see from this condition that the two trivial quantifiers, which yield either always a truth or always a falsehood when prefixed to a formula, are collapsible on any connective. For example, the following two sentences are clearly equivalent:

(334) Some number (possibly 0) of men are tall

(335) Some number (possibly 0) of things are men if and only if they are tall.

Other questions, however, are less obvious in their answers. For example, is there a non-trivial quantifier which collapses only to the connective ' $\vee$ '?

We can get an adequate characterization of collapsible quantifiers by considering the class of *monovalent* quantifiers. Call a quantifier  $Q$  1-monovalent if its characteristic function  $Q$  has the following property:

**(Def. 19)**  $Q$  is 1-monovalent if, for all sets  $X, Y, Y'$ ,  $Q(|X|, |Y|) = Q(|X|, |Y'|)$

Similarly, we define 2-monovalence:

**(Def. 20)**  $Q$  is 2-monovalent if, for all sets  $X, X', Y$ ,  $Q(|X|, |Y|) = Q(|X'|, |Y|)$

A quantifier is bivalent if it is neither 1- nor 2-monovalent. The bivalent quantifiers are thus exactly those which genuinely depend on both the extension and the anti-extension of the relevant formula. The

quantifier 'some', for example, is 1-monovalent, since  $\mathbf{some}(|X|, |Y|) = 1$  iff  $|X| \neq \emptyset$ , while 'all' is 2-monovalent, since  $\mathbf{all}(|X|, |Y|) = 1$  iff  $|Y| = \emptyset$ .

Given these definitions, we can first show that all 1-monovalent quantifiers collapse using ' $\wedge$ ' as a connective, and that all 2-monovalent quantifiers collapse using ' $\rightarrow$ ' as a connective.

**Theorem 1:** If  $Q$  is a 1-monovalent quantifier, then the following formulae are always equivalent:

$$[Qx: \varphi(x)]\psi(x)$$

$$(Qx)(\varphi(x) \wedge \psi(x))$$

**Proof:** Since  $Q$  is 1-monovalent, there is a well-defined projection function  $\mathfrak{q}_1$  as follows:

$$\mathfrak{q}_1(|X|) = \mathbf{Q}(|X|, |Y|) \text{ for arbitrary } Y$$

We now have that  $[Qx: \varphi(x)]\psi(x)$  is true in a model  $M = (D, I)$  iff:

$$\mathbf{Q}(|\underline{\varphi(x)} \cap \underline{\psi(x)}|, |\underline{\varphi(x)} \cap (D - \underline{\psi(x)})|) = 1$$

But:

$$\mathbf{Q}(|\underline{\varphi(x)} \cap \underline{\psi(x)}|, |\underline{\varphi(x)} \cap (D - \underline{\psi(x)})|) = 1 \text{ iff}$$

$$\mathfrak{q}_1(|\underline{\varphi(x)} \cap \underline{\psi(x)}|) = 1$$

Similarly, we have  $(Qx)(\varphi(x) \wedge \psi(x))$  is true in  $M$  iff:

$$\mathbf{Q}(|\underline{\varphi(x)} \cap \underline{\psi(x)}|, |D - (\underline{\varphi(x)} \cap \underline{\psi(x)})|) = 1$$

but again:

$$\mathbf{Q}(|\underline{\varphi(x)} \cap \underline{\psi(x)}|, |D - (\underline{\varphi(x)} \cap \underline{\psi(x)})|) = 1 \text{ iff}$$

$$\mathfrak{q}_1(|\underline{\varphi(x)} \cap \underline{\psi(x)}|) = 1$$

Thus  $[Qx: \varphi(x)]\psi(x)$  and  $(Qx)(\varphi(x) \wedge \psi(x))$  are equivalent.

**Theorem 2:** If  $Q$  is a 2-monovalent quantifier, then the following formulae are always equivalent:

$$\begin{aligned} & [Qx: \varphi(x)]\psi(x) \\ & (Qx)(\varphi(x) \rightarrow \psi(x)) \end{aligned}$$

**Proof:** Since  $Q$  is 2-monovalent, there is a well-defined projection function  $\mathfrak{q}_2$  as follows:

$$\mathfrak{q}_2(|Y|) = Q(|X|, |Y|) \text{ for arbitrary } X$$

We now have that  $[Qx: \varphi(x)]\psi(x)$  is true in a model  $M = (D, I)$  iff:

$$Q(|\underline{\varphi(x)} \cap \underline{\psi(x)}|, |\underline{\varphi(x)} \cap (D - \underline{\psi(x)})|) = 1$$

But:

$$\begin{aligned} & Q(|\underline{\varphi(x)} \cap \underline{\psi(x)}|, |\underline{\varphi(x)} \cap (D - \underline{\psi(x)})|) = 1 \text{ iff} \\ & \mathfrak{q}_2(|\underline{\varphi(x)} \cap (D - \underline{\psi(x)})|) = 1 \end{aligned}$$

Similarly, we have  $(Qx)(\varphi(x) \rightarrow \psi(x))$  is true in  $M$  iff:

$$Q(|(D - \underline{\varphi(x)}) \cup \underline{\psi(x)}|, |D - ((D - \underline{\varphi(x)}) \cup \underline{\psi(x)})|) = 1$$

but:

$$\begin{aligned} & Q(|(D - \underline{\varphi(x)}) \cup \underline{\psi(x)}|, |D - ((D - \underline{\varphi(x)}) \cup \underline{\psi(x)})|) = 1 \\ & \text{iff } \mathfrak{q}_2(|D - ((D - \underline{\varphi(x)}) \cup \underline{\psi(x)})|) = 1 \\ & \text{iff } \mathfrak{q}_2(|\underline{\varphi(x)} \cap (D - \underline{\psi(x)})|) = 1 \end{aligned}$$

Thus  $[Qx: \varphi(x)]\psi(x)$  and  $(Qx)(\varphi(x) \rightarrow \psi(x))$  are equivalent.

These two theorems show that monovalence, of either the 1- or 2-variety, is sufficient to show the collapsibility of a quantifier. We now add a theorem which shows that monovalence is also necessary.

**Collapsing Theorem:** If  $Q$  is a bivalent quantifier, then there is no truth functional connective  $\oplus$  such that  $Q$  collapses to  $\oplus$ .<sup>321</sup>

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<sup>321</sup>An equivalent result was independently derived by Keenan in [Keenan 1993]. His surrounding framework and the purposes to which he puts the result, however, are both substantially different.

**Proof:** Take an arbitrary truth-functional connective  $\oplus$ . To show that  $Q$  does not collapse to  $\oplus$ , it suffices to show that there is some model in which the following two formulae are not equivalent:

$$[Qx: Fx]Gx$$

$$(Qx)(Fx \oplus Gx)$$

That is, we need to show that there is some model  $M = (D, I)$  in which:

$$Q(|\underline{Fx} \cap \underline{Gx}|, |\underline{Fx} - \underline{Gx}|) \neq Q(|\underline{\oplus}(F\underline{x}, G\underline{x})|, |D - \underline{\oplus}(F\underline{x}, G\underline{x})|)$$

We first need to deal with one trivial case. We introduce the notion of a degenerate connective:

**(Def. 21)** A two-place truth-functional connective  $\oplus$  is degenerate if ' $\oplus(p, q)$ ' is true iff ' $\oplus(r, s)$ ' is true, for all  $p, q, r, s$ .

A connective is degenerate, then, if it either always yields true or always yields false. We now make the following observation:

**Claim:** If  $\oplus$  is a degenerate connective and  $Q$  is a bivalent quantifier, then  $Q$  does not collapse on  $\oplus$ .

**Proof:** Since  $\oplus$  is degenerate, we have one of two cases:

$$(C1) \quad Q(|\underline{\oplus}(F\underline{x}, G\underline{x})|, |D - \underline{\oplus}(F\underline{x}, G\underline{x})|) = Q(0, |D|)$$

$$(C2) \quad Q(|\underline{\oplus}(F\underline{x}, G\underline{x})|, |D - \underline{\oplus}(F\underline{x}, G\underline{x})|) = Q(|D|, 0)$$

We assume, without loss of generality, that the first case holds.

Since  $Q$  is bivalent, there are  $X, Y, X', Y'$  such that:

$$Q(|X|, |Y|) \neq Q(|X'|, |Y'|).$$

Take  $M$  such that  $|D| > 2 \times \text{Max}(|X|, |Y|, |X'|, |Y'|)$ . We now want to show that we can choose extensions for  $F$  and  $G$  such that:

$$Q(|\underline{Fx} \cap \underline{Gx}|, |\underline{Fx} - \underline{Gx}|) \neq Q(0, |D|).$$

But since  $Q(|X|, |Y|)$  is different from  $Q(|X'|, |Y'|)$ , one of the two must be different from  $Q(0, |D|)$ . Assume it is  $Q(|X|, |Y|)$  that

differs. We now take (as can always be done, since  $|X|, |Y| < 1/2$   
 $|D|$ )  $F$  and  $G$  such that:

$$|\underline{F}x \cap \underline{G}x| = X$$

$$|\underline{F}x - \underline{G}x| = Y$$

and we have a model in which:

$$\mathcal{Q}(|\underline{F}x \cap \underline{G}x|, |\underline{F}x - \underline{G}x|) \neq \mathcal{Q}(|\underline{\oplus}(\underline{F}x, \underline{G}x)|, |D - \underline{\oplus}(\underline{F}x, \underline{G}x)|)$$

so  $\mathcal{Q}$  does not collapse on  $\underline{\oplus}$ .

We thus assume from now on that  $\underline{\oplus}$  is a non-degenerate connective.

Consider the Venn diagram imposed by  $F$  and  $G$  on the universe of a model for our languages. There are four regions in this diagram: (1) Those things in  $F$  but not in  $G$  (i.e.,  $F-G$ ), (2) those things in both  $F$  and  $G$  ( $F \cap G$ ), (3) those things in  $G$  but not in  $F$  ( $G-F$ ), and (4) those things in neither  $F$  nor  $G$  ( $D - (F \cap G)$ ). Call these regions  $r_1$  through  $r_4$ , respectively, and their cardinalities  $R_1$  through  $R_4$ . Given this decomposition of the domain, note that any truth functional connective  $\underline{\oplus}$  has the following property:

**Decomposition Property:** There is some division of the set  $\{1,2,3,4\}$  into mutually disjoint and collectively exhaustive sequences  $i_1, \dots, i_n$  and  $j_1, \dots, j_m$ ,  $0 \leq n, m \leq 4$ , such that  $\mathcal{Q}(|\underline{\oplus}(\underline{F}x, \underline{G}x)|, |D - \underline{\oplus}(\underline{F}x, \underline{G}x)|) = \mathcal{Q}(R_{i_1} + \dots + R_{i_n}, R_{j_1} + \dots + R_{j_m})$ . We then say that the character of  $\underline{\oplus}$ ,  $\text{Char}(\underline{\oplus})$ , is  $\{i_1, \dots, i_n\}$ , and its anti-character,  $\text{Achar}(\underline{\oplus})$ , is  $\{j_1, \dots, j_m\}$ .

The important point here is that each of  $R_1, \dots, R_4$  makes some contribution to the  $\mathcal{Q}$ -value of  $(\mathcal{Q}x)(\underline{F}x \underline{\oplus} \underline{G}x)$ , and that each contributes in only one position of  $\mathcal{Q}$ .

Using this notation, what we need to show is that there is some model  $M = (D, I)$  such that:



$$\mathcal{Q}(R_2, R_1) \neq \mathcal{Q}(\text{Sum}(R_i \mid i \in \text{Char}(\oplus)), \text{Sum}(R_i \mid i \in \text{Achar}(\oplus)))$$

Note here that  $R_3$  and  $R_4$  do not show up on the left side of the inequality and do show up on the right side of the inequality. Call  $\oplus$  left-flexible if either 3 or 4 are in  $\text{Char}(\oplus)$ , right-flexible if either 3 or 4 are in  $\text{Achar}(\oplus)$ .

Assume without loss of generality that  $\oplus$  is left-flexible. We will show that if  $\mathcal{Q}$  is collapsible on  $\oplus$ , then  $\mathcal{Q}$  is 2-monovalent. Pick arbitrary values for  $R_1$  and  $R_2$ . If  $\mathcal{Q}$  is collapsible on  $\oplus$ , then there must be no value of  $R_3$  or  $R_4$  for which:

$$\mathcal{Q}(R_2, R_1) = \mathcal{Q}(\text{Sum}(R_i \mid i \in \text{Char}(\oplus)), \text{Sum}(R_i \mid i \in \text{Achar}(\oplus)))$$

If one thinks of  $\mathcal{Q}$  as imposing a two-leveled step function over the two-dimensional plane formed by two axes of the cardinal numbers, then one sees that this condition entails, given that  $\oplus$  is left-flexible, that there is a ray, starting at an x-value of 0,  $R_1$ ,  $R_2$ , or  $R_1+R_2$  (depending on the particular  $\oplus$ ) at a height of  $R_1+R_2$ ,  $R_2$ ,  $R_1$ , or 0 (respectively) and extending indefinitely, along which  $\mathcal{Q}$  is constant. (If  $\oplus$  is both left- and right-flexible, we can extend this constant region to a rectangle, but a ray suffices.)

In order to show that  $\mathcal{Q}$  is 2-monovalent, we must show that, for arbitrary cardinals  $\kappa$ ,

$$\mathcal{Q}(|X|, \kappa) = \mathcal{Q}(|Y|, \kappa)$$

for all  $X, Y$ .

We consider four separate cases:

(1)  $1, 2 \in \text{Achar}(\oplus)$ . Then, as above, the ray starting at x-value 0 at a height of  $R_1+R_2$  must be constant. Thus pick  $R_1$  and  $R_2$  such that  $R_1 + R_2 = \kappa$ , and we will get the necessary monovalence condition.

(2)  $1 \in \text{Char}(\oplus)$ ,  $2 \in \text{Achar}(\oplus)$ . Set  $R_2 = \kappa$  and  $R_1 = 0$ . We will then have (in the worst case scenario -- assuming  $\oplus$  is only left-flexible):

$$Q(\kappa, 0) = Q(R_3 + R_4, \kappa)$$

It then follows, through suitable choice of  $R_3$  and  $R_4$ , that:

$$Q(|X|, \kappa) = Q(\kappa, 0) = Q(|Y|, \kappa)$$

and the necessary monovalence condition is met.

(3)  $1 \in \text{Achar}(\oplus)$ ,  $2 \in \text{Char}(\oplus)$ . As above, the ray starting at x-value  $R_1$  and at height  $R_2$  must be constant. Thus by taking  $R_1 = 0$  and  $R_2 = \kappa$ , we get the necessary monovalence condition.

(4)  $1, 2 \in \text{Char}(\oplus)$ . Since  $\oplus$  is non-degenerate, it must here be both left- and right-flexible. By setting, then,  $R_1$  and  $R_2$  to 0, and appropriately adjusting  $R_3$  and  $R_4$ , we can obtain the necessary monovalence condition.

### **§3.2.1.3.2.3 Tokenability and Collapsibility**

We can now use this characterization of the collapsible quantifiers to show that almost all quantifiers are non-collapsible. Intuitively, the idea is clear: since both the 1-monovalent and the 2-monovalent quantifiers are projections of bivalent quantifiers, the space of these functions, under a reasonable metric, will be of smaller dimension than the space of all quantifiers, and its measure will thus be 0. We can make these remarks (more) precise by associating with each quantifier function a point in the class-of-all-cardinals size product of copies of the cardinal plane with itself. The appropriate point will be determined by associating with each cardinal  $\kappa$  the point  $(\kappa_1, \kappa_2)$ , where  $\kappa_1$  is the smallest cardinal such that  $Q(\kappa, \kappa_1) = 1$ , and  $\kappa_2$  is the smallest cardinal such that  $Q(\kappa_2, \kappa) = 1$ . Use the infinite series of such points to associate with  $Q$  a point in our product space. Now, using the standard measure for

such a space, consider the subspace of all 1-monovalent quantifiers. Since for any such quantifier  $Q$  and any two cardinals  $\kappa, \kappa'$ , the smallest cardinal  $\kappa_1$  such that  $Q(\kappa_1, \kappa)=1$  will also be the smallest cardinal such that  $Q(\kappa_1, \kappa')=1$ , it follows that any 1-monovalent quantifier essentially carves out a straight line through the product space, and thus that the measure of all such quantifiers is 0. Similarly, the measure of all 2-monovalent quantifiers is also 0.

We now know exactly which quantifiers are collapsible, and in some sense we have an answer to our initial question: the classical quantifiers are collapsible because they are either 1-monovalent ('some') or 2-monovalent ('all'). However, I at least find this answer unhelpful in addressing the philosophical question which motivated the technical results. Is, or is it not, part of the intuitive idea of a quantifier that a quantifier not be bivalent? I lack strong feelings either way on this question, so I want to propose an alternative explanation for the collapsibility of the classical quantifiers.

The classical quantifier 'some' possesses a property which I will call 'tokenability', and the quantifier 'all' the property of 'co-tokenability'. I want to define these two properties, show that the classical quantifiers possess them, and show that possession of these properties is sufficient to ensure monovalence.

Call a quantifier  $Q$  tokenable if, given any model  $M$  and any formula, true in  $M$ , which has the form ' $(Qx)\varphi(x)$ ', there are some objects in the domain of  $M$  -- the tokens for  $(Qx)\varphi(x)$  -- such that these objects are sufficient to ensure the truth of  $(Qx)\varphi(x)$ . Put more technically, we have:

**(Def. 22)**  $Q$  is tokenable if, for any open formula  $\varphi(x)$  and any model  $M = (D, I)$ , if  $(Qx)\varphi(x)$  is true in  $M$  then there is some  $X \subseteq D$  such that  $(Qx)\varphi(x)$  is true in any model  $M' = (D', I')$  in which: (1)  $X \subseteq D'$  and (2)  $I'|_X = I|_X$ .

A quantifier is similarly co-tokenable if there are some tokens which suffice to preserve the falsehood of the formula.

It's easy to see that 'some' is tokenable and 'all' is co-tokenable. Given any formula  $\varphi(x)$ , if  $(\exists x)\varphi(x)$  is true, take as token some object in the model which is  $\varphi$ . No matter what is changed about the model, so long as that object is left untouched,  $(\exists x)\varphi(x)$  will remain true. Similarly, if  $(\forall x)\varphi(x)$  is false, take as token any object which is not  $\varphi$ , and the preservation of that token will ensure the continued falsity of  $(\forall x)\varphi(x)$ .

Finally, we need a slight strengthening of the tokening condition, called uniform tokenability. A quantifier  $Q$  is uniformly tokenable if, whenever  $(Qx)\varphi(x)$  is true in a model  $M$ , the set  $X$  of all objects in  $M$  which satisfy  $\varphi(x)$  can serve as a token set for  $Q$  -- if, that is, there is a uniform method for choosing a group of tokens. It is again easy to see that 'some' is uniformly tokenable and that 'all' is uniformly co-tokenable.

We now use our earlier results to show that uniformly tokenable and co-tokenable quantifiers are not bivalent.

**Claim:** If  $Q$  is uniformly tokenable, then  $Q$  is 1-monovalent.

**Proof:** We first need the following lemma:

**Lemma:** If  $Q$  is uniformly tokenable, and there is in no model a token set for  $(Qx)Fx$  of cardinality  $\kappa$ , then  $Q(\kappa, \lambda) = 0$  for all cardinals  $\lambda$ .

**Proof:** Assume that  $Q(\kappa, \lambda) = 1$  for some  $\lambda$ . Then  $(Qx)Fx$  is true in a model  $M$  such that  $|Fx| = \kappa$  and  $|\neg Fx| = \lambda$ . If  $Q$  is uniformly tokenable, then  $Fx$  is a token set for  $Q$ . But then there is a model  $M$  in which there is a token set of cardinality  $\kappa$ .

Consider the formula  $(Qx)Fx$ . Now pick some arbitrary cardinal  $\kappa$ . There either is or is not some model  $M$  in which  $(Qx)Fx$  has a token set of cardinality  $\kappa$ . If there is no such model, then by the lemma,  $Q(\kappa, \lambda) = 0$  for all choices of  $\lambda$ . If there is such a model  $M$ , call the relevant token set  $T$ . Now construct two models  $M_1 = (D_1, I_1)$  and  $M_2 = (D_2, I_2)$  in the following way: leave everything in  $T$  unchanged. Get rid of the rest of the domain, and define a new domain by adding enough objects to satisfy the following conditions:

$$|D_1 - T| = \lambda$$

$$|D_2 - T| = \lambda'$$

In each case, assign all the new objects to the anti-extension of the predicate  $F$ .

Now, since we have maintained the token set,  $(Qx)Fx$  is true in  $M_1$  and  $M_2$ . Furthermore, since the anti-extension of  $Fx$  in  $M_1$  and  $M_2$  has cardinality of  $\lambda$  and  $\lambda'$ , respectively, it follows that:

$$Q(\kappa, \lambda) = Q(\kappa, \lambda') = 1$$

Thus in either case,  $Q(\kappa, \lambda)$  is constant for all choices of  $\lambda$ , so we see that  $Q$  is 1-monovalent.

A similar proof will show that any uniformly co-tokenable quantifier is 2-monovalent.

I want to suggest that it is the tokenability or co-tokenability of the classical quantifiers which is the interesting explanation of their collapsibility, and that it is of some philosophical interest that this be the source. There are thus two questions to be addressed. First, why is tokenability a more interesting explanation for the convenient collapsibility of the classical quantifiers than monovalence?<sup>322</sup> The answer, I think, is that it is easy to see how tokenability would arise naturally as a feature of first attempts at creating quantifiers, while it is not so easy to see why monovalence would tend to accompany such attempts.

Quantification can be seen as an outgrowth and generalization of reference. One moves from saying:

(336) John is tall

(337) Mary is tall

to saying:

(338) Some people are tall

(339) All people are tall

with the idea that there is something the same about what one is doing in both cases. But note that referential terms are paradigmatically tokenable. In the claim 'John is tall', all one needs to know in order to know that this claim is true is what is going on with John. The behaviour of other objects in the domain is irrelevant to the truth or falsity of this sentence. This tokenability captures what it is to be a referential term. It thus seems natural to me that when attempting to

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<sup>322</sup>Note that if one has negation and the freedom to insert connectives at any stage of formula creation, one is guaranteed to get at least the expressive power of co-tokenable quantifiers along with tokenable quantifiers. I will thus concentrate exclusively on tokenability from here on.

generalize on the notion of reference, the concept of tokenability would remain in play. 'Some people are tall' differs from 'Mary is tall' in that it doesn't pin down a definite token set, but it shares the feature that one can find a group of things such that inspection of them alone is sufficient to guarantee the truth of the sentence.<sup>323</sup> Given that it is understandable why early quantifiers would be tokenable, our technical results build this tokenability into an explanation of the collapsibility of those quantifiers.

The second question to be answered is, why should it be of any interest that it is the tokenability of the classical quantifiers which leads to their collapsibility? I think there are two key factors here. First, tokenability is not a necessary condition for collapsibility. There are plenty of 1-monovalent quantifiers which are not tokenable, such as 'exactly three'. But if it is tokenability, and not monovalence, which is the distinctive feature of the classical quantifiers, then there is not a plausible case to be made from the collapsibility of the classical quantifiers that unrestrictedness is built in to the notion of quantification. The collapsibility of the classical quantifiers is, on this way of looking at it, just an accidental consequence of their tokenability, not a deep feature of the quantifiers.

Second, it seems to me that tokenability is not in fact a feature which we want all of our quantifiers to possess. Whether one thinks of quantifiers as statements about the cardinality of various sets of objects or as predicates of predicates, the move is away from the particular objects which satisfy particular predicates. But tokenability

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<sup>323</sup>On this way of looking at things, the conceptual step forward in the move to quantifiers was separating tokenability from co-tokenability, since referential claims have both properties.

resists this move toward the general, insisting that the quantifier respect the behaviour of particular objects. Genuinely making the transition from object-dependent referential claims to object-independent quantificational claims involves giving up tokenability as a distinctive characteristic of quantifiers, and making this sacrifice removes any claim that unrestricted quantification might have, via the possibility of collapse, to being the true form of logic.

#### **§3.2.1.3.2.4 Three Exploitations of Collapsibility and Tokenability**

I now want to examine three projects for supplying conceptual underpinnings for the logic of quantification. Each of these three -- game-theoretic semantics, discourse representation theory, and predicate logic with flexibly binding operators<sup>324</sup> -- accords poorly with generalized quantifiers, so were any of them accepted as providing the correct philosophical explanation of quantification, the centrality of the classical ' $\forall$ ' and ' $\exists$ ' would be reaffirmed. However, we will discover on examination that each crucially exploits one of the two properties discussed above -- collapsibility to a connective or tokenability -- to make sense of its inner workings. Having argued that neither collapsibility nor tokenability ought to be regarded as essential features of quantification, I thus suggest that none of game-theoretic semantics, discourse representation theory, and predicate logic with flexibly binding operators provides a satisfactorily general explanation of how quantifiers work. To the extent that these approaches function appropriately with the classical quantifiers, and also in some extensions of the classical semantics, their success must be seen either

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<sup>324</sup>See, respectively, [Hintikka 1982], [Kamp 1981], and [Pagin & Westerståhl 1993].



as coincidental or as dependent on an unacknowledged exploitation of an underlying account of which they are merely a special case. I am not concerned here to examine the limited successes of these semantic stories; for now it suffices to show the dependence of these systems on the parochial properties of collapsibility and tokenability.<sup>325</sup>

#### **§3.2.1.3.2.4.1 Predicate Logic With Flexibly Binding Operators**

[Pagin & Westerståhl 1993] develops a modified semantics for quantification intended to capture compositionally some of the troubling examples of cross-clausal anaphora. They set out three key aspects in which their approach to semantics breaks with the classical tradition:

- (i) The variable-binding operators are binary. Besides being well-suited to natural-language quantification, this allows exploitation of the analogy between existential quantification and conjunction, and between universal quantification and implication: in fact, PFO [predicate logic with flexibly binding operators] fuses sentential and variable-binding operators and permits a formulation where the only symbols used, in addition to non-logical symbols, variables, and identity, are  $\perp$ , [, and ].
- (ii) Binding in PFO is unselective, in that all variables which are common to both immediate subformulas of a quantified formula become bound in the quantified formula.
- (iii) The 'binding priority' of PFO is from the outside in: every occurrence of a variable  $x$  occurring in both immediate subformulas  $\varphi$  and  $\psi$  of a quantified formula become bound, regardless of whether that occurrence was free or already bound in  $\varphi$  (or  $\psi$ ) taken by itself; previous bindings are thus in a sense canceled. [90-91]

In Pagin and Westerståhl's interest in the relation between quantifiers and sentential connectives, we already see the first sign of trouble. It remains to spell out the details of that difficulty.

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<sup>325</sup>I will presuppose throughout the subsequent discussion a reasonable familiarity with each of the formal systems being considered. While I will sketch certain features of each relevant to the present considerations, consultation of the original literature will be necessary in order to reconstruct the framework in which my sketches are situated.

In the Pagin and Westerståhl semantics, then, a (binary) quantifier contributes two elements: a quantificational rule and a sentential connective. Thus we have (using square brackets for the universal quantifier and round brackets for the existential quantifier):

$$(340) [\text{man } x, \text{mortal } x] \equiv (\forall x)(\text{man } x \rightarrow \text{mortal } x)$$

$$(341) (\text{man } x, \text{mortal } x) \equiv (\exists x)(\text{man } x \wedge \text{mortal } x)$$

In the trivial case in which the two quantified formulae have no mutual variables available to quantify, the binary quantifier is thus still able to contribute its distinctive sentential connective.

Putting together the pieces, let's consider the Pagin and Westerståhl regimentation and interpretation of a typical donkey sentence such as:

(98) Every man who owns a donkey vaccinates it.

They formalize (98) as:

$$(98\text{-PFO}) [(\text{farmer } x, (\text{donkey } y, \text{owns } x,y)), \text{vaccinates } x,y]$$

Since both  $x$  and  $y$  appear in both of the formulae immediately quantified by the universal quantifier of largest scope, they are both bound by this quantifier to give us (in an amalgam of notations):

$$(342) (\forall x)(\forall y)[(\text{farmer } x, (\text{donkey } y, \text{owns } x,y)) \rightarrow \text{vaccinates } x,y]$$

When we come to the two existential quantifiers, since all the available variables have already been bound (following the 'outside in' binding strategy of PFO), we merely insert the sentential connective associated with the quantifier to obtain:

$$(343) (\forall x)(\forall y)[(\text{farmer } x \wedge \text{donkey } y \wedge \text{owns } x,y) \rightarrow \text{vaccinates } x,y]$$

As is well known, (343) provides the correct truth condition for the donkey sentence (98).

What happens if we try to extend PFO to include generalized quantifiers? The semantic clauses for the existential and universal quantifiers are, respectively:

- (d)  $M, X \models_f (\varphi, \psi) \iff$  there are  $a_1, \dots, a_k \in M$  such that  $M, X \cup \{x_1, \dots, x_k\} \models_f(x_i/a_i) \varphi$  and  $M, X \cup \{x_1, \dots, x_k\} \models_f(x_i/a_i) \psi$ <sup>326</sup>
- (e)  $M, X \models_f [\varphi, \psi] \iff$  for all  $a_1, \dots, a_k \in M$ , if  $M, X \cup \{x_1, \dots, x_k\} \models_f(x_i/a_i) \varphi$  then  $M, X \cup \{x_1, \dots, x_k\} \models_f(x_i/a_i) \psi$

We can with no difficulty construct a clause for, say, 'most' which is reasonably similar to (d) and (e):

- (f)  $M, X \models_f \{\varphi, \psi\} \iff$  most  $a_1, \dots, a_k \in M$  such that  $M, X \cup \{x_1, \dots, x_k\} \models_f(x_i/a_i) \varphi$  are such that  $M, X \cup \{x_1, \dots, x_k\} \models_f(x_i/a_i) \psi$

Here we use the '{' brackets to indicate the binary 'most' quantifier.

In many cases, (f) will function as we would expect. However, because of the unselective binding of PFO, (f) causes problems when either (i)  $\varphi$  and  $\psi$  have no variables in common, or (ii) all of the variables  $\varphi$  and  $\psi$  have in common are bound at a higher level. In such cases, (d) and (e) collapse to their contained sentential connectives ' $\wedge$ ' and ' $\rightarrow$ '. But (f) will result simply in the assertion of  $\psi$ .

What's wrong with that? In standard formulations of generalized quantifier theory, a formula:

- (344)  $[\text{most } x: \varphi(x)]\psi$

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<sup>326</sup>Pagin and Westerståhl's paper throughout mistakenly adds  $\{a_1, \dots, a_k\}$  rather than  $\{x_1, \dots, x_k\}$  to the set  $X$  of marked variables. I have switched to the correct notation here.

where  $\psi$  contains no free occurrences of 'x', is equivalent to  $\psi$ . So isn't PFO just doing exactly what we want? Unfortunately, trouble arises when we consider the behaviour of 'most' quantifiers imbedded in other quantifiers, as driven by the 'outside-in' direction of binding. Thus we have:

(345) Every man who owns most donkeys vaccinates them.

Which will be formalized as:

(345-PFO)  $[(\text{man } x, \{\text{donkey } y, \text{owns } x,y\}), \text{vaccinates } x,y]$

Since both 'x' and 'y' are again bound by the outermost universal quantifier, the innermost 'most' quantifier collapses to a mere assertion of the second of its two quantified formulae, yielding:

(346)  $(\forall x)(\forall y)[(\text{man } x \wedge \text{owns } x,y) \rightarrow \text{vaccinates } x,y]$

which clearly is not an appropriate interpretation. Thus we see that PFO (to the extent that it is supposed to allow us to model the quantificational behaviour of natural language) relies on the assumption that corresponding to each quantifier, there is some sentential connective to which that quantifier can collapse when the quantification is trivial. It is this assumption which causes PFO to be particularly well-adapted to the classical quantifiers; once we abandon the idea that collapsibility is a trait indicative of logicality, we will have no reason to accept (a) that PFO tells us how quantification really works or (b) that PFO gives us reason to prefer the classical over the non-classical quantifiers.

In fact, PFO relies on an even stronger assumption than mere collapsibility. Add to PFO a generalized quantifier which does collapse, such as 'at least two' (symbolized by '| |'), with the obvious semantic clause. Now consider how PFO interprets:

(347) Every man who owns at least two donkeys vaccinates them.

We regiment as:

(347-PFO)  $[(\text{man } x, \text{ |donkey } y, \text{ owns } x,y|), \text{ vaccinates } x,y]$

Again, both  $x$  and  $y$  are bound on the outermost level, so the remaining existential and 'at least two' quantifiers collapse to their associated connectives -- ' $\wedge$ ', in both cases. We thus obtain the interpretation:

(343)  $(\forall x)(\forall y)[(\text{man } x \wedge \text{ donkey } y \wedge \text{ owns } x,y) \rightarrow \text{ vaccinates } x,y]$

But (343) is too strong, requiring that all donkey owners, not just all two-donkey owners, vaccinate. In fact, PFO (under this extension) is unable to distinguish (347) from (98). What PFO really requires, then, in order properly to distinguish quantifiers, is that each quantifier it acknowledges be uniquely collapsible -- collapsible, that is, on some connective on which no other quantifier in the system is collapsible. But as we have seen, any collapsible quantifier collapses either on ' $\wedge$ ' or on ' $\rightarrow$ ', so PFO can contain at most two uniquely collapsing quantifiers.

#### **§3.2.1.3.2.4.2 Discourse Representation Theory**

Discourse Representation Theory (DRT) is, like PFO, designed to accommodate within the structure of the usual variable-binding procedures cases of cross-clausal anaphora. DRT has become a booming industry of late, and I will address only its original formulation in [Kamp 1981].

DRT, unlike PFO, has no need for collapsibility in its quantifiers, but, as we shall see, it does have a built-in requirement for tokenability. Tokens clearly play a prominent role in DRT, as we associate with each sentence a discourse representation structure (DRS)

which incorporates token objects associated with names and existential quantifiers in the target sentence. Thus, for example, the discourse:

(348) Pedro owns a donkey. He beats it.

is associated with the DRS consisting of the following two discourse representations (from [Kamp 1981, 10]):

DR1(348):    u        v  
              .  
              .  
              Pedro owns a donkey  
              u = Pedro  
              u owns a donkey  
              donkey(v)  
              u owns v

DR2(348):    u        v  
              .  
              .  
              Pedro owns a donkey  
              u = Pedro  
              u owns a donkey  
              donkey(v)  
              u owns v  
              He beats it  
              u beats it  
              u beats v

Here the unspecified object *v* stands in as a token for the quantifier 'a donkey', and (348) will be true if there is some way of imbedding the DRS into the relevant model -- if, that is, some appropriate token can be found.

Since only the existential quantifier, of the two classical quantifiers, is tokenable, DRT is forced to provide a separate treatment of the universal quantifier. Thus, for example, the sentence:

(349) Every widow admires Pedro.

is associated with the three-part DRS:

DR1(349):   x       u  
              .       .  
              Every widow admires Pedro

DR2(349):   x  
              .  
              widow(x)

DR3(349):   x       u  
              .       .  
              widow(x)  
              x admires Pedro  
              u = Pedro  
              x admires u

Truth conditions are then obtained from the DRS by requiring that every embedding of DR2(349) (into the relevant model) be extendible to an embedding of DR3(349). Whereas (348) required only the existence of a single embedding, (349) places a condition on all embeddings. The difference between the two is triggered by the presence of the universal quantifier in DR1(349).

Kamp explicitly notes that the difference in treatment between the existential and the universal quantifiers in DR is driven by the tokenability of the existential and the non-tokenability of the universal quantifier:

The content of an existential sentence has been exhausted once an individual has been established which satisfies the conditions expressed by the indefinite description's common noun phrase and by the remainder of the sentence. But a universal sentence cannot be dealt with in such a once-and-for-all manner. It acts, rather, as a standing instruction: of each individual check whether it satisfies the conditions expressed by the common noun phrase of the universal term; if it does, you may infer that the individual also satisfies the conditions expressed by the remainder of the sentence. This is a message that simply cannot be expressed in a form more primitive than the universal sentence itself. [16]

The universal quantifier, that is, cannot be tokened, since we do not know in advance how large the domain of the model will be.

DRT, as it stands, imposes a condition even stricter than that of mere tokenability: it requires that its quantifiers (other than the universal) be *singly* tokenable. A quantifier is singly tokenable if a token set with only a single member can be found for it. This restriction can easily be removed, however, and DRT extended to cover sentences such as:

(350) Every man who owns two donkeys vaccinates them.

by associating the DRS:

DR1(350):    x                    u                    v  
               .                    .                    .  
               Every man who owns two donkeys vaccinates them.

DR2(350):    x                    u                    v  
               man(x)  
               x owns two donkeys  
               donkey(u)  
               donkey(v)  
               x owns u  
               x owns v



DR3(350):   x                   u                   v  
 .                   .                   .  
 man(x)  
 x owns two donkeys  
 donkey(u)  
 donkey(v)  
 x owns u  
 x owns v  
 he beats them  
 x beats them  
 x beats u  
 x beats v

with (350) being true if every imbedding of DR2(350) is extendible to an embedding of DR3(350). It might seem, in addition, that non-tokenable quantifiers could all be handled in the fashion of the universal quantifier, and thus that, say:

(351) Most men who own a donkey vaccinate it.

could be given the DRS:

DR1(351):   x                   v  
 .                   .  
 Most men who own a donkey vaccinate it

DR2(351):   x                   v  
 .                   .  
 man(x)  
 x owns a donkey  
 donkey(v)  
 x owns v

DR3(351):    x                    v  
  
                  .                    .  
  
                  man(x)  
  
                  x owns a donkey  
  
                  donkey(v)  
  
                  x owns v  
  
                  he beats it  
  
                  x beats it  
  
                  x beats v

with the truth conditions, induced by the presence of the 'most' quantifier in DR1(351), requiring that most ways of embedding DR2(351) into the model be extendible to embeddings of DR3(351). DRT would then have the ability to handle the full range of generalized quantifiers.<sup>327</sup>

However (as is well known), this approach does not function as desired. By requiring that most embeddings of DR2(351) be extendible to embeddings of DR3(351), we require only that most man-donkey ownership pairs are such that the first vaccinates the second. These truth conditions are compatible with both of the following situations:

(S1) Every man owns three donkeys and vaccinates two of them.

(S2) One man owns 4000 donkeys and vaccinates all of them; 3000 other men each own (exactly) one donkey and fail to vaccinate it.

Neither (S1) nor (S2), however, suffice to make (351) true. DRT, then, is forced to reject the vast array of generalized quantifiers. What it

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<sup>327</sup>Of course, even if this approach were to prove fruitful (and we shall see shortly that it does not), DRT would still handle *in a distinctively discourse representation-based style* only the tokenable quantifiers. Any claim that DRT made that discourse representation gave a generic account of the function of quantification would thus remain suspect.

can handle are the tokenable quantifiers and (essentially through a technical slight of hand) the universal quantifier. Relying as it does on tokenability, it is a poor candidate for laying bare the real inner workings of quantification.

### §3.2.1.3.2.4.3 Game-Theoretic Semantics

Unlike DRT, game-theoretic semantics (GTS) accommodates nicely both tokenable and co-tokenable quantifiers. It is just as impotent, however, when it comes to embracing the wider range of generalized quantifiers. GTS determines truth conditions for sentences for setting out rules for a semantic game between two players, called I and Nature. To play the semantic game on a sentence  $\varphi$ , the two players adopt the roles of Verifier and Falsifier (respectively)<sup>328</sup>, and proceed according to the following rules:

(G.A) If  $\varphi$  is atomic, then the Verifier wins if  $\varphi$  is true, while the Falsifier wins if  $\varphi$  is false.

(G. $\wedge$ ) If  $\varphi$  is of the form  $(\psi_1 \wedge \psi_2)$ , then the Falsifier chooses one of  $\psi_1$  and  $\psi_2$  and play continues on that sentence.

(G. $\vee$ ) If  $\varphi$  is of the form  $(\psi_1 \vee \psi_2)$ , then the Verifier chooses one of  $\psi_1$  and  $\psi_2$  and play continues on that sentence.

(G. $\neg$ ) If  $\varphi$  is of the form  $\neg\psi$ , then the two players switch roles, and play continues on the sentence  $\psi$ .

(G. $\forall$ ) If  $\varphi$  is of the form  $(\forall x)\psi(x)$ , then the Falsifier chooses some object  $b$ , and play continues on the sentence  $\psi(b)$ .<sup>329</sup>

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<sup>328</sup>The need for the distinction between player and role becomes apparent in the rule for negation.

<sup>329</sup>Where  $b$  is drawn from the domain of the model, if one is attempting to define truth-in-a-model.

(G.∃) If  $\phi$  is of the form  $(\exists x)\psi(x)$ , then the Verifier chooses some object  $b$ , and play continues on the sentence  $\psi(b)$ .

The sentence  $\phi$  is then true if I have a winning strategy, false if Nature has a winning strategy.

Our interest here will be in the two quantifier rules (G.∀) and (G.∃). Both of these rules proceed by requiring the relevant player to pick some token object. That the rules obtain the appropriate truth conditions is then due entirely to the tokenability (or co-tokenability) of the quantifier in question. Thus the rule (G.∃) can correctly determine whether an existentially quantified sentence is true just because the *tokenable* existential quantifier is such that, if the existential claim is true, then there will be some one object such that the behaviour of that object is sufficient to ensure the truth of the claim. It follows that if the Verifier picks the object, the existence of a successfully picking strategy will correlate appropriately with the existence of a token and hence with the truth of the claim. Similarly, since the universal claim is co-tokenable, if a universal claim is false there will be some token object ensuring its falsity, and the Falsifier will have a successful strategy available by picking that object.

GTS is standardly formulated to allow only the existential and universal quantifiers, but we can with only slight modification of the basic framework allow any tokenable or co-tokenable quantifier. We could, for example, introduce the rules:

(G.∃2) If  $\phi$  is of the form  $(\exists 2x)\psi(x)$ , then the Verifier picks some objects  $b$  and  $c$ , and play continues on the sentences  $\psi(b)$  and  $\psi(c)$ .

(G. $\exists$ <3) If  $\phi$  is of the form  $(\exists<3x)\psi(x)$ , then the Falsifier picks some objects  $b$ ,  $c$ , and  $d$ , and play continues on the sentences  $\psi(b)$ ,  $\psi(c)$ , and  $\psi(d)$ .

to incorporate the tokenable 'at least two' and the co-tokenable 'at most two' quantifiers.<sup>330</sup> Non-tokenable quantifiers, however, lie essentially outside of the GTS framework. What kind of rule, for example, would we introduce for the 'exactly two' quantifier? Who would make the move on such a quantifier, the Verifier or the Falsifier? And what kind of token set would be chosen? Clearly no answer suffices here. No matter what tokens the Verifier chooses, his choice cannot ensure the truth of a sentence of the form ' $(\exists!2)\phi$ ', because such a sentence can be made false also by too many things being  $\phi$  -- just because, that is, the 'exactly two' quantifier is not tokenable. *Mutatis mutandis*, we see that no choice by the Falsifier suffices to ensure the falsity of the sentence, because 'exactly two' is also not co-tokenable. GTS gives us a system, then, which builds into its notion of what a quantifier is the assumption that the quantifier is either tokenable or co-tokenable.<sup>331</sup>

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<sup>330</sup>The basic framework here is modified by allowing a game to proceed on multiple sentences simultaneously. Winning the game would then require winning on all atomic sentences. Standard GTS, by avoiding the introduction of this sort of branching game, imposes the stricter requirement (as does DRT in its standard formulation) that the quantifiers be singly tokenable. Since only the existential and universal quantifiers are singly tokenable, the classical system results.

<sup>331</sup>One can by doing sufficient violence to the basic framework of GTS make room for non-tokenable quantifiers. Thus, for example, we could introduce a rule for 'exactly two' of the form:

(G. $\exists$ !2) If  $\phi$  is of the form  $(\exists!2x)\psi(x)$ , then the Verifier chooses some objects  $b$  and  $c$ , and the Falsifier chooses some object  $d$  not identical to  $b$  or  $c$ , and play continues on  $\psi(b)$ ,  $\psi(c)$ , and  $\neg\psi(d)$ .

At some point, however, one begins to wonder if anything remains of the original assumption that there is a game-like structure to semantics or if we are just implementing, in a gratuitously involuted way, the standard set-theoretic assumptions.

#### **§3.2.1.3.2.4.4 Restricted Quantification and the Conceptual Basis of Quantification**

We should be clear on what has and has not been done here. Nothing said above impugns any of PFO, DRT, or GTS on their preferred grounds. Each of the three does obtain the results that its creators claim for it when confined to the classical quantifiers. However, each of these systems purports to be more than just a clever technical apparatus for regimenting certain obstinate natural language phenomena. Each has the more ambitious goal of providing an explanation of how quantification really works (an alternative, as one might think of it, to the dominant neo-Fregean tradition). Since each is hostile to the addition of generalized quantifiers, the fan of such quantifiers has reason to resist the adoption of any of these stories as his preferred explanation for the functioning of quantification. My limited goal here has been to show that there is good reason for that fan to do so, because the three formal systems under consideration all build in the assumption that quantifiers have one of the two properties of collapsibility and tokenability. Once we see that these properties fail to reflect anything 'deep' about quantification, as we did earlier, we find ourselves free to reject, *qua analysis of the nature of quantification*, those accounts which presuppose tokenability or collapsibility.

#### **§3.2.1.4 Mass Terms and the Limits of Objectual Quantification**

Classically, quantifiers are taken to range over objects. Thus a claim such as:

$$(352) (\forall x)Fx$$

is to be understood as asserting that every *object* (or every object in some privileged domain) has the property of being F. Classical quantification theory is thus well-suited for the analysis of natural language claims involving count nouns. Sentences like:

(353) Every philosopher should read *Naming and Necessity*.

(354) Most tigers have four legs.

can interact with classical quantification theory by allowing the count nouns 'philosopher' and 'tiger' to pick out a class of objects satisfying the count noun and then having the objectual quantifiers act on that class.

Classical quantification theory, on the other hand, is poorly suited for the analysis of natural language claims involving mass nouns. Sentences like:

(355) Most water is polluted.

(356) No clay was used in making this statue.

do not formalize well. If we attempt to formalize (355) as:

(355-RQ) [most x: water x](x is polluted)

using the classical notion of quantification, we are called on to provide *objects* which satisfy the predicate 'is water' and to see if most of those objects are polluted. But of course 'water', being a mass noun, is not satisfied by discrete objects but rather by continuous *stuff*.

To handle quantification restricted by mass nouns in a classical framework, then, one is forced to impose some objectual ontology onto the previously undifferentiated substance identified by the mass noun. Thus, for example, [Sharvy 1980] takes quantification involving mass

nouns to be quantification over *parts of substances*. A sentence of the form:

(357) Some water is polluted.

is true just in case there is some object such that that object is a part of the water, and it is polluted. A sentence of the form:

(358) All water is polluted.

is true just in case every object which is a part of the water is polluted.<sup>332</sup>

#### **§3.2.1.4.1 Some Difficulties With a Partitive Analysis of Mass**

##### **Quantification**

The analysis of quantification over mass terms by means of an imposition of an objectual ontology of parts of substances, however, is a manifestation of an underlying objectual bias. Just as I earlier observed (§3.2.1.1.1) that philosophers have a bias for talking about and admitting into their ontologies single objects rather than pluralities of objects, I here want to claim that philosophers have a similar bias in favor of object-like chunks of reality rather than non-discrete substances-like chunks of reality. Both of these biases come out of the preferred ontology of mathematics and hence are deeply imbedded in classical logic, which originally evolved as a language in which mathematics could be fully formalized.<sup>333</sup>

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<sup>332</sup>Sharvy's particular interest is in the analysis of definite descriptions involving mass terms, such as:

(FN 181) The water is polluted.

which he takes to be true just in case the maximal part which is water is polluted.

<sup>333</sup>The singularist bias and the objectual bias are not wholly independent -- they spring jointly from an atomistic conception of ontology which takes reality fundamentally to be a collection of atomic parts which enter into various relations with each other. Thus it is not surprising to find -- in, e.g., [Link 1983] and following work -- that those who attempt to analyze plurals in a singularist framework also



Rejection of the objectual bias would involve recognition of the fact that quantification over water simply is quantification over (the substance of) water, not over *parts of water* (even if, for independent reasons, we think that parts of water ought to be admitted into our ontology). And, in fact, quantification over parts, rather than directly over masses, introduces certain technical difficulties. First, while the analysis runs smoothly enough with the classical quantifiers ' $\forall$ ' and ' $\exists$ ', it is less clear how things will go when generalized quantifiers are introduced. Thus consider again:

(355) Most water is polluted.

If this is to be understood via an imposition of a part ontology, the resulting analysis is presumably:

(355-RQ) [most  $x$ :  $x$  is water]( $x$  is polluted)

where  $x$  ranges over water parts. But it is not at all clear that just because most water is polluted, most water parts will also be polluted. There are two sources of difficulty here: (a) ensuring that the cardinality relation between sets of water parts match the intuitive mostness condition on the starting mass of water, and (b) ensuring that the ascribed property of being polluted properly transmits downward from water to water parts.

Why should we believe that, just because most water has some property  $\phi$ , most water parts will also have that property  $\phi$ ? The worry is that it may not be true that most of the parts reside in the majority of the substance which is  $\phi$ . When the substances is only finitely divisible, as is water, we can perhaps dismiss this worry given an

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find themselves drawn toward an analysis of mass terms within the same framework.

auxiliary assumption that part density is evenly distributed throughout the substance. But not all masses are finitely divisible. Take, for example, a claim of the form:

(359) Most of the space in my office is taken up by books.

Assuming -- as is at least possible -- that space is infinitely finely divisible, then the space in my office which is taken up by books and the space which is not taken up by books will consist of exactly the same (infinite) number of space-parts.<sup>334</sup> We will thus be forced to conclude, rather counterintuitively, that both (359) and:

(360) Most of the space in my office is not taken up by books.

are both false.

Obviously what we want is to look not just at the bulk *number* of parts, but at the *sizes* of parts. Thus the appropriate analysis of (355) perhaps ought to be:

(356-M)  $(\exists x)(\text{water } x \ \& \ \text{polluted } x \ \& \ (\forall z)((\text{water } z \ \& \ \neg\text{polluted } z) \rightarrow \mu(x) > \mu(z))$

where  $\mu$  is a measure function on water parts.<sup>335</sup> But if the analysis of quantification over masses is genuinely to be situated in the context of a larger theory of objectual quantification via the imposition of a part-ontology, then the analysis of quantificational claims involving mass nouns ought to mirror the analysis of quantificational claims involving count nouns. That is, if the mass quantificational theory is not to be utterly ad hoc, we should expect a claim like:

(361) Most philosophers know logic.

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<sup>334</sup>There is, of course, a further worry about what the appropriate cardinality of infinity of space-parts to be found in my office is.

<sup>335</sup>Of course, we will need different metrics for different substances, since not all substances (e.g., honor) are measurable in space-time terms.

to be analyzed as:

$$(361-M) (\exists x)(\text{philosopher } x \ \& \ \text{knows-logic } x \ \& \ (\forall z)((\text{philosopher } z \ \& \ \neg\text{knows-logic } z) \rightarrow \mu(x) > \mu(z)))$$

Of course, this can't work quite as stated, because (a) philosophers, unlike water parts, do not combine to make new philosophers, and (b) the existence of one particularly large and ignorant philosopher can make (361), under its analysis (361-M), false when it ought to be true.

Thus mostness of parts is not, and cannot uniformly be made to simulate, mostness of substance. The second worry is that the properties of the parts of a substance may differ from the properties of the substance itself. Thus assume that we can correlate most of the water with the majority of the water parts because water is finitely divisible and we can thus take a collection of the majority of the water molecules. Even so, we will not get the right truth conditions for (355), because water can be polluted without any particular water molecules being polluted.<sup>336</sup>

Even if the above worries can be remedied and a successful partitive analysis of (355) can be given, there are further worries about how generally successful a reduction of quantification over masses to objectual quantification over parts can be. One difficulty lies in the fact that not all determiners can be meaningfully used in quantification over masses. Thus while we can say:

(355) Most water is polluted.

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<sup>336</sup>The best available response here seems to be a return to something like the metric analysis suggested in the previous paragraph, in which 'most water is polluted' is understood not as 'most water parts are polluted', but rather as 'some water part which is the sum of most water parts is polluted'. However, the worries about situating such a metric analysis within the broader context of a theory of objectual quantification remain.

(357) Some water is polluted.

(358) All water is polluted.

(362) The water is polluted.

we cannot say:

(363) \*Every water is polluted.

(364) \*A water is polluted.

(365) \*Three waters are polluted.

(366) \*Several waters are polluted.

(367) \*Few waters are polluted.<sup>337</sup>

But this incommensurability of certain determiners and mass nouns is an inexplicable mystery for the fan of the partitive analysis. For under this analysis, the general form of the quantified claim:

(368) DET water is polluted.

is:

(368-P) [DET x: x is a water part](x is polluted)<sup>338</sup>

But when we quantify over (objectual) parts of water rather than (the substance) water, any choice of determiner ought to be acceptable:

(369) Every water part is polluted.

(370) A water part is polluted.

(371) Three water parts are polluted.

(372) Several water parts are polluted.

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<sup>337</sup>All of (363)-(367) are acceptable, of course, on the assumption that 'water' is being used as a count noun rather than a mass noun.

<sup>338</sup>Or, under a metric analysis:

(FN 182)  $(\exists z)[\text{DET } x: x \text{ is a water part}](x \text{ is a part of } z \ \& \ z \text{ is polluted})$

or:

(FN 183)  $(\exists z)(\text{water } z \ \& \ \text{polluted } z \ \& \ (\forall x)((\text{water } x \ \& \ \neg\text{polluted } x) \rightarrow \mu(z) R_{\text{DET}} \mu(x)))$

where  $R_{\text{DET}}$  is an appropriate relation between part measures induced by the choice of determiner. The points in the main text carry over to either of these analyses.

(373) Few water parts are polluted.

The partitive approach, by attempting to make all quantification over masses into quantification over objects, is thus unable to explain the fact that certain types of quantification are distinctively objectual. The same point, of course, applies the other way around: certain determiners are acceptable with mass nouns but not with count nouns, a fact which is equally inexplicable for those who would make quantification over masses into quantification over parts of masses:

(374a) Little water is polluted.

(374b) \*Little water parts is (are?) polluted.

(375a) Less water than air is polluted.

(375b) \*Less water parts than air parts is (are?) polluted.

#### **§3.2.1.4.2 Mass Quantification and the Anaphoric Account**

The anaphoric account of variable binding, on the other hand, has a reasonably straightforward explanation of quantification involving mass nouns. On the anaphoric account, the first stage of quantification involves restriction of the variable, through which the variable comes to inherit semantic properties from its restrictor. Thus when the restrictor is a count noun, the restricted variable will come to refer to all those objects which satisfy the restricting count noun. When, however, the restrictor is a mass noun, the variable will come to refer to whatever substance it is that is distinguished by the mass noun. There is thus no objectual bias built into the anaphoric conception of quantification -- whatever type of semantic value the restrictor

possesses will be passed on to the variable, regardless of whether that semantic value is objectual in nature.<sup>339</sup>

There is no difficulty, then, in the anaphoric account in having quantification which is directly quantification over masses rather than quantification over objects. The underlying mechanisms of variable restriction are completely agnostic on what type of thing (broadly speaking) gets quantified over, and the particular choice of type in particular cases will be triggered by the actual restrictor. What we will need, however, is a conception of determiner or distributor which is loose enough to see them sometimes as acting on objects and sometimes as acting on substances. Of course we are free to have -- and, indeed, we will need to have -- certain determiners which can only act on objects and certain determiners which can only act on substances. But we will for many determiners -- such as 'most', 'all', 'some', and 'the' -- need to have some statement of their distributional impact which is neutral between impact on objects and impact on substances.

I think, in fact, that the requisite concepts of majority, universality (or exclusivity), and existence (or instantiation) are sufficiently neutral to undergird an agnostic account of 'most', 'all', 'some', and 'the'.<sup>340</sup> However, in my actual detailed discussion of distribution in §3.3 below, I will always treat determiners as distributors of objects. For now I take it as sufficient validation of

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<sup>339</sup>This same semantic agnosticism of the anaphoric account, as we will see in §3.2.2.2.2 below, makes the account particularly well-suited for explaining higher-order quantification.

<sup>340</sup>The difficulty, in fact, may be in explaining why determiners like 'every' and 'each', despite having the same universal force as 'all', do not allow distribution of substances. Perhaps it can simply be a stipulative fact about our language that 'every' and 'each' act on objects.

the anaphoric account that its starting notion of restriction allows it, unlike the classical account, directly to quantify over substances as well as objects.

### **§3.2.2 Restriction and Higher-Order Quantification**

I want to close this discussion of variable restriction under the anaphoric account with some discussion of how to understand higher-order quantification. The anaphoric account is intended to provide a wholly general account of quantification and variable binding of all forms, and I want here to show that is better suited for understanding higher-order quantification than are rival accounts such as the neo-Fregean account. This display will involve three phases. First, we will discuss the distinction between substitutional and objectual quantifiers, and situate the anaphoric account in this landscape. Second, we will argue that there are two importantly different ways of understanding the notion of higher-order quantification, and suggest that one these two ways is both preferable and more difficult to make sense of on classical stories about quantification. Third and finally, we will show how the features that the anaphoric account shares with substitutional quantification make it well-suited for capturing the preferred understanding of higher-order quantification, although we will close with some tentative suggestions that, nonetheless, the entire project of higher-order quantification may be misguided.

#### **§3.2.2.1 Substitutional vs. Objectual Quantification**

Recall (from §1.2.10) that substitutional quantifiers differ from objectual quantifiers in two important ways. First, the range of

quantification comes not from the world, but from the language. Thus in a substitutionally quantified sentence of the form:

$$(376) (\Sigma x)Fx^{341}$$

the domain of quantification is provided not by what objects exist, but rather by what names are in the language. These names are then substituted for the quantified variable, and (376) is true if each instance of the form:

$$(377) F\alpha$$

is true, for  $\alpha$  a name in the language. Second, substitutional quantification readily generalizes beyond the name-substitution case. Thus we can have substitutional quantifiers ranging over predicates:

$$(379) (\Pi X)Xa$$

or over sentential connectives:

$$(380) (\Sigma C)(Fa \text{ } C \text{ } Gb)$$

[Van Inwagen 1981] protests, I think rightly, against substitutional quantification that it is unclear what proposition is being expressed by substitutionally quantified claims. Take a claim like (376) above. We know that the proposition expressed is not the same as that expressed by the objectually quantified:

$$(381) (\exists x)Fx$$

Nor, we are told, is it the same as the metalinguistic proposition:

$$(382) \text{Some name } \alpha \text{ is such that the sentence '}\alpha \text{ is F' is true.}$$

although this sentence has the same truth conditions as (376). For these reasons, I am suspicious of substitutional quantification when

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<sup>341</sup>Recall that we are using ' $\Sigma$ ' for the substitutionally interpreted existential quantifier and ' $\Pi$ ' for the substitutionally interpreted universal quantifier.



understood as anything more than a notational shorthand for metalinguistic claims of the form (382).

My anaphoric account, however, might be called a pseudo-substitutional account of quantification. It shares with substitutional quantification the feature that what can be quantified over is dependent on the linguistic resources of the object language. Of course, in my case it is not because we are actually substituting in linguistic tokens in quantification, but because quantification, being essentially restricted quantification, draws its (objectual) range not directly from the (whole) world but from portions of the world as presented to it by restricting predicates. Furthermore, there is no need for sentences under the anaphoric account to be understood metalinguistically, since what is passed on via quantification is not linguistic tokens themselves, but meanings of linguistic tokens (in the first-order case, objects).

Also like substitutional quantification, anaphoric quantification easily generalizes past the first-order case. Any time we have two semantic categories  $C_1$  and  $C_2$  related in such a way that  $C_1$ -type items have as semantic value some method of distinguishing semantic values of type  $C_2$ , then a  $C_1$  restriction can pass on to a variable situated in a  $C_2$ -type syntactic location some  $C_2$ -type semantic values, which can then form a complete proposition.<sup>342</sup> Given the appropriate restricting category, then -- a category which stands to predicates as predicates

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<sup>342</sup>Again, note that my account is only pseudo-substitutional in that (a) it passes on real semantic values, not tokens bearing values, and (b) not just any syntactic category can be made the target of quantification (unlike genuine substitutional quantification) -- we must have a restricting category standing in the appropriate relation to the restricted category.

stand to names -- we are in a good position to make sense of higher-order quantification.

### **§3.2.2.2 Higher-Order Quantification**

In a better position to make sense of higher-order quantification, in fact, than traditional accounts of higher-order quantification.

Traditional second-order quantification generalizes the syntax of first-order logic by adding second-order variables which can appear in all of the same syntactic positions as predicates, and then allows quantification over those variables, where the range of quantification is provided by the power set of the domain of (first-order) quantification. Thus a second-order sentence of the form:

$$(383) (\forall X)(\exists x)(\forall y)(Xx \leftrightarrow Xy)$$

will be true just in case there is some subset of the domain which is either empty or total. I want now to turn to distinguishing two ways of thinking about higher-order quantification, and show that this traditional approach is well-suited only to one of the two.

#### **§3.2.2.2.1 Two Notions of Higher-Order Quantification**

Focus for the moment on the case of second-order logic. The traditional approach sketched above treats second-order logic as an *ontology-enriched disguised first-order logic*. We are still quantifying over objects, but we have enriched our ontology beyond our original collection of objects to include as well sets (or properties). We then have a two-typed first-order logic with one variable type ranging over the 'plain' objects and another variable type ranging over the new property-like objects, and an implicit predicate 'is instantiated by' which mediates concatenations of variables of the two types.

The classical conception of quantification is forced into this implicitly first-order treatment of higher-order quantification because it is, at its core, wholly objectual in its understanding of variable binding. What quantifiers do is cause variables to range over objects; thus when we move to the second-order case, in which we do not have objects to range over, we introduce a *new* type of object. But once the new object has been introduced, quantification over it is like quantification over any other type of object. Second-order logic, on the classical conception, is not really second order at all. It is first-order quantification over even more things. Thus Quine's complaint that second-order logic is merely set theory is disguise.

In trying to differentiate second-order (or higher-order in general) logic from an ontologically enriched first-order logic, one tends to be pushed to declarations that these second-order objects are somehow special types of objects -- higher-order types. Thus what was originally a theory of higher orders of quantification becomes a theory of higher orders of objects, and a mysterious and pernicious doctrine of higher-order objects is born. We want, for example, the 'things' ranged over by second-order quantifiers not really to be things at all, to be of a wholly different nature. But the scare quotes around 'things' in the previous sentence gives away the game. If they are things at all, then they can be ranged over by simple first-order quantifiers, and higher-order quantification gains us nothing new. But if they are not things, then our classical notion of quantification has no purchase on them. This dilemma forces, for example, Frege into the difficult position of insisting that functions are things, but things which cannot

be named, and thus are not legitimate targets for first-order quantification.

This is, I think, not what second-order logic was intended to be, and I want to suggest that it is certainly not what it *needs* to be. We don't want our second-order logic to be just more objectual quantification over some new (mysterious) type of object. There are no types of objects; all entities are equally first-order.<sup>343</sup> We want it to be a logical operation which stands to predicates as first-order quantification stands to variables. One way of putting this point is in terms of ontological commitment. The type of commitment incurred by a first-order quantified sentence of the form:

$$(384) (\forall x)Fx$$

is the same as the type of commitment incurred by a corresponding unquantified sentence of the form:

$$(385) Fa$$

-- that is, a commitment to objects. Similarly, the type of commitment incurred by a second-order quantified sentence of the form:

$$(386) (\forall X)Xa$$

ought to be the same as the type of commitment incurred by (the predicate in) a corresponding unquantified sentence of the form:

$$(387) Fa$$

-- that is, no commitment at all (not a commitment to a property, or to some sort of higher-order entity). Second-order quantification should

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<sup>343</sup>This is not to deny, of course, that there can be (for example) a coherent type theory within set theory, distinguishing sets by their stage of formation in an iterative conception of the set. But any set, regardless of its type, is equally a set once formed. The types are not metaphysically discriminatory.

not be objectual at all. We seek an *ontologically neutral, genuinely higher-order* notion of quantification.

#### **§3.2.2.2.2 Anaphoric Binding, Pseudo-Substitutionality, and Higher-Order Categories**

While the classical account of quantification is objectual to the core and thus resistant to a non-objectual account of higher-order quantification of the form sketched above, the anaphoric account, with its pseudo-substitutional character, is ideally suited to provide such an account. The anaphoric account is objectual in the first-order case just because (a) objects are the semantic values carried by names, and (b) in the first-order case variables are restricted by lexical items of a category which serves to distinguish names. The objectuality of the quantification, then, is a consequence of the particular level in the semantic hierarchy on which the quantification occurs, rather than a deeply rooted feature of the quantification.

When we move our quantification to a different semantic level, however, the objectuality of the quantification vanishes. Assume we have some semantic category C which stands to predicates as predicates stand to names. The semantic value of C-type terms, then, serves to distinguish the semantic values of predicate-type terms.<sup>344</sup> We can then form sentences such as:

$$(388) [C]_X (\exists X) Xa$$

in which the (second-order) variable 'X' is first brought to have the semantic behaviour of all those predicates whose semantic values are

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<sup>344</sup>One might call C-type semantic values properties of properties, but to do so is to reduce both C-type terms and predicates back down to the first-order case, with both serving merely to name some special type of object.

distinguished by the restricting term  $C$ , and then is distributed via the distribution rule associated with  $\exists$ . No entities, whether first- or higher-order, are needed to formulate this notion of quantification.

#### **§3.2.2.2.2.1 Higher-Order Quantification in Natural Language**

Despite what I see as the success of the anaphoric account in making sense of a genuinely higher-order notion of quantification, I remain skeptical about both the utility and the sensibility of such quantification. Note that in the previous discussion we made a crucial assumption:

**(Categorical Assumption)** Assume we have some semantic category  $C$  which stands to predicates as predicates stand to names

This assumption, of course, is crucial to getting the whole project of higher-order quantification off the ground. Nevertheless, it seems to me that there is good reason to think that it is false.

The difficulty is that it simply isn't clear what such a category would be. Were we still imbedded in the earlier ontologically-enriched first-order notion of quantification, of course, the appropriate (third-order) category would be easy to construct -- we would simply select from the power set of the power set of the first-order domain of quantification. Once we abandon the doctrine of higher-order objects, however, we cannot simply construct our semantic values in such a way. We must understand what kind of semantic function could stand to predicates as predicates stand to constants.

I admit a failure to grasp what such a category could be like, although such a failure of the imagination is of course no argument (and should some good explanation of the requisite category come along, I

would be happy to endorse it). But it is worth noting that natural languages do not seem to make use of second-order quantification. Where such quantification seems to be called for, the language of first-order quantification gets used instead:

(389) Socrates has every property that Plato does.

(390) Socrates did everything that Plato did.

and no appropriate third-order restricting categories appear. Cases of VP-deletion have something like the phenomenon we are seeking, as the content of one predicate is anaphorically passed to another syntactic position:

(391) John loves his wife, and Albert does too.

but here there is no quantification, because there is no third-order quantifier to restrict the variable. It appears that natural language contains second-order variables, and simple second-order anaphora, but lacks the third-order restrictors necessary to develop this core syntactic machinery into genuine second-order quantification. Again, absence of a logical phenomenon in natural language is by no means an argument that the phenomenon does not or cannot exist, but it is, I think, reason for some skepticism.

### **§3.3 Variable Distribution**

Our next task is to investigate the second component of quantification according to the anaphoric account -- variable distribution. After opening with an initial sketch of the mechanisms of distribution in which we first compare my variable distribution to the neo-Fregean conception of determiners, as exemplified in [Barwise & Cooper 1981] and then identify some interesting and useful taxonomic features of variable

distributors, we turn to a detailed investigation of one technical issue in variable distribution; examining at length the possibility of specifying a 'branching' conception of quantification, in which quantifier prefixes are merely partially, rather than linearly, ordered. The discussion of distribution will close with a brief note on the possibility of polyadic distributors, which affect multiple variables simultaneously -- a note which will look forward to future work while raising some worries to be addressed in any such work.

### **§3.3.1 Determiners and Distribution**

On the anaphoric account view of variable binding, a variable begins life as semantically contentless lexical material. Then, by undergoing the binding relation which I have called 'restriction' with some open formula in the lexical vicinity, the variable becomes a plural referring expression, referring to those things which satisfied the restricting formula. At this point, the process of variable binding *may* be complete. Thus, in sentences like:

(392) Every man who owns a donkey binds *it*.

(393) Good citizens pay their taxes on time. *They* even enjoy doing  
so.

the italicized pronouns are restricted by 'donkey he owns' and 'good citizen' respectively, and thus come to refer (plurally) to the donkeys owned by each man and to the good citizens. Nothing further is done to these new-found referring expressions, so they proceed to enter into the truth conditions of the sentences in which they appear.

In other cases, however, a type of semantic operator which I have called the 'distributor' attaches to the (now plurally referential)



variable.<sup>345</sup> These distributors correspond to the determiners of the system of [Barwise & Cooper 1981]. They are exemplified, then, by terms such as 'all', 'some', 'the', 'most', 'many', etc.<sup>346</sup> Broadly speaking, these terms are used to specify a quantity of objects. On my account, their function will be to take a plural referring expression and break it up into a (possibly infinite) sequence of referring expressions, each of which meets the relevant quantitative test. This we call the distribution of the reference; it is to the details of the nature of distribution that we turn now.

### **§3.3.1.1 The Nature of Distribution**

Consider the distributor 'most' as applied to a plural referring expression R such that R refers to Albert, Beth, and Charles. Then the result of applying 'most' to R would be four new referring expressions  $R_1$ ,  $R_2$ ,  $R_3$ , and  $R_4$  such that:

- (a)  $R_1$  refers to Albert and Beth
- (b)  $R_2$  refers to Albert and Charles
- (c)  $R_3$  refers to Beth and Charles
- (d)  $R_4$  refers to Albert, Beth, and Charles

If we have some monadic predicate P, then the truth conditions of:

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<sup>345</sup>More generally, a distributor will attach to a chain of variables. Given a sentence such as:

(FN 184) Every man admires his father.

analyzed as:

(FN 185) [man x]x ( $\forall x$ )(x admires x's father)

we cannot view the universal distributor as acting on a single variable, or even as acting separately on two variables. Instead, we ought to think of the two (co-indexed) occurrences of 'x' as forming a single syntactic entity -- a chain -- to which the distributor is applied. Here we see one of the sources of the treatment of quantifiers as sentential operators, rather than noun phrases, since these chains can be distributed throughout entire sentences.

<sup>346</sup>The attentive reader will note that these determiners are all members of a rather special subset of [Barwise & Cooper 1981]'s determiners.

(394)  $\text{most}(P(R))$

will be:

(395)  $P(R_1) \vee P(R_2) \vee P(R_3) \vee P(R_4)$

What we ask, then, is that some appropriate distribution of the reference meet the condition imposed by the predicate.

More generally, we can associate with each distributor  $D$  a function  $D_D$  such that, for all sets  $X$ <sup>347</sup>,

(396)  $D_D(X) \subseteq \wp(X)$

which specifies how that distributor distributes references. We can then state the following basic law of distribution:

**(Fundamental Law of Distribution)** Given any determiner  $D$ , any predicate  $P$ , and any plurally referring expression  $R$ ,

$D(P(R))$  is true iff  $\exists r \in D_D(\text{ref}(R))$  such that  $P$  is true of  $r$ .

If you like, you can think of the distributor as creating a massive new disjunction of the form:

(397)  $\bigvee_{r \in D_D(\text{ref}(R))} P(\underline{r})$

(where  $\underline{r}$  is a plural referring expression referring to the elements of  $r$ ), with the following caveats:

(A) In some cases, the distributor will return the null sequence when applied to a plural referring expression. Examples include the distributor 'more than 100' applied to the plural referring expression 'John and Albert', or the distributor 'both' applied to any expression referring to more or less than two objects. In these cases, the disjunction indicated above would be empty. However, when the

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<sup>347</sup>I here use a set to represent the plural reference of a term, despite my earlier insistence that plural reference not be assimilated to singular reference to sets (containing a plurality of objects). See my earlier comments on the singularist bias of mathematical language.

distributor returns a null sequence, the distributed formula is not meaningless (as the non-existent disjunction might seem to indicate) but false (as the formal truth clause for distributors ensures).

(B) In some cases, application of a distributor will create an infinite sequence of new references, as when the distributor 'some' is applied to a term receiving its (plural) reference from the formula 'integer(x)'. In such cases, the corresponding disjunction will be a formula of infinite length. I do not mean to be working in a metalanguage which allows for such formulae; to the extent that they figure in the disjunctive analysis, that analysis must be taken as merely heuristic.<sup>348</sup>

(C) Despite appearances, the process of distribution does not create any new lexical items. The appearance of new such items in the disjunctive analysis indicates again that that analysis is merely heuristic.

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<sup>348</sup> [Dunn and Belnap 1968] introduces and dismisses a criticism related to these observations:

The substitutional interpretation involves an illegitimate use of 'etc.' and thereby absurdly tries to reduce quantificational logic to propositional logic. No. There is an explicit reference in the semantics to an infinite totality (of names), and reduction to propositional logic is manifestly impossible. [184]

I am not entirely certain why a reduction of quantificational to propositional logic would be absurd, but in any case my system also fails to provide such an analysis. There is no purely propositional sentence --- even allowing for sentences of infinite length -- which is equivalent (in all models) to any non-trivial quantificational sentence. Even if one limits oneself to a class of models all of which agree on the reference of all singular terms, almost no quantificational sentences are equivalent (relative to these models) to propositional sentences, because (as is noted in comment (A) above), were the distributed plural referring expression refer to nothing, the putatively corresponding disjunction would (absurdly) have zero disjuncts. Nevertheless, quantificational sentences do have in common with disjunctions that there are multiple ways in which the same sentence can be made true.

These cautionary notes aside, I will often appeal to the disjunctive analysis of the distributive process in illustrating the semantic analysis of various sentences.

Given this framework, we can now state distribution rules for some commonly used distributors:

$$(398) D_{\forall}(X) = D_{\text{all}}(X) = D_{\text{every}}(X) = D_{\text{each}}(X) = \{X\}^{349}$$

$$(399) D_{\exists}(X) = D_{\text{some}}(X) = D_a(X) = \{Z \mid Z \subseteq X, |Z| = 1\}$$

$$(400) D_2(X) = \{Z \mid Z \subseteq X, |Z| = 2\}$$

$$(401) D_n(X) = \{Z \mid Z \subseteq X, |Z| = n\}$$

$$(402) D_{\text{most}}(X) = \{Y \mid Y \subseteq X, |Y| > |X - Y|\}$$

$$(403) D_{\text{uncountably many}}(X) = \{Y \mid Y \subseteq X \ \& \ |Y| > \aleph_0\}$$

$$(404) D_{\text{the}}(X) = \{X\}, \text{ if } |X| = 1 \quad [\text{singular case}]$$

$$(405) D_{\text{the}}(X) = \{X\}, \text{ if } |X| \neq 0 \quad [\text{plural case}]^{350}$$

<sup>349</sup>Some proposals for the differences among 'all', 'every' and 'each' are discussed both in §3.2.1.4.1 and in §3.3.2.2.3.3.

<sup>350</sup>The choice between the singular and the plural 'the' determiner seems largely to be triggered by the number of the accompanying lexical context (although consider examples such as 'the whale is a mammal' as possible violations of this rule). It would be nice to have a more generic description of the function of 'the' which did not force a choice between the singular and plural readings.

$$(406) D_{\text{both}}(X) = \begin{cases} \{X\}, & \text{if } |X| = 2 \\ \emptyset & \text{otherwise} \end{cases} \quad 351$$

If 'people' has Albert, Beth, and Charles in its extension, then consider:

(407) Both people are tall.

(408) All people are tall.

The distributor 'both', when applied to the plural referring expression R restricted by 'people', returns the null set because:

$$(409) D_{\text{both}}(\{\text{Albert, Beth, Charles}\}) = \emptyset$$

Our truth conditions then tell us that (16) is true iff some element of the distributed reference of R is tall, but since the distributed reference has no members, this is impossible, and (407) comes out false -- the desired (Russellian) result. 'All', on the other hand, has the following distributive effect:

$$(410) D_{\text{all}}(\{\text{Albert, Beth, Charles}\}) = \{\{\text{Albert, Beth, Charles}\}\}$$

So (408) will be analyzable as the following disjunction:

(411) Albert and Beth and Charles are tall.

-- again yielding the correct truth conditions.

### §3.3.1.1.1 Strong and Weak Distribution

In stating distribution rules for distributors above, we gave in some cases two options. Thus, for example, the distributor 'some' can be interpreted either according to the rule:

$$(412) D_{\text{some}}(X) = \wp(X) - \emptyset$$

or according to the rule:

$$(413) D_{\text{some}}(X) = \{\{z\} \mid z \in X\}$$

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<sup>351</sup>Note that 'both', unlike 'the', does not allow for singular and plural readings.

The first of these rules distributes the reference in every way in which each distributed referent satisfies the appropriate cardinality condition 'some'. The second rule, however, distributes only to those referents which minimally satisfy the cardinality rule -- i.e., to those groups which contain only a single referent. Call the first sort of distribution the *weak* reading of 'some' and the second sort the *strong* reading of 'some'.

Any distributor which sets a minimal cardinality condition can be given weak and strong readings. Thus, for example, the distributor 'two' has a weak reading in which it distributes its target into all groups of at least two objects, and another strong reading on which it distributes its target only into groups of exactly two objects.<sup>352</sup> In general, any

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<sup>352</sup>Determining whether a cardinality quantifier in English ought to be given a (monotone increasing) 'at least' reading or a (non-monotonic) 'exactly' reading is a notoriously difficult process. A sentence such as:

(FN 186) Three students left the room.

can, in appropriate contexts, be heard either as:

(FN 187) At least three students left the room.

or as:

(FN 188) Exactly three students left the room.

Because, on my system, all primitive distributors are interpreted as monotone increasing distributors (see §3.3.1.3.1 below for details), I opt for assuming that bare cardinality quantifiers are always given the 'at least' reading and that pragmatic considerations can account for the apparent exceptions. However, a related puzzle with bare cardinality quantifiers lies in understanding, whether one chooses to treat them as monotone increasing or as non-monotonic, how adverbial modification by 'at least' and 'exactly' can be sensible. For example, if we say (as I think we should) that 'three' is always to be understood as 'at least three', how are we to make sense of (FN 187) above, which ought then to be equivalent to:

(FN 189) Exactly at least three students left the room.

On the other hand, if we say that 'three' ought to be understood as 'exactly three', then (FN 186) above is problematic, being equivalent to:

(FN 190) At least exactly three students left the room.

My hope, although I make no attempt to work out the details here, is that the distinction between weak and strong readings of the underlying bare cardinality distributor can help make sense of the apparently impossible adverbial modifications (note that the weak and strong readings do not in themselves capture the monotonicity differences; see the discussion below in the main text).

'n'-type cardinality distributor has weak and strong readings. A distributor like 'all', of course, is trivially barred from having weak and strong readings, as is the singular reading of 'the'. Other distributors, like 'several' and the plural 'the' have no weak/strong distinction because there is no minimal cardinality condition to which we can restrict ourselves in the strong case (we cannot consider only groups of exactly several objects, for example).

Note that, despite appearances, the weak and strong readings of cardinality distributors do not account for the distinction between 'at least' and 'exactly' claims. The sentence:

(414) Two books are on the table.

for example, is read as:

(415) At least two books are on the table.

regardless of whether 'two' is given a weak or strong reading. If there are three books A, B, and C on the table, then each of the following is true:

(414-WEAK) A and B are on the table or B and C are on the table or A and C are on the table or A, B, and C are on the table.

(414-STRONG) A and B are on the table or B and C are on the table or A and C are on the table.<sup>353</sup>

However, there are constructions in which weak and strong readings are distinguishable. If the predicate being quantified prefers a collective, rather than a distributive, reading, differences between weak and strong readings may emerge. Thus consider the truth value of:

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<sup>353</sup>This truth-conditional equivalence is, as we will see below, a consequence of the fact that all primitive distributors are, on my system, monotone increasing distributors.

(416) Two men pushed the car up the hill.

when David, Egbert, and Francis pushed the car up the hill together. On the weak reading we get the true disjunction:

(416-WEAK) David and Egbert pushed the car up the hill or David and Francis pushed the car up the hill or Egbert and Francis pushed the car up the hill or David, Egbert, and Francis pushed the car up the hill.

On the strong reading, we drop the final disjunct and thus get the false:

(416-STRONG) David and Egbert pushed the car up the hill or David and Francis pushed the car up the hill or Egbert and Francis pushed the car up the hill.

A preference for the weak reading can be created by adding an explicit 'at least':

(417) At least two men pushed the car up the hill.<sup>354</sup>

### **§3.3.1.2 Some Taxonomic Features of Determiners**

On [Barwise & Cooper 1981]'s view, there are quite a lot of quantifiers.<sup>355</sup> In order to bring some order to this zoo, Barwise and Cooper impose some taxonomy on the quantifiers by identifying certain logical properties which distinguish interesting subclasses of quantifiers. Since some of these taxonomic categories will be of interest to us, I want to begin by listing the major of Barwise and

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<sup>354</sup>For an example of a case in which the strong reading of a cardinality distributor creates a reading distinct both from that created by the weak reading of the same distributor and from that created by the corresponding non-monotonic 'exactly' distributor, see the discussion of independently branching quantification in §3.3.2.2.2.3.1.5.

<sup>355</sup>To be precise, on a domain of  $n$  objects, there are  $2^{2^n}$  distinct quantifiers.



Cooper's distinguished properties. In all of the below, a quantifier is interpreted as a set of subsets of the domain (i.e., a property of a property). Determiners -- the equivalent of my distributors -- are interpreted as functions from sets to quantifiers. Throughout, double bars ('||') will be used to map terms into their denotations, and the set E will be the domain of discourse in the model:

**(Living On)** A quantifier  $||D||(A)$  *lives on* A if, for all X,  $X \in ||D||(A)$  iff  $X \cap A \in ||D||(A)$ . A determiner D satisfies the *living on* condition iff for any set A,  $||D||(A)$  lives on A.

**(Sieves)** A quantifier Q is a *sieve* if  $||Q|| \notin \wp(E)$  and  $||Q|| \notin \emptyset$ .<sup>356</sup>

**(Partially Defined Determiners)** A determiner is partially defined if it does not create a quantifier in combination with every set. According to Barwise and Cooper, 'the', 'both', and 'neither' are all partially defined. They give the following clause for the determiner 'the':

$$||\text{the}||(A) = \begin{cases} ||\text{every}||(A) & \text{if } |A|=1 \\ \text{undefined} & \text{otherwise} \end{cases} \quad [169]$$

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<sup>356</sup>Note that the same determiner can, when attached to different sets, create sometimes a quantifier which is a sieve and sometimes one which is not. Thus, for example:

(FN 191) every film by Maya Deren  
is a sieve -- true of 'is black and white' and false of 'stars Cary Grant'. The quantifier:

(FN 192) every object traveling faster than the speed of light  
on the other hand, is not a sieve. It denotes the power set of the domain and is thus true of any predicate. Any normal natural language determiner can at least sometimes create quantifiers which are sieves, though certain unusual constructions, such as:

(FN 193) either more than five or fewer than three men

(FN 194) few and many philosophers

never give rise to sieves.

so that the quantifier 'the present king of France' is undefined.<sup>357</sup>

**(Strength and Weakness)** A determiner D is:

(a) *positive strong* if for every model  $M = \langle E, || \cdot || \rangle$  and for every  $A \subseteq E$ , if the quantifier  $||D||(A)$  is defined, then  $A \in ||D||(A)$ .

(b) *negative strong* if for every model  $M = \langle E, || \cdot || \rangle$  and for every  $A \subseteq E$ , if the quantifier  $||D||(A)$  is defined, then  $A \notin ||D||(A)$ .

(c) *weak*, if neither positive nor negative strong. [182]

**(Definiteness)** A determiner D is definite if for every model  $M = \langle E, || \cdot || \rangle$  and every A for which  $||D||(A)$  is defined, there is a non-empty set B, so that  $||D||(A)$  is the sieve  $\{X \subseteq E \mid B \subseteq X\}$  [183-184]

**(Monotonicity)** A quantifier Q is:

(a) *monotone increasing* (mon  $\uparrow$ ) if  $X \in Q$  and  $X \subseteq Y \subseteq E$  implies  $Y \in Q$ .

(b) *monotone decreasing* (mon  $\downarrow$ ) if  $X \in Q$  and  $Y \subseteq X \subseteq E$  implies  $Y \in Q$ .

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<sup>357</sup>Barwise and Cooper's particular uses of partially defined determiners thus amount to a rejection of the Russellian analysis of definite descriptions and a preference for an 'undefined' rather than a 'false' result when a non-denoting definite description is attached to a predicate. Note that the introduction of undefined quantifiers is purely a matter of taste on the part of Barwise and Cooper. There can also be, under their formalism, a determiner 'the<sub>R</sub>' defined as:

$$(FN 195) \quad ||the_R||(A) = \begin{cases} ||every||(A) & \text{if } |A| = 1 \\ \text{otherwise} & \end{cases}$$

which exactly matches the behaviour of the Russellian description. This fully defined Russellian 'the' determiner matches the behaviour of the singular 'the' distributor as defined above.

(c) *non-monotonic*, if neither monotone increasing nor monotone decreasing. [184-185]

**(Duality)** The *dual* of a quantifier  $Q$  on  $E$  is the quantifier  $\bar{Q}$  defined by  $\bar{Q} = \{X \subseteq E \mid (E - X) \notin Q\}$ . If  $Q = \bar{Q}$ , then  $Q$  is *self-dual*.

**(Persistence)** A determiner  $D$  is:

(a) *persistent*, if for all  $M = \langle || \quad ||, E \rangle$  and all  $A \subseteq B \subseteq E$ , if  $X \in ||D||(A)$  then  $X \in ||D||(B)$ .

(b) *anti-persistent*, if for all  $M = \langle || \quad ||, E \rangle$  and all  $A \subseteq B \subseteq E$ , if  $X \in ||D||(B)$  then  $X \in ||D||(A)$ .

(c) *non-persistent*, if neither persistent nor anti-persistent.<sup>358</sup>

All of Barwise and Cooper's taxonomic distinctions translate readily from their neo-Fregean determiner-based language to our distributor-based language. Two of these distinctions will be of special interest to us: those of living on and monotonicity. A determiner  $D$  satisfies the 'living on' condition just in case the truth of a sentence of the form:

(418) [<sub>S</sub> [<sub>NP</sub> [<sub>DET</sub>  $D$ ] [<sub>N'</sub>  $\varphi$ ]] [<sub>VP</sub>  $\psi$ ]]

depends only on what goes on with the  $\varphi$ s, and not with the non- $\varphi$ s. Thus a determiner satisfies the living on condition just in case:

(419)  $D \varphi$ s are  $\psi$ .

is equivalent to:

(420)  $D \varphi$ 's are  $\varphi$ s and  $\psi$ s.

Natural language determiners all satisfy the living on condition, as the following test cases indicate:

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<sup>358</sup>Note that the property of persistence is logically equivalent to the property of tokenability as defined in §3.2.1.3.2.3 above.

- (421) All men are tall  $\Leftrightarrow$  All men are men who are tall
- (422) Some men are tall  $\Leftrightarrow$  Some men are men who are tall.
- (423) Few men are tall  $\Leftrightarrow$  Few men are men who are tall.
- (424) Exactly three men are tall  $\Leftrightarrow$  Exactly three men are men who are tall.

However, on the neo-Fregean analysis, it is a trivial exercise to specify determiners which do not satisfy the living on condition, such as:

$$(425) \quad ||D||(A) = \{X \mid (E - A) \cap X \neq \emptyset\}$$

A quantifier is monotone increasing just in case an expansion of the extension of the predicate being applied to the quantified noun phrase preserves truth. Thus 'every man' is mon  $\uparrow$ , because:

(426) Every man is mortal.

implies:

(427) Every man is mortal or immortal.

A quantifier is monotone decreasing if a contraction of the extension of the predicate preserves truth. Thus 'few men' is mon  $\downarrow$ , because:

(428) Few men are mortal or immortal.

implies:

(429) Few men are mortal.<sup>359</sup>

A quantifier is non-monotonic if neither contraction nor expansion creates any valid inferences. Thus 'exactly two' is non-monotonic, since the following are logically independent:

(430) Exactly two men are mortal.

(431) Exactly two men are mortal or immortal.

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<sup>359</sup>Note that these implications are not reversible in either case. (427) does not imply (426), and (428) does not imply (427).

The language of monotonicity can easily be extended from quantifiers to determiners (or distributors) by calling a determiner monotone increasing if it creates a monotone increasing quantifier when combined with any set, monotone decreasing if it creates a monotone decreasing quantifier when combined with any set, and non-monotonic if it is neither  $\text{mon } \uparrow$  nor  $\text{mon } \downarrow$ .

Barwise and Cooper do not discuss quantifier logicity, but others in the neo-Fregean tradition have proposed permutation invariance as the appropriate test of logicity. We thus have:

**(Logicity)** A quantifier  $Q$  is logical if, given any model  $M = \langle \mathcal{U}, E \rangle$  and any permutation  $p$  of  $E$ ,  $\|Q\| = p(\|Q\|)$ . A determiner  $D$  is logical if  $\|D\|(A) = p(\|D\|(p(A)))$ .

In natural language, quantifiers (i.e., quantified noun phrases) will not typically be logical, but determiners will be.

### **§3.3.1.3 Deriving Some Universals**

Having made the taxonomic distinctions listed above, Barwise and Cooper use their categorization to isolate ten claims describing the behaviour of (restricted) quantifiers, claims which they hypothesize hold of all natural languages. Their ten linguistic universals are:

**U1. NP-Quantifier Universal:** Every natural language has syntactic constituents (called noun-phrases) whose semantic function is to express generalized quantifiers over the domain of discourse.

**U2. Dislocated phrase universal.** If a language allows phrases to occur in a dislocated position associated with a rule of variable binding, then at least NP's (i.e. the

syntactic category corresponding to quantifiers over the domain of discourse) will occur in this position.

**U3. Determiner universal.** Every natural language contains basic expressions, (called determiners) whose semantic function is to assign to common count noun denotations (i.e., sets)  $A$  a quantifier that lives on  $A$ .

**U4. Constraint on determiners that can create undefined**

**NP's.** Let  $D$  represent a simple determiner such that  $||D||(A)$  is sometimes undefined.

1. Whenever  $||D||(A)$  is defined it is a sieve.
2. There is a simple determiner  $D+$  such that  $||D+||(A)$  is always defined and whenever  $||D||(A)$  is defined,  $||D||(A) = ||D+||(A)$ .

**U5. Monotonicity correspondence universal.** There is a simple NP which expresses the mon  $\downarrow$  quantifier  $\neg Q$  if and only if there is a simple NP with a weak non-cardinal determiner which expresses the mon  $\uparrow$  quantifier  $Q$ .

**U6. Monotonicity constraint.** The simple NP's of any natural language express monotone quantifiers or conjunctions of monotone quantifiers.

**U7. Strong determiner constraint.** In natural languages, positive strong determiners are monotone increasing. Negative strong determiners are monotone decreasing.

**U8. Persistent determiner universal.** Every persistent determiner of human language is mon  $\uparrow$  and weak.

**U9. Constraint on negating self-dual and mon  $\downarrow$  quantifiers.** If a language has a syntactic construction whose semantic

function is to negate a quantifier, then this construction will not be used with NP's expressing mon  $\downarrow$  or self-dual quantifiers.

**U10. Dual quantifier universal.** If a natural language has a basic determiner for each of  $D$  and  $\bar{D}$ , then these are semantically equivalent to 'some' and 'every'.

On the neo-Fregean account of quantification, U1 through U10 represent real constraints on the range of quantifiers and thus real limitations on the expressive power of natural languages. An example of a determiner which violates the living on condition -- and hence violates universal U3 -- is given above, and similar examples can readily be constructed for other of the universals. Barwise and Cooper take this contingency of U1 through U10 as an asset of their account, holding that it shows how the investigation of semantic phenomena can lead to empirically testable predictions about the range of natural languages.

It would be preferable, however, to see the range of natural languages not as *ad hoc* carved out of a larger class of possible formal languages, but as necessitated by the nature of the logical devices employed in those languages. I would take it, then, to count heavily in favor of a theory of quantification that it could *predict*, rather than merely *stipulate*, U1 through U10 (as well, of course, as other distinctive features of natural language quantification). In the next two sections, I will show that two of these universals do follow from the way distribution is defined in the anaphoric account. My suspicion is that many, if not all, of the rest of Barwise and Cooper's universals admit similar 'proofs' under my account, but (as these other universals and their corresponding taxonomic properties are of lesser interest for

the project at hand) we will not attempt the wholesale derivation here.<sup>360</sup>

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<sup>360</sup>Some scattered remarks on various of the universals:

(U1), (U2) Both of these universals are strongly supported by the picture of the relation between quantification and natural language proposed in chapter 2.

(U4) Barwise and Cooper claim that any determiner which can create undefined quantifiers is always a sieve when it is defined. As a devout Russellian, I don't believe there are any undefined quantifiers, and my system does not contain any. I thus find it a bit difficult to evaluate U4 in full generality, not knowing which quantifiers might be taken by others as undefined. Assuming, however, that the partially defined determiners are just the Russellian determiners (and Barwise and Cooper give us no reason to assume otherwise), we consider the reasons why a quantifier might be considered undefined, and see why U4 might look like a reasonable generalization (provided one believed in undefined quantifiers). Undefined quantifiers occur, it seems, when an NP *which acts enough like a referring expression that we tend to think of it as such* fails to denote. We then presumably assimilate this failure of denotation with those failures of reference on the part of proper names which give rise to semantic anomaly.

From this general characterization of undefined quantifiers, we can draw some specific conclusions. First, when a partially defined determiner does give rise to a defined quantifier, that quantifier is of the type which we tend to take as referential, so there must be some individual or individuals which we take as the 'referent' of the quantifier (although, of course, we may not know *which* individuals fit the bill). Put in the terminology of Barwise and Cooper, when a partially defined determiner D gives rise to a defined quantifier D(A), there must be some individual x such that:

$$(FN 196) \forall Y \in ||D||(A), x \in Y$$

Since, given any individual x, there will be some predicates which hold of x and others which don't, it follows immediately that  $||D||(A)$  will be a sieve. This accounts for the first half of U4.

Now, why should there be a simple totally defined determiner corresponding to each partially defined determiner in the way described? Well, note first that that determiner is in fact *always* 'every' (see below for the case of 'neither'). If these potentially undefined quantifiers are to be taken as referential, then it must be assumed that there is something definite to which they refer. Moreover, what they refer to must be a function of what noun is governed by the determiner. But the only way a definite reference can be obtained, given that the relevant noun may pick out several objects with no way to choose among them, is to have the quantifier refer to all the objects satisfying the noun. Thus the quantifier, when defined (i.e., denoting), acts like a universal quantification. Since 'every' itself is a totally defined determiner, the second half of U4 follows.

Put more succinctly, the two halves of U4 follow from the existential and universal components, respectively, which Russell identified in definite-description-like quantifiers. The above analysis needs slight modification in the case of 'neither', which is clearly analyzable as 'both not'. Here there are 'referents' which must not be in the sets denoted by the quantifier. An interesting linguistic question is why there is no singular analog to 'neither'.



### §3.3.1.3.1 Monotonicity and Distribution

Recall from above that the application of the distributor 'two' in:

(414) Two books are on the table.

treated the distributor 'two' (on both the weak and the strong reading) as the monotone increasing distributor 'at least two', as opposed to the non-monotonic distributor 'exactly two'. It treated (414), that is, as:

(432)  $[\exists x: \text{book } x]_{\text{on-the-table}} x$

rather than as:

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(U7) Barwise and Cooper claim that in natural language, all positive strong determiners are  $\text{mon } \uparrow$  and all negative strong determiners are  $\text{mon } \downarrow$ . Note that determiners in natural language seem generally to serve one of two types of function. We have *cardinality* quantifiers, which impose minimal or maximal conditions on the raw number of objects which must have a certain property. We also have *ratio* quantifiers, which impose minimal or maximal conditions on the percentage of objects which must behave in a certain way. Thus 'some' and 'many' are cardinality quantifiers, while 'every' and 'most' are ratio quantifiers. Note furthermore that any cardinality quantifier is weak, since when A is of the wrong size,  $A \notin ||D||(A)$ . Ratio quantifiers, on the other hand, will all be either positive or negative strong (depending on whether they impose a minimal or a maximal percentage). So a positive strong determiner in natural language is one which imposes a minimal condition on the percentage of objects which must have a certain property. Obviously, if we expand the requisite property, we are guaranteed not to contract the percentage of objects which have that property, so the 'minimal percentage' ratio quantifier will be  $\text{mon } \uparrow$ . Similarly, the 'maximal percentage' ratio quantifier will be  $\text{mon } \downarrow$ .

(U8) Assume we have a persistent determiner D. Then  $A \subseteq B$  implies that  $||D||(A) \subseteq ||D||(B)$ . But then D cannot be a ratio determiner. If D requires that a certain minimal percentage of the quantified objects have a certain property, then expanding the number of objects cannot guarantee preservation of that ratio. But if D is a cardinality quantifier, then expansion of the number of objects will preserve at least the minimally required raw number of objects having the requisite property. Thus cardinality quantifiers, and not ratio quantifiers, will be persistent. As observed earlier, strong quantifiers are ratio quantifiers and weak quantifiers are cardinality quantifiers, so persistency implies weakness.

(U9) That quantifier negation will not be used with  $\text{mon } \downarrow$  quantifiers is not surprising, given that, as shown below, all basic quantifiers in my system are  $\text{mon } \uparrow$ . Thus any  $\text{mon } \downarrow$  quantifier is already an implicit negation of some  $\text{mon } \uparrow$  quantifier, and rather than negating it, it would be more efficient simply to use the unnegated  $\text{mon } \uparrow$  quantifier. Similar efficiency considerations obvious weigh against using negations of self-dual quantifiers, since the negation is equivalent to the unnegated form.

(433)  $[\exists x: \text{book } x] \text{on-the-table } x$

The monotone increasing sense of 'two', of course, is a perfectly legitimate one, but what distribution rule would we use to capture the non-monotonic sense of '(exactly) two'?

Surprisingly, my system does not allow for such a reading. In fact, my system will not allow for any but monotone increasing distributors. Consider the sentence:

(434) Few men are philosophers.

along with what ought, *prima facie*, to be the distribution rule for 'few':

(435)  $D_{\text{few}}(X) = \{Y \mid Y \subseteq X \text{ and } Y \text{ has few members}\}$

Let  $R$  be a plural referring expression referring to all men. Then:

(436)  $D_{\text{few}}(\text{ref}(R)) = \{\text{ref}(R_1), \text{ref}(R_2), \dots, \text{ref}(R_n)\}$

where each  $R_i$  refers to some few men. So (434) will be true if some sentence of the form:

(437)  $R_i$  are philosophers.

is true. But now assume  $R_1$  refers to John Searle and Stephen Neale. John Searle and Stephen Neale are few men, so  $\text{ref}(R_1)$  is in the distributed reference of  $R$ . Furthermore, it's true that John Searle and Stephen Neale are philosophers. So (437) comes out true -- even though there are thousands of other men who are philosophers.<sup>361</sup> This is not the desired result.

In fact, there is an easy proof that all distributors in my system are monotone increasing:

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<sup>361</sup>Regardless, in fact, of *how many* men other than John Searle and Stephen Neale are philosophers.

**Claim:** If  $D$  is a distributor with distribution function  $D_D$ , then  $D$  is monotone increasing.

**Proof:** Assume  $x$  to have been restricted to an arbitrary plural referring expression, and take arbitrary open formulae  $\varphi(x)$  and  $\psi(x)$  such that  $\text{ext}(\varphi(x)) \subseteq \text{ext}(\psi(x))$ . Now assume:

$(Dx)(\varphi(x))$  is true.

Then if  $D_D(x) = \{\text{ref}(X_1), \text{ref}(X_2), \dots, \text{ref}(X_n), \dots\}$

for some plural referring expressions  $X_1, \dots, X_n, \dots$ , then:

$\varphi(X_i)$

must be true for some  $i$ . But  $\varphi(X_i)$  is true iff  $\text{ref}(X_i) \subseteq \text{ext}(\varphi(x))$ , so since  $\text{ext}(\varphi(x)) \subseteq \text{ext}(\psi(x))$  we have  $\text{ref}(X_i) \subseteq \text{ext}(\psi(x))$  and hence  $\psi(X_i)$  true. But if  $\psi(X_i)$  is true, then  $\psi$  is true of some element of the  $D$ -distributed reference of  $x$ , so:

$(Dx)(\psi(x))$

is true. This shows that  $D$  is monotone increasing.

The way in which I have implemented distributors, as creators of referring expressions (rather than as creators of quantifiers-as-functions-on-sets), makes it impossible for there to be non-monotone-increasing determiners.

The loss of expressive power, however, is not so severe as it might first appear. Barwise and Cooper note that all monotone decreasing quantifiers can be expressed through a combination of negation and monotone increasing quantifiers [186]. Thus sentences with monotone decreasing quantifiers like:

(438) Few men are tall

(439) No philosophers are immortal

(440) Cary Grant was in no more than four Hitchcock films.

can be rephrased, without alteration in truth conditions, as:

(441) It's not true that many men are tall.

(442) It's not true that some philosophers are immortal.

(443) It's not true that Cary Grant was in at least five Hitchcock  
films.

If distributors can be monotone increasing, then, and we have negation in the language -- which we do -- then we can express any monotone decreasing quantifier.<sup>362</sup>

This leaves only the non-monotonic quantifiers. Of these, many are expressible using Boolean combinations of monotone increasing and monotone decreasing quantifiers. Thus the non-monotonic:

(444) Exactly two men attended.

(445) Hitchcock directed between 44 and 57 films.

can be captured using monotone increasing and decreasing quantifiers:

(446) At least two men attended, and at most two men attended.

(447) Hitchcock directed at least 44 films, and at most 57 films.

and thus ultimately require only monotone increasing quantifiers plus a full array of sentence connectives.

Of course, there are plenty of non-monotonic quantifiers which cannot be reduced to Boolean combinations of mon  $\uparrow$  quantifiers.

Consider, for example:

(448) Exactly a prime number of elephants charged into view.

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<sup>362</sup>In addition to these two requirements, we also need the ability to group a negation operator with a particular quantifier. In classical (linearly-structured) syntaxes, this is unproblematic, but as we will see below, the issue is more complicated when we turn to branching structures.

Such quantifiers cannot be expressed in my system.<sup>363</sup> However, as we've just seen Barwise and Cooper posit (in universal U6) that no natural language will have simple determiners which cannot be expressed through a combination of mon ↑ quantifiers and sundry sentential connectives. If this claim is correct, then my system matches the expressive power of natural language 'simple' quantifiers. Moreover, my system *predicts* this linguistic universal, since it holds that *all* quantifiers are Boolean combinations of mon ↑ quantifiers. I suggest, then, that my system's inherent limitation to mon ↑ quantifiers is, far from being a defect of the system, a virtue which allows for an explanation, rather than a stipulation, of a feature of natural language quantification.

That monotone increasing quantifiers are the fundamental form of quantifiers mean that all monotone decreasing quantifiers are implicitly negated constructions.<sup>364</sup> There is in fact linguistic evidence supporting the claim that monotone decreasing quantifiers involve negation. 'Any' is sensitive in its interpretation to the presence of negation, typically receiving a universal reading in unnegated contexts and an existential reading in negated contexts.<sup>365</sup> Thus consider:

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<sup>363</sup>Although, of course, equivalent claims can be made through the introduction of adequate mathematical ontology. Once we have a set of prime numbers, we can say purely through the use of mon ↑ quantifiers that some member of that set numbers the elephants charging into view. I would tend to read (448) as having such set-theoretic commitment in it.

<sup>364</sup>Note that, had we had a system which took monotone decreasing quantifiers as basic, all monotone increasing quantifiers would be implicitly negated.

<sup>365</sup>Thus, for example, 'any' tends to receive an existential reading in the antecedent of a conditional, but a universal reading in the consequent of a conditional, since the equivalence:

$$(FN 197) P \rightarrow Q \Leftrightarrow \neg P \vee Q$$

shows the antecedent but not the consequent to be a negated position. Consider:

(FN 198) If anyone kills Patrokles, Achilles will kill anyone he sees.

(449) John will trust anyone.

(450) John won't trust anyone.

(449) calls for John to have universal trust, but (450) requires that there not be even one person that John trusts. Now contrast:

(451) Some people will trust anyone.

(452) Few people will trust anyone.

(451) requires that some people have universal trust, while (452) allows only that few people have even some trust. The presence of the monotone decreasing quantifier thus induces the existential reading of 'any'.

Note furthermore that 'any' tends to be highly ambiguous when combined with non-monotonic quantifiers. Thus consider:

(453) Exactly two people will trust anyone.

(453) can be heard either as asserting that there are exactly two people with universal trust or exactly two people with even some trust.<sup>366</sup>

Again, this result is what would be expected from a system which suggested that non-monotonic quantifiers had negated and unnegated components.

### **§3.3.1.3.2 Other Miscellaneous Results**

Barwise's universal U3 requires that all natural language determiners satisfy the living on condition. As mentioned above, for the neo-Fregean

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Two difficult cases are the behaviour of 'any' in questions and in the context of 'only'. Both of these contexts, despite not being obviously negated, induce existential readings. Thus compare:

(FN 199) Any communist is a spy.

(FN 200) Is any communist a spy?

(FN 201) John can answer any question.

(FN 202) Only John can answer any question.

<sup>366</sup>Although this is a hard phenomenon to pin down, there is some sort of gestalt shift in hearing these two readings. My suggestion is that, since 'exactly two' is, on my system, a conjunction of a negated and an unnegated quantifier, the gestalt shift involves allowing or disallowing the negation access to the 'any'. Obviously much more needs to be said here to make this suggestion helpful.

this claim is an empirical one; there are definable determiners for the neo-Fregean which do not satisfy the living on condition. As I have defined the process of distribution, however, it is necessary that all distributors satisfy the condition. I have required that all distributors map a plural reference to a collection of plural references in such a way that  $D_D(X) \subseteq X$ , and this requirement guarantees that each determiner produces quantifiers which live on their corresponding N' expression. This requirement, moreover, is not gratuitously imposed to ensure the truth of U3. Instead, it is a natural consequence of the intuitive picture of a distributor as taking a pre-existing reference and breaking it into smaller pieces according to some rule. Distribution of a whole cannot produce anything which way not in the whole to begin with. Once again, we see that certain claims which are empirical for the neo-Fregean follow for me from the very nature of quantification. Rather than massively overgenerating and then searching for methods to cull out the logical fauna of natural language, we seek a breed of quantifier such that natural language phenomena display its exact range of phenotypes.<sup>367</sup>

Of course, it is not entirely obvious that U3 is correct. There are some constructions in English which appear to be violations of the claim that all determiners satisfy the 'living on' constraint. Thus consider the behaviour of the word 'only'. In a sentence such as:

(454) Only philosophers understand Kant.

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<sup>367</sup>That every natural language contain distributors -- the second component of Barwise and Cooper's prediction U3 -- is not necessitated by my analysis of variable binding. However, the utility of the variable binding apparatus of restriction in a language lacking distribution would clearly be considerably reduced; it is not clear that such a language would possess the simple usability necessary to sustain a natural language.

the string 'only philosophers', if understood as a quantified noun phrase, cannot be understood as living on the set of philosophers. To see this, note that (454) is not equivalent to:

(455) Only philosophers are philosophers and understand Kant. 'Only', then, (in 'only philosophers') induces consideration of the behaviour of non-philosophers as well as that of philosophers, and thus does not satisfy the living on condition. But if words like 'only' (and 'just' and 'even') violate U3, then they also falsify the anaphoric account of quantification, since that account cannot allow distributors which violate the living on condition. There is no way 'only' could distribute a reference to all philosophers such that the resulting distribution could depend for its truth on the behaviour of non-philosophers.

Fortunately, there is good reason to doubt that 'only philosophers' is a quantified noun phrase. 'Only', rather than a determiner/distributor, seems to belong to some much more general syntactic and semantic category. Note that 'only' can also combine with both proper names and with quantified noun phrases:

(456) Only Achilles can kill Hector.

(457) Only a few Greeks fear Paris.

Other determiners do not share this feature:<sup>368</sup>

(458) \*Some Achilles can kill Hector.

(459) \*Every a few Greeks fear Paris.

'Only', in fact, can combine with *any* part of speech. Thus consider the following constructions:

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<sup>368</sup>For a discussion of apparent determiner-proper name combinations, see footnote 40 above.



(460) Achilles killed Hector *only because* Hector killed Patrokles.

(461) Achilles *only wounded* Hector.

(462) Achilles killed Hector *only yesterday*.

(463) Achilles is *only angry*.

(464) Achilles is *only angry at Agamemnon*.

(465) *Only if* Patrokles is killed will Achilles fight Hector.

It seems clear, then, that 'only' ought not be treated as a quantifier-forming determiner, and if it is not such, it can pose no threat to U3 or to the anaphoric account.<sup>369</sup>

The anaphoric account has little to say about the logicality of quantifiers or distributors. If, however, we accept the adequacy of the permutation invariance standard of logicality, we can impose this condition on the notion of distribution by requiring that, for any set  $X$  and any automorphism  $h$  on  $X$  and any subset  $Y$  of  $X$ ,  $D_D(Y) = D_D(h(Y))$ . As it stands, we can have distributors which, say, return the empty set if John Searle is among the things referred to, and return the original reference if John Searle is not in the reference. It strikes me as plausible (a) that only logical distributors are called for in the analysis of natural language and (b) that it is in the nature of distribution to have this sort of object-independence, but I will not argue for these claims here.

### **§3.3.2 Linear and Partial Ordering of Quantifiers**

One distinctive feature of quantifiers as they appear in sentences of classical first-order logic is that they are linearly ordered. They are linearly ordered in two, interrelated ways. First, and most obviously,

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<sup>369</sup>See [McCawley 1993] for more detailed arguments against a quantificational treatment of 'only'.

they are linearly ordered by the syntax of the language. Given any sentence  $\phi$  of first order logic of the form:

$$(466) (Q_1x_1)(Q_2x_2)\dots(Q_nx_n)\psi(x_1,\dots,x_n)^{370}$$

we can impose an order on the quantifiers through their scope by defining:

$$(\text{Def. 23}) (Q_ix_i) \geq (Q_jx_j) \text{ if } (Q_jx_j) \text{ is in the scope of } (Q_ix_i)$$

It's then a trivial consequence of the way scope is defined in classical logic that (a)  $(Q_ix_i) \geq (Q_jx_j)$  iff  $(Q_ix_i)$  appears to the left of  $(Q_jx_j)$ , and (b) hence ' $\geq$ ' imposes a linear ordering on the quantifiers of  $\phi$ . The quantifiers are also semantically linearly ordered, although specifying the exact nature of that linearity is more difficult than in the case of the syntactic linearity. However, they are ordered in at least the following minimal sense: given two sentences of the form:

$$(467) (Q_1 x_1)(Q_2 x_2)\phi$$

$$(468) (Q_2 x_2)(Q_1 x_1)\phi$$

we do not generally have (467) equivalent to (468).<sup>371</sup> The order in which quantifiers appear, that is, matters to the truth conditions of sentences containing those quantifiers.

Once one sees this linearity feature of classical logic, it becomes natural to wonder if it expresses an essential feature of

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<sup>370</sup>I am interested here in occurrences of quantifiers, not in types of quantifiers, so for some  $i, j$  we might have  $Q_i = Q_j$  and  $x_i = x_j$ .

<sup>371</sup>Note, as is observed by [Van Bentham 1989], that there can be order dependence between quantifiers even when they are of the same type. While within the classical system we always have:

$$(\text{FN 203}) (\forall x)(\forall y)\phi(x,y) \leftrightarrow (\forall y)(\forall x)\phi(x,y)$$

$$(\text{FN 204}) (\exists x)(\exists y)\phi(x,y) \leftrightarrow (\exists y)(\exists x)\phi(x,y)$$

this interchangeability is in fact a rare property of generalized quantifiers. Thus the following are not equivalent:

$$(\text{FN 205}) (\text{Most } x)(\text{Most } y)\phi(x,y)$$

$$(\text{FN 206}) (\text{Most } y)(\text{Most } x)\phi(x,y)$$

See §3.3.2.2.2.1.1 below for a demonstration of their nonequivalence.

quantification. The seeming parochiality of the syntactic linearity, a consequence at least in part of the contingent convention of writing out formulae in straight lines, combined with the ease with which we can eliminate that parochiality, either by explicitly introducing non-linear writing conventions such as:

$$(469) \begin{array}{l|l} (Q_1x_1) & \\ (Q_2x_2) & \\ \dots & \Psi(x_1, \dots, x_n) \\ (Q_nx_n) & \end{array}$$

or by indicating quantifier scope not through physical position but through an indexing system, as in:

$$(470) (Q_1x_1)^i(Q_2x_2)^i \dots (Q_nx_n)^i [\Psi(x_1, \dots, x_n)]_i$$

makes it tempting to think that there must be a concept of the variable binding operator which allows for the possibility of non-linearity, or of branching.

I want to enter into an extensive discussion of the possibility of having non-linearly ordered quantifier prefixes. In part, the goal here will be to show that the anaphoric account leads to a branching semantics in a more natural and satisfying way than other semantic accounts of quantification and variable binding. Additionally, we will along the way reach some conclusions about what it is about quantification that makes it linearly ordered, when it is so, and in reaching these conclusions we will gain insight into some underlying features of quantification which help explain the appeal of the Fregean shift from quantifiers-as-noun-phrases to quantifiers-as-sentential operators.

### §3.3.2.1 The Syntax of Branching Quantification

The first difficulty in trying to give up the assumption that quantification is linearly ordered is that it is not clear what we are supposed to replace that assumption with. This is a problem for syntax as well as semantics: it is not entirely clear what even the branching syntax is, let alone the branching semantics. We want to allow non-linearly ordered quantifier prefixes, but how far do we want to take this? Presumably we want to allow for more than purely unordered prefix, countenancing structures such as:

$$\begin{array}{r}
 (Q_1 x)(Q_2 y) \setminus \\
 \qquad \qquad \qquad \setminus \\
 \qquad \qquad \qquad \qquad \setminus \\
 (471) \qquad \qquad \qquad \qquad \qquad (Q_6 v) \varphi(x,y,z,w,u,v) \\
 (Q_3 z) \setminus \qquad \qquad \qquad / \\
 \qquad \qquad \qquad \setminus \qquad \qquad / \\
 \qquad \qquad \qquad \qquad (Q_5 u) / \\
 \qquad \qquad \qquad / \\
 (Q_4 w) /
 \end{array}$$

But do we also want to allow for, say, quantifier prefixes which order themselves circularly, or for quantifier prefixes which branch inward, or for completely unordered quantifiers, all of which are exhibited in:

$$\begin{array}{c}
 \varphi(x, v) \\
 / \\
 (Q_1 x) \\
 \backslash \\
 (472) \quad (Q_2 y) \dashrightarrow (Q_3 z) \\
 \quad \quad \quad \wedge \quad \quad | \\
 (Q_6 v) \quad | \quad \quad | \dashrightarrow \psi(x, y, z, w, u, v) \\
 \quad \quad \quad | \quad \quad \vee \\
 \quad \quad \quad (Q_5 u) \dashrightarrow (Q_4 w)
 \end{array}$$

We might also wonder whether we could use various Boolean connectives within branching structures, creating forms such as:

$$\begin{array}{c}
 (Q_1 x) \backslash \\
 \quad \quad \quad \backslash \\
 (473) \quad \text{---} \quad \text{---} \quad \varphi(x, y) \\
 \quad \quad \quad \text{---} \quad \text{---} \\
 \quad \quad \quad / \\
 (Q_2 y) /
 \end{array}$$

or:

$$\begin{array}{c}
 \neg(Q_1 x) \backslash \\
 \quad \quad \quad \backslash \\
 (474) \quad \varphi(x, y) \\
 \quad \quad \quad / \\
 (Q_2 y) /
 \end{array}$$

or:

$$((\exists x)\psi(z) \rightarrow (Q_1 x)) \backslash$$

$$(475) \quad \begin{array}{c} \backslash \\ \varphi(x,y) \\ / \\ (Q_2 y) / \end{array}$$

With additional creativity, even more recondite structures can be presented. Which of these need fall under the umbrella of our account of 'branching' quantification (or, indeed, of our account of quantification in general)?

The standard accounts of branching quantifiers address at most a syntax obtained by replacing the normal syntactic rule:

**(S-Q)** If  $\varphi$  is a formula,  $\chi$  is a variable, and  $Q$  is a quantifier, then ' $(Q \chi) \varphi$ ' is a formula

with the rule:

**(S-POQ)** If  $\varphi$  is a formula,  $\chi_1, \dots, \chi_n$  are variables,  $Q_1, \dots, Q_n$  are quantifiers, and  $>_p$  is a partial order on  $\{1, \dots, n\}$ , then:

$$\begin{array}{c} (Q_i \chi_i) \backslash \\ \dots \\ \dots \quad \varphi \\ \dots \\ (Q_j \chi_j) / \end{array}$$

is a formula, where a path from  $(Q_i \chi_i)$  to  $\varphi$  passes through  $(Q_k \chi_k)$  iff  $i >_p k$ .

(S-POQ) allows forms such as (469), (471), and (473) above, but rules out the more recondite (472) and, by introducing partially ordered prefixes only *en masse*, (474) and (475). More usually, however, treatments of branching weaken (S-POQ) somewhat by allowing the

introduction of partially ordered prefixes only in the final stage of formula composition, thus ruling out (473) as well. It is this weakened rule that gives rise to the language called (from [Enderton 1970]) FOQ.

One is of course free to design the syntax of one's language in whatever way one pleases, but if we *arbitrarily* limit ourselves to the cases allowed by (S-POQ)<sup>372</sup> it will be difficult to believe that we have developed a system which fully captures the notion of the partially ordered quantifier. What we would like, ideally, is a system which explains to us why the range of available syntactic forms is what it is, and then tells us what those forms mean. That explanation could build off of syntactic or semantic concepts, although the ideal explanation would show us how the two necessarily interact.

#### **§3.3.2.2 The Semantics of Branching Quantification**

I propose that we put these worries about the syntax of branching on hold while we move to thinking about the *semantics* of branching. We will return (in §3.3.2.2.3.3.2.1 below) to the question of the range of syntactic constructions to which a theory of quantification ought to be held responsible, but there our goal will be to have already in hand an independently motivated story about the semantics of linearity and non-linearity in quantification, a story which will help motivate the syntactic constraints. For now, our sole lesson from the above syntactic worries will be to keep firmly in mind that the appropriate subject matter of branching quantification is by no means a predefined target -- any account which blithely announces a preferred domain of branching structures, or which presupposes in its semantic applicability such a

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<sup>372</sup>Or its weakened form generating FOQ.

domain, without explaining why non-linearity should extend that far and no farther, is to be greeted with skepticism.

#### **§3.3.2.2.1 In Search of a Branching Semantics**

Most of those who write on branching quantification are interested in the applicability of branching structures to the logical analysis of natural language. Thus there are typically, following [Hintikka 1973], claims that certain constructions in English require or at least profit from analysis in terms of partially ordered quantification. Because these claims of linguistic utility are made, we can (and frequently will, in the subsequent discussion) attack this or that proposed semantic analysis of branching on the grounds that the truth conditions it predicts for certain natural language sentences are highly unnatural. However, there is a more fundamental level of criticism of many accounts of branching quantification: they give us no explanation of *why* the account they give is the right way to think about branching quantification, but merely posit some semantic analysis or another. And, as we will see, it is by no means obvious *what* the right way is.

##### **§3.3.2.2.1.1 A Prima Facie Problem**

Even if we set aside the syntactic worries of §3.3.2.1 and assume that we are broadly interested in structures in which quantifiers can be partially, rather than linearly, ordered (without worrying about the details), the difficulty is in translating this syntactic interest to a semantic one. The naive approach to the semantics is to say that, just as the ordering of the quantifiers *doesn't matter* to the syntax of the sentence, it should not matter to the semantics of the sentence. The



problem is, though, that the order *does* matter. I know what it is to give up my normal syntactic view that:

$$(476) (\exists x)(\forall y)Fxy$$

is a different formula from:

$$(477) (\forall y)(\exists x)Fxy$$

and accept that the following are mere notational variants:

$$(478) \begin{array}{l} (\exists x) \quad | \\ \quad \quad | Fxy \\ (\forall y) \quad | \end{array}$$

and:

$$(479) \begin{array}{l} (\forall y) \quad | \\ \quad \quad | Fxy \\ (\exists x) \quad | \end{array}$$

but how am I supposed to give up my view that (476) and (477) have different truth conditions?

Consider the analogy with sentential connectives. Say, after reflecting on the symmetry of 'and' and 'or', that we decided we ought to abandon the linear joining structure of sentential connectives (which, after all, just gives rise to those deceptive Gricean implicatures that the 'p' in 'p and q' happened before 'q', and so on) and adopt a partially ordered syntax for the connectives. We would thus abandon the formation rule:

$$(R1) \text{ If } P \text{ and } Q \text{ are wffs, then } 'P \wedge Q' \text{ is a wff}$$

in favor of the formation rule:

$$(R2) \text{ If } P \text{ and } Q \text{ are wffs, then:}$$

$$\begin{array}{l} P \quad | \\ \quad \quad | \wedge \end{array}$$

$Q \mid$

is a wff.

where the line indicates that the prefix to the conjunction is only partially ordered. We can now give a perfectly adequate semantics for the partially ordered conjunction:

(AX26) Given any set  $X$  of sentences, ' $X \mid \wedge$ ' is true iff every member of  $X$  is true.

Note that no ordering on the conjuncts is necessary in order to make this semantic clause work. Similarly, we can say that a partially ordered disjunction ' $X \mid \vee$ ' is true if some member of the partially ordered set of disjuncts is true.

But now, unsurprisingly, consider the conditional. What happens if we try to use partially ordered components in front of the conditional? The syntax is still perfectly straight-forward: we have the following formation rule:

(R3) If  $P$  and  $Q$  are wffs, then:

$P \mid$   
 $\mid \rightarrow$   
 $Q \mid$

is a wff

But what kind of semantics can we give for this formula? None that will both respect the original meaning of the conditional and the partially ordered nature of the connective prefix -- precisely because the conditional *is in fact* sensitive to the order in which the sentential components appear. There's no point in searching for a partially ordered understanding of the conditional, because the very logic of the conditional makes it impossible that there could be such a thing.

The lesson here is, I hope, clear. We were able to impose a partially ordered structure onto conjunction and disjunction precisely because these connectives have underlying logics which are not order-dependent -- that is, because 'p & q' and 'q & p' are logically equivalent regardless of the particular identity of p and q. Similarly, there are cases in which the logic tells us that we don't need to look at the order of quantifiers. If we have the formula:

$$(480) (\forall x)(\forall y)Fxy$$

we know that is logically equivalent to the formula:

$$(481) (\forall y)(\forall x)Fxy$$

so if we want to insist on a partially ordered syntactic form for quantifier prefixes consisting entirely of universal quantifiers, we will be able to make sense of it. The form:

$$(482) \begin{array}{l} (\forall x) \quad | \\ \quad \quad | \\ \quad \quad | \quad Fxy \\ (\forall y) \quad | \end{array}$$

will be satisfied by a sequence  $\sigma$  if every sequence  $\sigma'$  differing from  $\sigma$  in at most the x and y positions satisfies 'Fxy'. But this is a fluke feature of the universal quantifier. If one picks a quantifier, or collection of quantifiers, in which one can't simply ignore the order in which the quantifiers appear, then saying that we want to make the choice of x independent of the choice of y may simply make no sense at all.

An adequate semantics of branching, then, must give us some explanation of how it is that we are able to treat quantifiers, whose linear order previously mattered to truth conditions, as newly unordered. It must specify what core notion of these quantifiers

persists through both their linearly ordered and their partially ordered deployments.

#### **§3.3.2.2.1.2 Two Desiderata for a Semantics**

In order to guide us on our quest for an acceptable semantics for branching quantification, I want to offer two methodological desiderata which hopefully will couple with those meager intuitions on appropriate truth conditions of branching structures we can muster (mostly, as stated earlier, on the basis of proposed correlations between natural language constructions and formal sentences with partially-ordered quantifiers) to yield substantive tools for deciding among positions. The first desiderata follows from the considerations of the previous section:

**(Contiguity)** The branching semantics should follow naturally from a general conception of quantifiers. There should not be one semantics for linear quantification and another for branching quantification, nor should linear quantification be merely a special case of branching quantification without some deeper explanation of why that branching semantics should be the way it is.

In addition to the requirement of Contiguity, I also want to place the following second requirement of Simplicity:

**(Simplicity)** Since linearly ordered quantification is the result of the imposition of a higher degree of structure onto the language than partially ordered quantification, the branching semantics should be, in some real sense, simpler than the linear semantics.

One should, I think, expect that the more syntactically structured form of linear quantification would be accompanied by the employment of additional logical mechanisms. We will find, however, that few accounts respect this condition.

### §3.3.2.2.2 Some Problems with Some Proposed Semantics

Before turning to my positive suggestions on the interpretation of partially ordered quantificational structures, I want to review what I take to be the major camps in the literature and give some indication of the deficiencies of each. Three major approaches to branching quantification have by now developed. The oldest of these, deriving from [Henkin 1959], involves the use of Skolem functions to simulate the intuitively required degree of quantifier independence in partially ordered prefixes. The second tradition, deriving from [Hintikka 1973], attempts to situate the Skolemization semantics within the framework of game-theoretic semantics. The third, exemplified in [Barwise 1979] and [Sher 1990], use the resources of the neo-Fregean account of quantification (see §1.3) to devise semantics for branching.<sup>373</sup>

#### §3.3.2.2.2.1 Skolemization Semantics

Any sentence of classical quantified logic is equivalent to some second-order sentence in *Skolem normal form*. A sentence is in Skolem normal form if it is of the form:

$$(483) (\exists f_1) \dots (\exists f_n) (\forall x_1) \dots (\forall x_m) \phi(f_1, \dots, f_n, x_1, \dots, x_m)$$

---

<sup>373</sup>Skolemization semantics are developed in [Henkin 1959], [Walkoe 1970], and [Enderton 1970]. GTS-based semantics can be found in [Hintikka 1973], [Hintikka 1976], and [Hand 1993]. Neo-Fregean accounts include [Barwise 1979], [Westerståhl 1987], [Van Bentham 1989], and [Sher 1990]. Other discussion of branching quantifiers, including both critical discussion and some idiosyncratic approaches, include [Quine 1972], [Fauconnier 1975], [Stenius 1976], [Humberstone 1987], [Patton 1989], and [Patton 1991].

where  $f_1, \dots, f_n$  are second-order variables ranging over functions and  $\varphi$  is a quantifier-free formula. Thus for example:

$$(484) (\forall x)(\exists y)Fxy$$

is equivalent to:

$$(485) (\exists f)(\forall x)F(x, f(x))$$

since, when (484) is true, we can let  $f$  be that function which picks out, for each choice of  $x$ , some object  $y$  to which  $x$  bears the relationship  $F$ .

In general in Skolem normal forms first-order existential quantifiers over objects within the scope of universal quantifiers are replaced by wide scope second-order existential quantifiers over functions, where those functions act to pick out, for given choices of objects corresponding to the universal quantifiers, appropriate objects. Thus the classical:

$$(486) (\forall x)(\exists y)(\forall z)(\exists w)Rxyzw$$

is equivalent to the Skolem normal form:

$$(487) (\exists f_1)(\exists f_2)(\forall x)(\forall z)R(x, f_1(x), z, f_2(z, w))$$

Here the function  $f_1$  is a function only of  $x$ , since the ' $(\exists y)$ ' quantifier is in the scope of the ' $(\forall x)$ ' but not the ' $(\forall z)$ ' quantifier, while the function  $f_2$  is a function of  $x$  and  $z$ , since the ' $(\exists w)$ ' quantifier has smallest scope. The arguments of the Skolem function in the Skolem normal form, then, track the dependencies of the existential quantifier in the original classical sentence.

While every sentence of classical first-order logic has a Skolem normal form, not every sentence in Skolem normal form corresponds to a sentence of first-order logic. Following [Henkin 1959], we can identify

the following necessary and sufficient condition for a Skolem normal form representing a classical formula:

The functional existential quantifiers  $(\exists f_1), \dots, (\exists f_m)$  can be ordered in such a way that for all  $1 \leq i, j \leq m$ , if  $(\exists f_i)$  syntactically precedes  $(\exists f_j)$ , then the set of arguments of  $f_i$  in  $\phi$  is essentially included in the set of arguments of  $f_j$  in  $\phi$ . [Sher 1990, 107]

Thus, for example, the following Skolem normal form does not correspond to any classical sentence:

$$(488) \quad (\exists f_1)(\exists f_2)(\forall x)(\forall y)F(x, y, f_1(x), f_2(y))$$

Following our earlier explanation, we would like to say that this sentence corresponds to a first-order sentence in which one existential quantifier is in the scope only of the ' $(\forall x)$ ' quantifier while the second existential quantifier is in the scope only of the ' $(\forall z)$ ' quantifier. But, of course, no such first-order sentence can be written down.

Not, at least, if we assume that quantifier prefixes are linearly ordered. However, if we relax the linear ordering requirement, we can give syntactic structures which meet the scope conditions set out above. The most obvious is the following branching structure:

$$(489) \quad \begin{array}{c} (\forall x) \text{ -- } (\exists z) \backslash \\ \qquad \qquad \qquad \qquad \qquad \backslash \\ \qquad \qquad \qquad \qquad \qquad \qquad \qquad F(x, y, z, w) \\ \qquad \qquad \qquad \qquad \qquad \qquad \qquad / \\ (\forall y) \text{ -- } (\exists w) / \end{array}$$

The proposal, then, is that those Skolem normal forms which do not correspond to (linear) first-order sentences correspond to branching structures.

### §3.3.2.2.2.1.1 Skolemization and Generalized Quantifiers

The Skolemization semantics makes only the smallest of gestures toward compliance with the Contiguity requirement. The branching treatment of quantifiers can be seen as a natural outgrowth of, or an alternative expression of an underlying commonality to, classical linear quantification only if we assume that all classical first-order quantification, whether linear or branching, is *really* second-order quantification involving Skolem functions -- otherwise the move to Skolemized semantics in the branching, but not the linear, case is ad hoc. However, this is not a plausible interpretation of the nature of quantification.

In the classical case, it is at least true that all linear formulae have Skolemized equivalents. However, once we introduce generalized quantifiers, this equivalence breaks down. In fact, the utility of the Skolemization semantics for branching constructions depends on three assumptions about the nature of quantification, assumptions which are peculiar to the classical quantifiers. First, we need to assume that relativization of one quantifier to another can be re-expressed on the second-order level through functional expression of that relativization. Second, and relatedly, we need to assume that smaller scoped quantifiers serve the role of allowing the choice of particular objects to which objects ranged over by larger scoped quantifiers bear the relevant relations. Third, we need to assume that scope distinctions among like-typed quantifiers are truth-functionally (and more broadly semantically) irrelevant. None of these assumptions hold of generalized quantifiers.



In classical systems, the implicit process of a choice of some object for each object induced by the presence of an existential quantifier in the scope of a universal quantifier can be replaced by an initial choice of a function specifying the necessary choice for each value hit by the larger-scoped universal. With some generalized quantifiers, this functional analysis of relativization carries over.

Thus a first-order sentence with generalized quantifiers like:

$$(490) \text{ (MOST } x)(\exists y)Fxy$$

is equivalent to:

$$(491) (\exists f)(\text{MOST } x)F(x, f(x))$$

However, there is not always a second-order Skolemization available.

Thus consider the relation between:

$$(492) \text{ (FEW } x)(\exists y)Fxy$$

and:

$$(493) (\exists f)(\text{FEW } x)F(x, f(x))$$

These two are not equivalent. Even if each object in the domain bears F to some object (thus making (492) false), (493) can still be true if we simply choose a function f which for any value of x maps x to some object to which x does not bear F.<sup>374</sup>

Skolem normal forms are available only when the quantifier having large scope over the existential quantifier is a monotone increasing

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<sup>374</sup>Similarly, we can see that:

$$(FN 207) \text{ (NO } x)(\exists y)Fxy$$

is not equivalent to:

$$(FN 208) (\exists f)(\text{NO } x)F(x, f(x))$$

The difficulty, then, is not due entirely to the introduction of quantifiers which extend beyond the expressive reach of classical quantification, since 'NO' is classically definable. The Skolemization semantics needs (and lacks) some explanation of why apparently notational differences between a 'NO' quantifier and a negated existential quantifier make crucial differences in the sensibility of branching structures.

quantifier.<sup>375</sup> Thus the Skolemization semantics here does not get hold of a deep or fundamental feature of quantifiers, but depends for its function on special peculiarities of the classical system.<sup>376</sup>

The second way in which Skolemization fails when generalized quantifiers are added manifests itself when we place other than existential quantifiers in the small scope position. Thus consider the process of determining a Skolem normal form for:

$$(494) (\text{MOST } x)(\exists y)Fxy$$

Obviously we cannot simply use:

$$(491) (\exists f)(\text{MOST } x)F(x, f(x))$$

since this is equivalent to (490) above, and does not guarantee that most objects bear  $F$  to at least two objects. But it doesn't help to reuse the small-scoped quantifier in the Skolemization:

$$(495) (\exists f)(\text{MOST } x)F(x, f(x))$$

unless we add in some apparently unmotivated requirement that the two functions quantified over be everywhere distinct in their values.

The third and final problem introduced by extending Skolemization to generalized quantifiers is that generalized quantifiers generally do not have the feature of logical interchangeability of like-typed quantifiers. In a classical system, we have the following equivalences:

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<sup>375</sup>When the large-scope quantifier is monotone decreasing, as in (492) above, we can give a simulated Skolem normal form by using universal quantification over functions:

$$(\text{FN } 209) (\forall f)(\text{FEW } x)F(x, f(x))$$

This tactic, however, will not work when we have quantifiers of mixed monotonicity.

<sup>376</sup>I will argue, however, in §§3.3.2.2.2.3.1, 3.3.2.2.3.2.2 that there are good reasons to think that branching quantification is sensitive to the monotonicity properties of the quantifiers involved. However, the Skolemization semantics, unlike my semantics, still has no explanation of that sensitivity, since the Skolemization semantics is not -- at least as stated -- tied together with an account of quantification which gives special weight to monotonicity properties.

$$(496) (\forall x)(\forall y)Fxy \leftrightarrow (\forall y)(\forall x)Fxy$$

$$(497) (\exists x)(\exists y)Fxy \leftrightarrow (\exists y)(\exists x)Fxy$$

It is this interchangeability which allows us to take the Skolem normal form:

$$(488) (\exists f_1)(\exists f_2)(\forall x)(\forall y)F(x,y,f_1(x),f_2(y))$$

as a reasonable analysis of the branching structure:

$$(489) \begin{array}{c} (\forall x) \text{ -- } (\exists z) \backslash \\ \phantom{(\forall x) \text{ -- } (\exists z)} \phantom{\text{ -- }} \phantom{(\exists z)} \backslash \\ \phantom{(\forall x) \text{ -- } (\exists z)} \phantom{\text{ -- }} \phantom{(\exists z)} \phantom{\backslash} F(x,y,z,w) \\ \phantom{(\forall x) \text{ -- } (\exists z)} \phantom{\text{ -- }} \phantom{(\exists z)} / \\ (\forall y) \text{ -- } (\exists w) / \end{array}$$

since the universal quantifiers are no more semantically ordered in (488) than in (489) (given their interchangeability), and similarly for the existential quantifiers. But an attempt to capture the generalized branching structure:

$$(498) \begin{array}{c} (\text{MOST } x) \text{ -- } (\exists z) \backslash \\ \phantom{(\text{MOST } x) \text{ -- } (\exists z)} \phantom{\text{ -- }} \phantom{(\exists z)} \backslash \\ \phantom{(\text{MOST } x) \text{ -- } (\exists z)} \phantom{\text{ -- }} \phantom{(\exists z)} \phantom{\backslash} F(x,y,z,w) \\ \phantom{(\text{MOST } x) \text{ -- } (\exists z)} \phantom{\text{ -- }} \phantom{(\exists z)} / \\ (\text{MOST } y) \text{ -- } (\exists w) / \end{array}$$

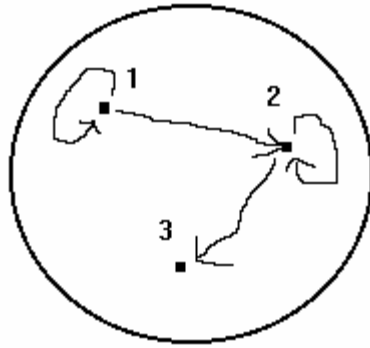
through the Skolemization:

$$(499) (\exists f_1)(\exists f_2)(\text{MOST } x)(\text{MOST } y)F(x,y,f_1(x),f_2(y))$$

(even picking a case in which Skolemization is possible) runs up against the problem that 'MOST' quantifiers are not, like classical quantifiers, interchangeable. We do not have:

$$(500) (\text{MOST } x)(\text{MOST } y)Fxy \leftrightarrow (\text{MOST } y)(\text{MOST } x)Fxy$$

To see this, consider an interpretation with the following structure:



Here 1 bears the relation to 1 and 2, and 2 bears the relation to 2 and 3, so most objects bear the relation to most objects:

$$(501) \text{ (MOST } x)(\text{MOST } y)Fxy$$

However, of the three points, only 2 is borne the relation by more than one object, so it is not true that most objects are borne the relation by most objects; the following is false:

$$(502) \text{ (MOST } y)(\text{MOST } x)Fxy$$

Thus the proposed Skolemization (499) is not equivalent to:

$$(503) (\exists f_1)(\exists f_2)(\text{MOST } y)(\text{MOST } z)F(x,y,f_1(x),f_2(y))$$

and neither can be a good analysis of the proposed branching structure, in which the two 'MOST' quantifiers are unordered with respect to each other.

### §3.3.2.2.2.1.2 Skolemization and Classical Quantifiers

The failure of Skolemization to support the addition of generalized quantifiers to the system is thus a sign that Contiguity has been flouted, and that the Skolemization semantics forces branched readings out through ad hoc mechanisms rather than through a deep understanding of how quantifier non-linearity can be possible. Even when we restrict ourselves to the realm of classical quantifiers, however, we can see

problems for the Skolemization semantics -- problems which this time come from disregard for the Simplicity requirement.

It seems clear to me that the Skolemization strategy is not attentive to the Simplicity requirement. Rather than seeing branched readings as the more fundamental starting point from which linear readings are derived through the imposition of additional logical structure, Skolemization takes linearity for granted (on the second-order level) and adds new logical complexity to the branched readings by way of moving from first-order quantification over objects to second-order quantification over functions.<sup>377</sup> By choosing this method of analysis, however, Skolemization makes itself suitable only for certain types of branching. Skolemization is well-suited to branching structures of the form:

$$\begin{array}{c}
 (\forall x) \text{ -- } (\exists z) \backslash \\
 \phantom{(\forall x) \text{ -- } (\exists z)} \phantom{\backslash} \\
 \phantom{(\forall x) \text{ -- } (\exists z)} \phantom{\backslash} \phantom{\backslash} \\
 \dots\dots\dots \phantom{\backslash} \\
 (504 \quad \dots\dots\dots \phantom{\dots} \phantom{F(x,y,z,w,\dots)} \\
 \phantom{(\forall x) \text{ -- } (\exists z)} \phantom{\backslash} \phantom{\backslash} \\
 \phantom{(\forall x) \text{ -- } (\exists z)} \phantom{\backslash} / \\
 (\forall y) \text{ -- } (\exists w) /
 \end{array}$$

in which branches contain universal-existential pairs, since universal-existential pairs can be interpreted as corresponding to ranges of

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<sup>377</sup>The one (inevitable) nod which Skolemization makes toward the Simplicity requirement is that linearity, when viewed on the level of Skolem normal forms, involves greater functional dependency than partiality. The fact remains, however, that *linear* readings of the Skolem normal forms are required to make sense even of branched readings of the target first-order sentences.

functions. However, Skolemization deals less well with other classical branching combinations, such as:

$$\begin{array}{l}
 (\forall x) \backslash \\
 \quad \backslash \\
 (505) \quad Fxy \\
 \quad / \\
 (\forall y)
 \end{array}$$

or:

$$\begin{array}{l}
 (\exists x) \backslash \\
 \quad \backslash \\
 (506) \quad Fxy \\
 \quad / \\
 (\exists y)
 \end{array}$$

or:

$$\begin{array}{l}
 (\exists x) \backslash \\
 \quad \backslash \\
 (507) \quad Fxy \\
 \quad / \\
 (\forall y)
 \end{array}$$

or:

$$\begin{array}{l}
 (\exists x) \text{ -- } (\forall z) \backslash \\
 \quad \quad \quad \quad \quad \quad \quad \backslash \\
 (508) \quad \quad \quad \quad \quad \quad Fxyzw \\
 \quad \quad \quad \quad \quad \quad \quad / \\
 (\exists y) \text{ -- } (\forall w) /
 \end{array}$$

none of which fall naturally into functional language.

Of course, it is not at all clear what the right analysis of any of these claims ought to be. [Walkoe 1970] and [Barwise 1979] both take it that these expressions are all equivalent to linear first-order expressions:

(505')  $(\forall x)(\forall y)Fxy$

(506')  $(\exists x)(\exists y)Fxy$

(507')  $(\exists x)(\forall y)Fxy$

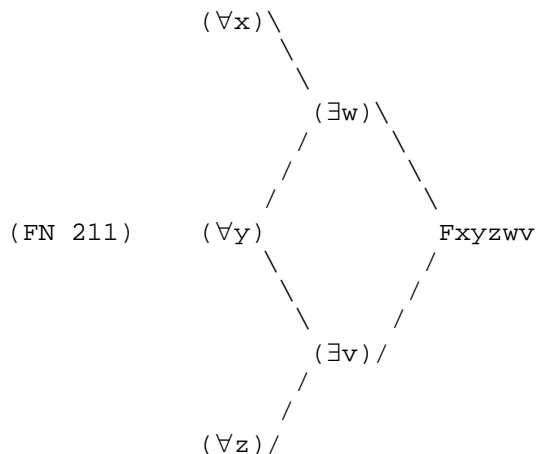
(508')  $(\exists x)(\exists y)(\forall z)(\forall w)Fxyzw$

However, others, such as [Sher 1990], give different truth conditions for the same formulae. The important fact here is that no obvious standards for settling the issue are provided by the Skolemization semantics, because that semantics relies on a type of functional type-lifting which applies only to a certain subset of the prima facie possible branching structures.<sup>378</sup>

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<sup>378</sup>Depending on what one takes the syntax of branching to be, some Skolem normal forms may also exceed the bounds of branched syntax. The worry here is that some Skolem forms correspond to branched structures in which there is 'inward branching' as well as outward branching. Thus the Skolem normal form:

(FN 210)  $(\exists f_1)(\exists f_2)(\forall x)(\forall y)(\forall z)F(x,y,z,f(x,y),f(yz))$   
 is best represented in branching format as:



(Such inward-branching syntactic structures, incidentally, will have considerably more complicated formation rules than systems with only outward branching.)  
 Any attempt to read this as a branching structure needs to have

**§3.3.2.2.2.1.3 Skolemization and Natural Language**

[Hintikka 1973] introduces the idea that part of the utility of branching quantification lies in its applicability to the analysis of natural language. His example is the following sentence:

(509) Some relative of every townsman and some relative of every villager hate each other.

He claims that since the two noun phrases 'some relative of every townsman' and 'some relative of every villager' are in coordinated positions, neither should have scope over the other.<sup>379</sup> Thus both of the following linear analyses, which do privilege one noun phrase over the other, are ruled unacceptable:

(509-1) [every x: townsman x][some y: relative y,x][every z: villager z][some w: relative w,z]hate-each-other y,w

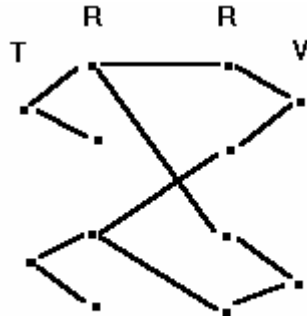
(509-2) [every x: villager x][some y: relative y,x][every z: villager z][some w: townsman w,z]hate-each-other y,w

The two readings are not equivalent<sup>380</sup>, and hence neither can be an appropriate analysis of the original (509).

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<sup>379</sup>As attested to by the prima facie equivalence between (509) and:  
(FN 212) Some relative of every villager and some relative of every townsman hate each other.

<sup>380</sup>To see this, consider the following two structures:



(FN 213)



Hintikka thus suggests that the real branching structure of (509)

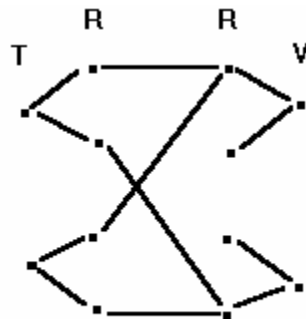
is:

$$\begin{array}{l}
 \text{(510)} \quad [\text{every } x: \text{ townsman } x] \text{ -- } [\text{some } y: \text{ relative } y, x] \backslash \\
 \hspace{20em} \hspace{10em} \backslash \\
 \hspace{20em} \hspace{10em} \text{Hate } y, w^{381} \\
 \hspace{20em} \hspace{10em} / \\
 \hspace{20em} \hspace{10em} / \\
 \text{(511)} \quad [\text{every } x: \text{ villager } x] \text{ -- } [\text{some } y: \text{ relative } y, x] /
 \end{array}$$

and that the corresponding underlying semantic analysis is:

$$\begin{array}{l}
 \text{(511)} \quad (\exists f_1)(\exists f_2)[\text{every } x: \text{ townsman } x][\text{every } y: \text{ villager } y] \\
 \quad \quad \quad (\text{relative } f_1(x), x \wedge \text{relative } f_2(y), y \wedge \text{Hate } f_1(x), f_2(y))
 \end{array}$$

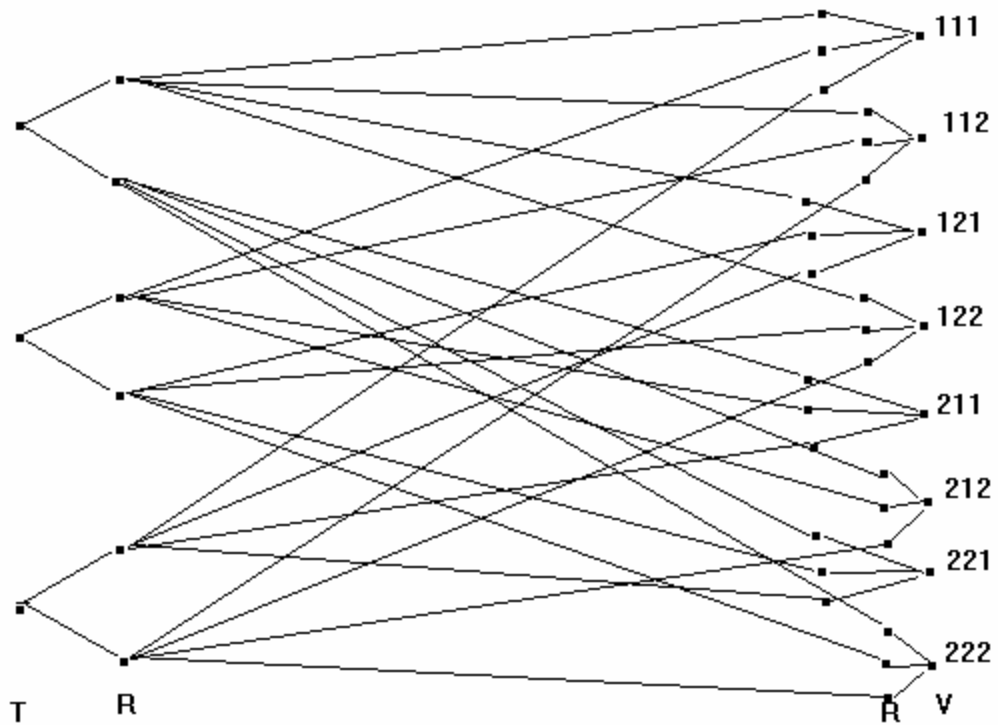
However, [Barwise 1979] argues persuasively against the intuitive acceptability of Hintikka's proposed analysis. Consider the following rather complex situation:



(FN 214)

Where the dots in the 'T' column represent townsman, the dots in the 'V' column represent villagers, the dots in the 'R' columns represent relatives (and lines between T's or V's and R's indicate relations), and lines between dots in R columns represent mutual hatred. Then (FN 213) makes (509-1) true and (509-2) false, while (FN 214) makes (509-2) true and (509-1) false.

<sup>381</sup>Where 'Hate y,w' indicates the relationship of y and w hating each other. In fact, the reciprocal 'each other' probably ought to be analyzed as introducing additional quantification. I will have little to say about the exact nature of 'each other' constructions, although my tendency is to read them as pairs of definite descriptions (analyzed quantificationally). This reading is not without difficulties. It is interesting that many putative examples of branching constructions involve reciprocals in the quantified predicate, although I am not entirely sure what to make of it.



where the dots in the T column are townsmen, the dots in the V column are villagers, the dots in the R columns are relatives, the lines between R dots and T or V dots indicate relation, and the lines between R dots indicate *lack of mutual hatred*.

Two claims are now necessary: first, that Hintikka's branched reading (511) is not true in this situation, and second that the original English sentence is true. On the first point, note that Hintikka's analysis requires:

that we can choose one [relative of] each [villager], once and for all, and one [relative of] each [townsman], again once and for all, such that none of the three [townsman-relatives] and the eight [villager-relatives] thus chosen are connected by lines. [Barwise 1979, 51]

The net effect of Hintikka's analysis is that there is some set of townsman relatives (with one representative per townsman) and some set of villager relatives (with one representative per villager) such that there is universal enmity between the two sets. But our diagram is set

up to block this possibility. If we name the two relatives of each townsman '1' and '2', and then name each villager by a triple of numbers drawn from {1,2} according to which relative of each townsman has a line pointing to each of that villager's relatives (so that the villagers are then named 111, 112, ..., 222), we will easily see that whichever set of three townsman-relatives  $\alpha$ ,  $\beta$ , and  $\gamma$  we pick, the villager named ' $\alpha\beta\gamma$ ' will not have any relative who hates all of the chosen townsman-relatives. Thus Hintikka's reading is false in Barwise's scenario.

The second claim is that (509) is true in Barwise's scenario. Intuitive, the idea here is that no matter what townsman we pick, and what villager we pick, we can find some relative of the first and some relative of the second such that the two hate each other. Barwise thus takes the sentence to have the following maximally-branched syntactic structure:

$$\begin{array}{l}
 \text{(512)} \quad [\text{every } x: \text{ townsman } x] \setminus \text{ -- } / [\text{some } y: \text{ relative } y, x] \setminus \\
 \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad | \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \setminus \\
 \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad | \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \text{Hate } y, w \\
 \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad | \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad / \\
 \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad \qquad [\text{every } x: \text{ villager } x] / \text{ -- } \setminus [\text{some } y: \text{ relative } y, x] /
 \end{array}$$

with both universal quantifiers branching inward to both existential quantifiers, and thus to be equivalent to the linear:

$$\text{(513)} \quad [\text{every } x: \text{ townsman } x][\text{every } z: \text{ villager } z][\text{some } y: \text{ relative } y, x][\text{some } w: \text{ relative } w, z]\text{hate-each-other } y, w$$

Assuming Barwise is right about the inadequacy of the Hintikka analysis -- which it seems to me he is -- the importance is not that the Skolemization semantics is left completely without an account of (509), for it of course has at its beck the linear resources necessary to

produce (513). The point is rather that, despite Hintikka's claims, the reading which the Skolemization semantics naturally gives to (509) -- in which the two universal-existential quantifier blocks are, due to their mutually coordinate position, taken to be unordered relative to one another, thus invoking the branching Skolemization procedure -- does not capture the right truth conditions. This is further evidence that the notion of branching which Skolemization appeals to is not capturing whatever dim grasp of quantifier independence we may have.

#### **§3.3.2.2.2.2 Game Theoretic Semantics and Games of Partial Information**

The Skolemization semantics suffers from a severe flouting of the Contiguity requirement, providing no underlying general account of quantification which would explain why the weakening of linearity to partial ordering should produce the specified branching semantics. The deficiency is remedied, however, in the work of [Hintikka 1973], which originally poses the possibility of branching structures in natural language and also provides a method for seeing the branching structures as a natural outgrowth of the nature of quantification. Hintikka favors a game-theoretic semantics of quantified logic, and suggests that natural extensions of the game-theoretic approach will give rise to an account of branching quantification.

In game-theoretic semantics (GTS)<sup>382</sup>, the truth or falsity of a sentence is determined by the presence or want of a winning strategy to a particular game, a semantic game designed to extend an assignment of truth for atomic sentences to an assignment for all sentences. This semantic game is played as follows: one starts with a sentence  $\phi$  and

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<sup>382</sup>As seen earlier in §3.2.1.3.2.4.3. The following paragraphs provide a recapitulation and expansion of the discussion found there.

with two players, typically called 'Myself' and 'Nature'. There are two roles within the game: that of Verifier and that of Falsifier. At the beginning of the game, I am the Verifier and Nature is the Falsifier.

Play then proceeds in accordance with the following rules:

- (G.A) If  $\phi$  is atomic, then the Verifier win if  $\phi$  is true, while the Falsifier wins if  $\phi$  is false.
- (G.&) If  $\phi$  is of the form  $(\psi_1 \wedge \psi_2)$ , then the Falsifier chooses one of  $\psi_1$  and  $\psi_2$ , and play continues on that sentence.
- (G.v) If  $\phi$  is of the form  $(\psi_1 \vee \psi_2)$ , then the Verifier choose one of  $\psi_1$  and  $\psi_2$ , and play continues on that sentence.
- (G. $\neg$ ) If  $\phi$  is of the form  $(\neg\psi)$ , then the two players switch roles, and play continues on sentence  $\psi$ .
- (G. $\forall$ ) If  $\phi$  is of the form  $(\forall x)\psi(x)$ , then the Falsifier chooses some object  $b$ , and play continues on the sentence  $\psi(b)$ .
- (G. $\exists$ ) If  $\phi$  is of the form  $(\exists x)\psi(x)$ , then the Verifier choose some object  $b$ , and play continues on the sentence  $\psi(b)$ .

The sentence  $\phi$  is then true if I have a winning strategy and false if Nature has a winning strategy.<sup>383</sup>

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<sup>383</sup>Note that since a winning strategy is defined as an algorithm, taking as input the other player's moves and outputting one's own moves, such that, no matter what moves the other player makes, one wins the game, it follows immediately that a sentence cannot be both true and false. That a sentence is either true or false is considerably less obvious -- to show this, one must show that one of I and Nature must always have a winning strategy, which is a non-trivial proof (see [Hand 1988] for a sample such proof). Hintikka claims this feature of game-theoretic semantics as an asset:

But who says that either one of us has a winning strategy? The law of the excluded middle says so. On the basic game theory we now see that this law is by no means trivial or unproblematic. For in general it is not a foregone conclusion that there should exist a winning strategy for either one of the players in a zero-sum two-person game. When one exists, the game is said to be determinate. From game theory we know that the determinateness of a game is usually a highly nontrivial result (or assumption). Indeed,

Consider a simple example. Take the sentence:

(514)  $\neg(\exists x)(\forall y)\text{danced-with } x,y$

A typical play of the semantic game will begin with me as Verifier and Nature as Falsifier. (514) is of the form ' $\neg\psi$ ', so according to rule (G. $\neg$ ), I become the Falsifier and Nature the verifier, and play continues on the sentence:

(515)  $(\exists x)(\forall y)\text{danced-with } x,y$

According to rule (G. $\exists$ ), the Verifier (now Nature) must pick some object. Let's say Nature picks Fred Astaire. Play then continues with the sentence:

(516)  $(\forall y)(\text{danced-with}(\text{Fred Astaire}, y))$

Now by rule (G. $\forall$ ), the Falsifier (that is, I) must pick an individual. Let's say I pick Ginger Rogers. Play then continues with:

(517)  $\text{danced-with}(\text{Fred Astaire}, \text{Ginger Rogers})$

Since Fred Astaire has danced with Ginger Rogers<sup>384</sup>, the Verifier -- that is, nature -- wins the game.

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determinateness assumptions for certain infinite games have recently played an important role as potential axioms in the higher reaches of axiomatic set theory. But even apart from such sophisticated situations, determinateness (and hence the law of excluded middle) can fairly easily fail. Thus the principle of excluded middle is at once put into an interesting general perspective by GTS. [Hintikka 1982, 221].

I admit to being somewhat dogmatic on this issue, but I don't want any 'interesting general perspective' for the law of the excluded middle. That law is in fact 'trivial and unproblematic', and so much the worse for any semantic approach which claims that it is not.

Hintikka is being modest when he says 'it is not a foregone conclusion that there should be a winning strategy for either one of the two players in a zero-sum two-player game'. In fact, almost all such games lack a winning strategy -- and thus are useless for capturing the logical underpinnings of semantics. In the absence of a generic philosophical explanation of what differentiates the semantically interesting games from the vast fields of semantic junk, I find it difficult to take GTS seriously as a genuinely explanatory semantic account.

<sup>384</sup>See, e.g., *Flying Down to Rio* or *The Gay Divorcee*.

Now this alone doesn't show anything about the truth value of (514). Had I picked Gottlob Frege instead of Ginger Rogers, I/the Falsifier would have won the game. What matters here is whether I have a *winning strategy*. In this case, that amounts to a way of making my choice (when  $(G.\forall)$  is applied) so that, no matter what Nature does, I win the game. Such a strategy exists, obviously, iff for any person, there is some second person with whom that first person has not danced. If there is such a strategy (as there is), then -- since I have a winning strategy -- the sentence is true. Thus GTS delivers the correct result here.

#### **§3.3.2.2.2.1 Branching and Partial Information**

In order to allow for branching quantifiers, we modify the *information conditions* of the game. In standard GTS, it is assumed that each player knows what moves have been made previously by the other player. Thus when, in the example used above, Nature picks Fred Astaire, I know that this choice has been made, and can then choose my individual (under rule  $(G.\forall)$ ) in accordance with this knowledge, seeking an individual with whom Fred Astaire has not danced (such as Frege). This knowledge is evinced in the structure of a winning strategy for me: such a strategy can be encoded as a list of choices made by Nature under rule  $(G.\exists)$  and winning responses to those choices on my part.

However, we could assume that the one player doesn't get to know what choice has been made by the other player when proceeding. To take the simplest case of this, assume that we've got a formula  $\phi$  in which there is a block of quantifiers  $(Q_1 x_1), \dots, (Q_n x_n)$  which are somehow

(syntactically) marked as being unordered. We then define a PK-Strategy (for 'partial knowledge') as follows:

**PK-Strategy:**  $S$  is a PK-strategy for  $\phi$  for player  $P$  if, given any initial string of moves  $\langle m_1, \dots, m_n \rangle$  in a game for  $\phi$ ,  $S$  specifies a subsequent move  $S(\langle m_1, \dots, m_n \rangle)$  for  $P$ , and if, whenever move  $m_i$  is made in response to a rule  $(G.Q_i)$  applied to a quantifier marked as partially ordered,  $S(\langle m_1, \dots, m_i, \dots, m_n \rangle) = S(\langle m_1, \dots, x, \dots, m_n \rangle)$  for any other permissible move  $x$ .<sup>385</sup>

If, for example, we are playing through a sentence of the form:

$$(518) \quad (\forall x) \mid \text{danced-with}(x, y) \\ (\exists y) \mid$$

and Nature has chosen an individual in response to the universal quantifier, my strategy, which now tells me which individual to pick in response to the existential quantifier, cannot be sensitive to Nature's choice at the universal quantifier -- simply because I am taken not to know what Nature has chosen.<sup>386</sup> If my strategy tells me to pick Gottlob Frege, I pick Gottlob, regardless of whether Nature has chosen Ginger Rogers or Bertrand Russell. A little thought will then show that I have a winning PK-strategy for (518) just in case there is one person who has danced with every person.

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<sup>385</sup>See [Hand 1991] for a more general discussion of the use of partial knowledge states to induce partially ordered quantificational structures.

<sup>386</sup>Similarly, Nature's strategy cannot be sensitive to my choice at the existential quantifier, although this is automatically imposed by the assumption that the universal quantifier is processed first. Intuitively, one can think of the PK-strategies as imposing the requirement that multiple moves by different players be carried out simultaneously, although this intuitive picture doesn't capture the *full* force of PK-strategies.



The 'partial knowledge' GTS implementations of partially-ordered quantifier prefixes yield (and are intended to yield) the same results as the Henkin approach based on the generalization of Skolem functions. Given a formula  $\psi(x_1, \dots, x_n)$  preceded by a classical quantifier prefix  $(Q_1 x_1) \dots (Q_n x_n)$  on which some partial order  $<_p$  has been imposed, that is, the PK-strategy truth conditions yield something equivalent to the second-order:

$$(519) (\exists f_1)(\exists f_2)\dots(\exists f_m)(\forall x_{j_1})(\forall x_{j_2})\dots(\forall x_{j_{n-m}}) \\ \psi(\dots, x_{j_k}, \dots, f_1(x_{g_1^{i_1}}, \dots, x_{g_{n_1}^{i_1}}), \dots)$$

where  $Q_{i_1}, \dots, Q_{i_m}$  are the existential quantifiers, and  $g_1^{i_1}, \dots, g_{n_1}^{i_1}$  are such that  $Q_x >_p Q_{i_1}$  iff  $x$  is one of  $g_1^{i_1}, \dots, g_{n_1}^{i_1}$ . Each existential (first-order) quantifier is thus replaced with an existential quantifier ranging over functions taking as argument all universal quantifiers having scope (according to the partial order  $>_p$ ) over the original ' $\exists$ '.

While the GTS account, with games of partial knowledge, provides the same truth conditions as the Skolemization semantics, it provides, I think, an undeniable conceptual advance. We now have genuine Contiguity -- the game-theoretic conditions yield a natural account of quantification which bridges the gap between linearly and partially ordered quantifier prefixes, through variations in knowledge conditions. Furthermore, the GTS approach is reasonably respectful of the Simplicity requirement, since the *increase* in knowledge required to yield linear quantification is plausibly taken as an increase in logical structure.

### §3.3.2.2.2.2 Three Problems With Games of Partial Knowledge

Despite the conceptual advances of the GTS approach to branching, serious worries remain. I want to focus on three such worries here, of

increasing severity. The first worry should be immediately obvious. Since games of partial information in classical systems are truth-conditionally equivalent to Skolemization interpretations of branching structures, Hintikka's GTS analysis of a sentence like (509) above:

(509) Some relative of every townsman and some relative of every villager hate each other.

will, like the Skolemization semantics, produce a reading which is subject to the Barwise counterexample discussed in §3.3.2.2.2.1.3 above. Partial games, whatever other virtues they may have, seem a poor tool for the analysis of putative natural language branching.

The second worry for games of partial knowledge is related to the worries on the appropriate syntactic domain of branching theories discussed in §3.3.2.1 above. GTS achieves a natural extension to branching quantification by highlighting knowledge of game history as a crucial element of the semantic analysis and then considering variations in the requisite knowledge conditions. This tactic, however, can be carried considerably further than it is by Hintikka in analyzing branching structures. Hintikka alters the knowledge conditions of the game by imposing the condition:

**(Branch)** If quantifiers  $Q_1$  and  $Q_2$  are mutually unordered, then the player acting on the rule  $(G.Q_1)$  is unaware of the move made by the player acting on  $(G.Q_2)$ , and vice versa.

But consider the following knowledge conditions which could be incorporated into GTS semantics:

**(K1)** If quantifiers  $Q_1$ ,  $Q_2$ , and  $Q_3$  are all unordered relative to one another, then the player acting on  $(G.Q_1)$  is unaware of the move made by the player acting on  $(G.Q_2)$ , the player

acting on  $(G.Q_2)$  is unaware of the move made by the player acting on  $(G.Q_3)$ , and the player acting on  $(G.Q_3)$  is unaware of the move made by the player acting on  $(G.Q_1)$

- (K2) If quantifiers  $Q_1$ ,  $Q_2$ , and  $Q_3$  are all unordered relative to one another, then for any  $Q_i$ ,  $i = 1,2,3$ , the player acting on  $(G.Q_i)$  knows what moves were made by the players acting on the rules for the other two quantifiers, but not which move was made by which player.
- (K3) If a move is made acting on rule  $(G.Q_1)$ , then no player ever knows what that move was.
- (K4) No player ever knows what moves he has made in the past, but always knows what moves his opponent made in the past.
- (K5) If quantifiers  $Q_1$  and  $Q_2$  are mutually unordered, then no player acting on any rule after either the rule  $(G.Q_1)$  or the rule  $(G.Q_2)$  has been applied knows what move was made in the applications of those
- (K6) If quantifiers  $Q_1$  and  $Q_2$  are mutually unordered, then a player moving after the application either of the rule  $(G.Q_1)$  or the rule  $(G.Q_2)$  knows what move was made under that rule only if an even number of moves have been made since that rule was invoked.
- (K7) All players know all past and future moves of all other players.

These knowledge constraints correspond (at least roughly) to a wide variety of ways of reading branched structures. (K7), for example, is plausibly read as imposing a circular ordering onto quantifiers, in which a series of quantifiers all have scope over each other. (K3)

effectively makes certain quantifiers completely unordered with respect to any other quantifiers in the sentence. (K2) creates odd dual-branched structures in which the branches are identical, except that quantifier orders have been reversed in them. With some creativity, knowledge conditions can be crafted which back just about any reading of any quantified sentence.

The problem, of course, is that it's not at all clear that we *want* quantified sentences to be readable in all these ways. GTS, once we see that the implicit 'full knowledge' condition in the initial formulation can be weakened or altered, severely overgenerates, and leaves us with no plausible standard for distinguishing 'good' readings -- games corresponding to some real notion of quantification -- with bizarre readings.

The third and most serious problem with the GTS approach of quantification is familiar from the earlier discussion of Skolemization -- it fails to support generalized quantifiers. In the case of the Skolemization semantics, this failure is evidence of a violation of Contiguity, since it shows that the notion of a quantifier, once generalized appropriately, does not in fact carry over in a natural way to the notion of a branched quantifier. The same criticism, however, cannot be leveled at GTS. As we saw earlier (§3.2.1.3.2.4.3), it is already in the nature of GTS not to allow any but tokenable quantifiers. Thus GTS does respect Contiguity in its analysis of branching, but in a perverse way. It avoids the Skolemizing flouting of Contiguity only by ruling out of bounds from the beginning the potential trouble cases of generalized quantifiers, and thus restricting its domain to the classical quantifiers where Skolemization is reasonably successful. But,

as was argued earlier (§3.2.1.3.2), an account which rejects generalized quantifiers (especially on the grounds of non-tokenability) is unacceptable. Thus the GTS use of games of partial knowledge cannot be the true key to branching semantics.

### **§3.3.2.2.2.3 Barwise and Neo-Fregean Analyses of Branching**

#### **Quantification**

[Barwise 1979], mating the two exotica of branching and generalized quantifiers, gives rise to a semantics which allows us to interpret such stylistic infelicities as:

(520) Most relatives of each villager and most relatives of each townsman hate each other.

(521) More than half of the dots and more than half of the stars are all connected by lines.

(522) Few philosophers and few linguists agree with each other about branching quantification.

(523) The richer the country, the more powerful one of its officials.

Barwise makes use of the neo-Fregean account of quantification (see §1.3) to situate generalized quantifiers in branching structures. Rather surprisingly, however, Barwise argues that there are two semantic modes of interpretation for branched structures, and that the appropriate mode of evaluation is determined by the monotonicity properties of the quantifiers involved. Barwise thus gives one truth definition for branching structures involving only mon  $\uparrow$  quantifiers and another for structures involving only mon  $\downarrow$  quantifiers:

**(Def. 24)** A branching formula of the form:

$$\frac{(Q_1 x) \backslash \varphi(x, y)}{(Q_2 y) /}$$

is true if either:

(A)  $Q_1$  and  $Q_2$  are monotone increasing, and:

$$(\exists X)(\exists Y)[(Q_1 x)(x \in X) \ \& \ (Q_2 y)(y \in Y) \ \& \ (\forall x)(\forall y)((x \in X \ \& \ y \in Y) \rightarrow \varphi(x, y))]$$

or:

(B)  $Q_1$  and  $Q_2$  are monotone decreasing, and:

$$(\exists X)(\exists Y)[(Q_1 x)(x \in X) \ \& \ (Q_2 y)(y \in Y) \ \& \ (\forall x)(\forall y)(\varphi(x, y) \rightarrow (x \in X \ \& \ y \in Y))]^{387} \ [63-64]$$

The distinction between clauses (A) and (B) is necessary because when the quantifiers are mon  $\downarrow$ , the mere fact that there are sets of the requisite size satisfying the quantified relation is not enough to ensure the truth of the branched sentence, for there might be yet more objects satisfying that relation. For example, in:

(524) Few philosophers and few linguists agree with each other about branching quantification.

it does not suffice to guarantee the truth of (524) that there are sets of few philosophers and few linguists whose members all agree about branching quantification -- for, with no constraints on what happens

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<sup>387</sup>This definition can be generalized to cover more complex partially ordered quantifier prefixes, but the simple form given here suffices for our current purposes.

outside these distinguished sets, we could have *all* philosophers and *all* linguists in agreement.<sup>388</sup>

In addition to providing different semantics for  $\text{mon } \uparrow$  and  $\text{mon } \downarrow$  structures, Barwise denies that branching structures with quantifiers of mixed monotonicity are sensible. Thus he claims that:

(525) Few of the boys in my class and most of the girls in your class have all dated each other.

while it is grammatical, is semantically uninterpretable.<sup>389</sup> He does not address at all structures with non-monotonic quantifiers, but presumably would claim that these structures, such as:

(526) Exactly five of the dots and exactly four of the stars are all connected by lines.

are also uninterpretable.

Barwise's distinctions among branched structures based on the monotonicity of their quantifiers is worrying, because the neo-Fregean account of quantification gives no reason to suppose that quantifiers should act differently in partially ordered configurations based on their monotonicity properties. My feeling is that this worry is a more general manifestation of the worry that the neo-Fregean account, by

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<sup>388</sup>Barwise's clause (A), for example, would make any statement of the form:

$$\begin{array}{r}
 (\text{NO } x) \backslash \\
 (\text{FN } 215) \quad \quad \quad \varphi(x, y) \\
 (\text{NO } y) /
 \end{array}$$

trivially true.

<sup>389</sup>Note that in the case in which there are no boys in my class, the quantifier 'few boys in my class', as opposed to the determiner 'few', is monotone increasing. Thus clause (A) ought to make (525) interpretable in such situations. That (525) is, if anything, even worse when there are no boys in my class indicates to me that Barwise really wants to apply the semantic rules on the basis of the monotonicity of the determiner, rather than on that of the quantifier.

lifting all first order quantification, linear or branching, into second-order structure, has a natural tendency to overgeneralize and thus has difficulty explaining the borders of what seem to be the natural domains of certain types of quantification. One philosophical manifestation of this blurring effect of the neo-Fregean account is Barwise's peculiar views on the ontology of branching quantification.

One of Hintikka's aims, in the paper Hintikka (1974), was to show that there are simple sentences of English which contain essential uses of branching quantification. If he is correct, it is a discovery with significant implications for linguistics, for the philosophy of language, and perhaps even for mathematical logic. Philosophically, it would influence our views of the ontological commitment inherent in specific natural language constructions, since branching quantification is a way of hiding quantification over various kinds of abstract abstract [sic] objects (functions from individuals to individuals, sets of individuals, etc.).  
(47)

Contra Barwise, there is a clear sense in which branching quantification ought to be ontologically committed in exactly the same way as first-order quantification -- namely, to the ordinary individuals which are quantified over (albeit in a branched way). Once all quantification is given a second-order, set-theoretic explanation, however, such fine details of ontological commitment tend to be lost.

Before abandoning the Barwise approach altogether, however, I want to consider [Sher 1990]'s attempt to reconfigure the Barwise account to remove the monotonicity sensitivity from branching semantics. In the process, we will make some discoveries about the ineliminability of such sensitivity.

#### **§3.3.2.2.2.3.1 Sher, Maximality, and Monotonicity**

[Sher 1990], dissatisfied with the ad hoc patchwork of the Barwise branching semantics, proposes a semantic rule which is independent of



the monotonicity properties of the involved quantifiers. She thus suggests adding to Barwise's clause (A) the requirement that the chosen sets be *maximal* sets satisfying the quantified relation. We obtain the following semantic rule:

**(Def. 25)** A branching formula of the form:

$$\begin{array}{c} (Q_1 x) \backslash \\ \phantom{(Q_1 x) \backslash} \\ \phantom{(Q_1 x) \backslash} \backslash \\ \phantom{(Q_1 x) \backslash} \phantom{(Q_1 x) \backslash} \varphi(x, y) \\ \phantom{(Q_1 x) \backslash} / \\ (Q_2 y) / \end{array}$$

is true if there is at least one pair  $\langle X, Y \rangle$  of subsets of the domain for which (a)-(d) below hold

- (a) X satisfies the quantifier-condition Q1.
- (b) Y satisfies the quantifier-condition Q2.
- (c) Each element of X stands in the relation  $\varphi$  to all the elements of Y.
- (d) The pair  $\langle X, Y \rangle$  is a maximal pair satisfying (3). [412]

Clause (d) is the novelty here, and the claim is that by its inclusion we obtain a definition that will work for any combination of quantifiers, regardless of their monotonicity. Thus consider again:

(524) Few philosophers and few linguists agree with each other about branching quantification.

in the situation in which all philosophers and all linguists agree with each other.<sup>390</sup> Here we could pick sets X and Y, such that X had few philosophers, Y had few linguists, and each member of X agreed with all

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<sup>390</sup>Assume also that there are more than a few philosophers and linguists.

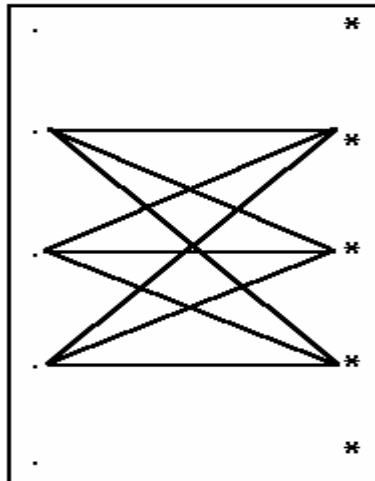


we will again, under Barwise's analysis, place overly stringent requirements on the hatred present.

On Barwise's analysis, the truth of (528) requires that there must be some set  $X$  such that for each villager  $v$ , most of the relatives of  $v$  are in  $X$ , and some set  $Y$  such that for each townsman  $t$ , most of the relatives of  $t$  are in  $Y$ , such that every member of  $X$  hates every member of  $Y$ , and vice versa. There must be, as Sher terms it, a massive nucleus of haters. Sher, however, suggests that these truth conditions are too strong. To see this most clearly, consider a simpler example:

(529) Most of the dots and most of the stars are connected by lines.

Barwise requires, for the truth of (529) that there be some set  $X$  of most of the stars and some set  $Y$  of most of the dots, such that every member of  $X$  is connected to every member of  $Y$  by lines. Barwise thus requires a configuration of the form:



Sher suggests, however, that there is a weaker reading of (529) available, in which it suffices merely to have each member of  $X$



(532) Most of my friends saw at least two of the same few Truffuat movies.

Sher wants to be able to replace the condition that each member of set X bear the relevant relation to all members of set Y with any number of weaker conditions, such as:

- (a) Each member of X bears the relation to exactly one member of the set Y.
- (b) Each member of X bears the relation to at least two members of the set Y.
- (c) Each member of X bears the relation to at least half of the members of Y, and each member of Y is borne the relation by at least half the members of X.

Note that each of these conditions is logically implied by the original 'each-to-all' condition<sup>392</sup>, so what Sher offers us here is a variety of ways of weakening the default semantics for branching structures in order to capture new interpretations of branched sentences. Thus Sher offers the following final modification of the Barwise semantics:

**(Def. 26)** A branching formula of the form:

$$\begin{array}{c} (\mathcal{Q}_1 x) \backslash \\ \quad \quad \quad \backslash \\ \quad \quad \quad \quad \varphi(x,y) \\ \quad \quad \quad \quad / \\ (\mathcal{Q}_2 y) / \end{array}$$

is true if there is at least one pair  $\langle X, Y \rangle$  of subsets of the domain for which (a)-(d) below hold

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<sup>392</sup>Provided, in the case of condition (b), that the set Y in the maximal pair has at least two elements.

- (a) X satisfies the quantifier-condition Q1.
- (b) Y satisfies the quantifier-condition Q2.
- (c) The pair  $\langle X, Y \rangle$  is a  $\Sigma$ - $\varphi$  pair (for some condition  $\Sigma$ ).
- (d) The pair  $\langle X, Y \rangle$  is a maximal pair satisfying (c). [412]

where, for example,  $\langle X, Y \rangle$  are a 'each-all'- $\varphi$  pair if each member of X bears  $\varphi$  to all members of Y, and a 'each-some'- $\varphi$  pair if each member of X bears  $\varphi$  to some member of Y. Sher thus gives us a family of readings of any given branched sentence.

### **§3.3.2.2.2.3.1.2 Two Versions of Maximality**

Sher's generalization of the Barwise position looks initially promising, but I want to illustrate that the introduction of maximality conditions raises more problems than it solves. To see why this is, let's consider a slight modification of (524), designed to duck complications introduced by the transitivity of 'agrees':

(533) Few philosophers and few linguists hate each other.

I will focus, until we reach §3.3.2.2.2.3.1.4 below, exclusively on the 'each-all' readings. Assume now that there is one philosopher -- call him S -- who hates and is hated by all linguists, and that there are three other philosophers, P1, P2, and P3, and three linguists, L1, L2, and L3, all of whom hate each other. All other philosophers and all other linguists like each other. The reader is first asked to consider whether (533) is true under this situation.

I admit considerable difficulty in determining the truth value of (533) in the situation described above, so let's consult Sher's semantics. We need then to find some two sets X and Y of philosophers and linguists such that the pair is a maximal pair all of whose members

satisfy the 'hate each other' relation. The problem here is that there are two plausible candidates for maximal pairs:

(M1)  $\langle \{S\}, \{x \mid x \text{ is a linguist} \} \rangle$

(M2)  $\langle \{P1, P2, P3\}, \{L1, L2, L3\} \rangle$

Neither (M1) nor (M2) contains the other, and we can't just combined them to get:

(M3)  $\langle \{S, \{P1, P2, P3\}\}, \{x \mid x \text{ is a linguist} \} \rangle$

because we will not then have universal hatred between X and Y: P1, for example, will not hate those linguists other than L1, L2, and L3 who will now be in the set Y. So which of (M1) and (M2) is maximal?

In order to clarify the issue, we need to observe that there are two senses in which a pair  $\langle X, Y \rangle$  can be maximal satisfying the relation  $\varphi(x, y)$ :

**(Weak Maximality)** A pair  $\langle X, Y \rangle$  is weakly maximal for the relation  $\varphi$  if there are no sets  $X'$  and  $Y'$  such that  $X' \times Y'$  satisfies  $\varphi$  and  $X \times Y \subset X' \times Y'$ .

**(Strong Maximality)** A pair  $\langle X, Y \rangle$  is strongly maximal for the relation  $\varphi$  if, given any sets  $X', Y'$  such that  $X' \times Y'$  satisfies  $\varphi$ ,  $X' \times Y' \subseteq X \times Y$ .

The logic of strong maximality requires that there be at most one strongly maximal pair for a given relation, but there may well be no strongly maximal pair. Weakly maximal pairs, on the other hand, are guaranteed to exist, but may not be unique for a given relation. Both (M1) and (M2) are weakly maximal, since neither is contained in any larger pair which satisfies the 'hate each other' relation. Neither,

however, is strongly maximal, since neither contains all pairs satisfying that relation.<sup>393</sup>

So which does Sher want, a weakly or a strongly maximal pair? She does not herself draw the distinction or discuss the precise ramifications of maximality, but there are two conclusive bits of internal evidence indicating that she wants weak maximality. First, she makes the following claim about the well-formedness of her semantic rule:

(6.C) [Def. 2 above] is formally correct. (I.e., given a model  $\mathbf{A}$  with a universe  $A$ , a binary relation  $\Phi^{\mathbf{A}}$  and two subsets,  $B$  and  $C$ , of  $A$  s.t.  $B \times C \subseteq \Phi^{\mathbf{A}}$ , there are subsets  $B'$  and  $C'$  of  $A$  s.t.  $B \subseteq B'$ ,  $C \subseteq C'$ , and  $B' \times C'$  is a maximal Cartesian product included in  $\Phi^{\mathbf{A}}$ .) [412]

But this claim, with its guarantee of the existence of a maximal pair, can only be true if the maximality in question is weak, since there may not be a strong maximum. If we demand strong maximality here, our semantic rule will often gratuitously output either nonsense or falsity. Second, when Sher turns to the application of her rule to an example, she makes the following claim:

$$\begin{array}{l} (Q1 \ x) \backslash \\ \quad \backslash \\ \quad \quad \varphi(x,y) =df \\ \quad \quad / \\ (Q2 \ y) / \end{array}$$

$$\begin{array}{l} (\exists X)(\exists Y)\{ (Q1x)Xx \ \& \ (Q2y)Yy \ \& \ (\forall x)(\forall y)[Xx \ \& \ Yy \ \rightarrow \ \varphi(x,y)] \ \& \\ (\forall X')(\forall Y') [ (\forall x)(\forall y)(Xx \ \& \ Yy \ \rightarrow \ X'x \ \& \ Y'y) \ \& \ (\forall x)(\forall y)(X'x \ \& \ Y'y \ \rightarrow \\ \varphi(x,y)) \ \rightarrow \ (\forall x)(\forall y)(Xx \ \& \ Yy \ \leftrightarrow \ X'x \ \& \ Y'y)] \} \end{array} \quad [412]$$


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<sup>393</sup>And, in fact, there is no strongly maximal pair on the 'hate each other' relation in the described situation.



But this second-order formula exploits the weak maximality condition -- a requirement for strong maximality would omit the conjunct ' $(\forall x)(\forall y)(Xx \ \& \ Yy \ \rightarrow \ X'x \ \& \ Y'y)$ '.

### **§3.3.2.2.2.3.1.3 A Problem with Weak Maximality**

Sher wants weak maximality, but unfortunately what she wants and what she needs are not the same. The weak maximality condition leaves us with too much flexibility. There can be many weakly maximal pairs, but Sher just requires that one of them meet the cardinality condition imposed by the quantifiers. Thus consider (533) again, but now assume the following:

- (a) P1 and L1 hate each other.
- (b) P1 likes all other linguists; L1 likes all other philosophers.
- (c) All philosophers other than P1 hate all linguists other than L1.
- (d) All linguists other than L1 hate all philosophers other than P1.

Clearly in such a situation (533) is false, since we could well have thousands of philosophers and linguists in a state of universal mutual hatred. But note that in this situation there are *two* pairs which are weakly maximal on the relation 'hate each other':

(M4)  $\langle \{P1\}, \{L1\} \rangle$

(M5)  $\langle \{x \mid x \text{ is a philosopher other than } P1\}, \{x \mid x \text{ is a linguist other than } L1\} \rangle$

Neither (M4) nor (M5) can be extended. Since (M4) meets the fewness cardinality condition on both X and Y, there is some maximal pair

meeting that condition, so under Sher's semantics (533) comes out true. But that is simply the wrong result.

The best available fix is to require that *all* the weakly maximal pairs meet the cardinality conditions, rather than just one:

**(Def. 27)** A branching formula of the form:

$$\begin{array}{c} (Q_1 x) \backslash \\ \quad \quad \quad \backslash \\ \quad \quad \quad \quad \varphi(x,y) \\ \quad \quad \quad \quad / \\ (Q_2 y) / \end{array}$$

is true if for every pair  $\langle X, Y \rangle$  of subsets of the domain such that:

- (a) Each element of  $X$  stands in the relation  $\varphi$  to all the elements of  $Y$ .
- (b) The pair  $\langle X, Y \rangle$  is a maximal pair satisfying (a).

we have:

- (c)  $X$  satisfies the quantifier-condition  $Q_1$ .
- (d)  $Y$  satisfies the quantifier-condition  $Q_2$ .

But this modified condition is both too weak and too strong. To see that it is too strong, consider:

(534) (At least) two philosophers hate exactly one linguist.

Assume now that  $P_1$  and  $P_2$  both hate  $L_1$ , that  $P_3$  hates  $L_2$  and  $L_3$ , and that all other philosopher-linguist pairs are amicable. (534) looks true to me in this situation, as best I can understand it as a branching construction. But we have here two weakly maximal pairs on the 'hate each other' relation:

(M6)  $\langle \{P_1, P_2\}, \{L_1\} \rangle$

(M7)  $\langle \{P_3\}, \{L_2, L_3\} \rangle$

(M6) meets the cardinality constraints 'at least two' on X and 'exactly one' on Y, but (M7) meets neither, so by Def. 3, (534) is false.

The undergeneration of Def. 27 depends, in my opinion, on tenuous semantic intuitions regarding the truth conditions for branched sentences with quantifiers of mixed cardinality. The case for the weakness of that definition, however, is more straightforward. Return now to (16), and consider the following situation. Take all of the philosophers to be named by  $P_1, P_2, \dots, P_n$ , and all of the linguists to be named by  $L_1, L_2, \dots, L_n$ .<sup>394</sup> Now assume that  $P_i$  and  $L_i$  hate each other, for each  $i$ , and that  $P_i$  and  $L_j$  like each other when  $i \neq j$ . We then have  $n$  different weakly maximal pairs of haters, each of the form:

(M8)  $\langle \{P_i\}, \{L_i\} \rangle$

Each of these maximal pairs, moreover, meets the cardinality constraints imposed by the 'few' quantifiers. But surely (533) is not true in this situation. We have thousands of philosophers hating thousands of linguists, and vice versa. That is not few philosophers and few linguists hating each other.

#### **§3.3.2.2.2.3.1.4 A Weakened Form of Weak Maximality**

One final stab at a fix for Sher. We could weaken the density of hatred that needs to exist between the members of X and the members of Y. In the situation described above, even though there was, in some sense, enmity between all of the philosophers and all of the linguists, we didn't have each philosopher hating all linguists, but just one

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<sup>394</sup>I assume for the sake of convenience that there are exactly as many philosophers as there are linguists. This assumption is dispensable.

particular linguist. We thus offer the following modification of Def.

27:

**(Def. 28)** A branching formula of the form:

$$\begin{array}{c} (Q_1 x) \backslash \\ \quad \quad \quad \backslash \\ \quad \quad \quad \quad \varphi(x,y) \\ \quad \quad \quad \quad / \\ (Q_2 y) / \end{array}$$

is true if for every pair  $\langle X, Y \rangle$  of subsets of the domain such that:

- (a) Each element of  $X$  stands in the relation  $\varphi$  to *some* element of  $Y$ .<sup>395</sup>
- (b) The pair  $\langle X, Y \rangle$  is a maximal pair satisfying (a).

we have:

- (c)  $X$  satisfies the quantifier-condition  $Q_1$ .
- (d)  $Y$  satisfies the quantifier-condition  $Q_2$ .

The crucial change here is in clause (a): we require only that each member of  $X$  have *some* member of  $Y$ . Given this weakening, we now have a new weakly<sup>396</sup> maximal set:

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<sup>395</sup>We also require the corresponding condition that each element of  $Y$  stand in the relation  $\varphi$  to some element of  $X$ . Technically, then, this is an 'each-some/some-each' condition, though for brevity I will refer to it simply as an 'each-some' condition.

Note that Sher's system itself gives us no particular reason to require the additional '/some-each' half of this condition. However, if we think of the  $\varphi$ -like relation between  $X$  and  $Y$  as corresponding to a polyadic plurally referring sentence of the form:

(FN 218)  $X \varphi$ 's  $Y$

where  $X$  and  $Y$  are plurally referring expressions referring to the elements of  $X$  and  $Y$  respectively, then our earlier observation (§3.2.1.1.2.2) that 'each-some/some-each' conditions are minimal requirements on fundamentally singular readings of PPR sentences imposed by the Involvement Principle, then the necessity for this further condition is made clear.

(M9)  $\langle \{x \mid x \text{ is a philosopher}\}, \{y \mid y \text{ is a linguist}\} \rangle$

And this minimal set does not meet the 'fewness' cardinality condition, yielding the correct prediction of falsity for (533).

Def. 28 does nothing about the undergeneration illustrated above, but let's continue to set that problem aside on the grounds that the semantic intuitions behind it are too weak to rely on. Def. 28 also dictates a clear answer to our early question about the truth value of (533) when philosopher S hates all linguists and a few other philosophers hate a few other linguists -- according to Def. 28, (533) is false in this situation. Again, my intuitions on what happens here with (533) are too hazy to use as a tool for evaluating def. 28.<sup>397</sup> What is not hazy, however, is that Def. 28 deprives Sher of one of the major innovations of her semantics -- once we weaken the initial semantics for the branching structures to require just that each member of X bear the relation to some member of Y, we cannot further weaken it to get readings of the form:

- (a) Each member of X bears the relation to exactly one member of the set Y.
- (b) Each member of X bears the relation to at least two members of the set Y.
- (c) Each member of X bears the relation to at least half of the members of Y, and each member of Y is borne the relation by at least half the members of X.

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<sup>396</sup>In fact, once we replace the 'each-all' condition with the 'each-some' condition, the distinction between weak and strong maximality collapses.

<sup>397</sup>I am tempted to count it against Def. 28 that it yields a clear answer of any sort for (533) under these circumstances, since it fails to respect the conflict of my semantic intuitions.

Each of these readings is stronger than that of the default semantics -- the default semantics is in fact as weak as it can plausibly be made.<sup>398</sup> Instead of allowing us a whole family of interpretations for different situations, Sher is forced, by Def. 28, to leapfrog straight to the weakest reading, abandoning the rest of the family.<sup>399</sup>

### §3.3.2.2.3.1.5 Independently Branching Quantification

In fact, by moving to Def. 28, Sher would be giving up her entire project of 'complexly dependent' branching quantification. As she notes (p.417), weakening the maximality condition to the 'each-some' level results in what she calls 'independently branching' structures. Sher distinguishes between independently and complexly dependently branching structures:

It is characteristic of a linear quantifier-prefix that each quantifier (but the outermost) is directly dependent on exactly one other quantifier. We shall therefore call linear quantification uni- or simply-dependent. There are two natural alternatives to simple dependence: (i) no dependence, i.e., independence, and (ii) complex dependence. These correspond to two ways in which we can view relations in a non-linear way: we can view each domain separately,

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<sup>398</sup>Although note again that Sher gives us no explanation as to why the 'each-some' condition is the theoretically minimal correlating condition. In fact, Sher gives us no standards at all for determining what correlations between X and Y generate permissible classes of readings. See §3.3.2.2.3.2.1 below for discussion of how this lacuna is remedied under my analysis.

<sup>399</sup>Sher could suggest that the readings such as (a) - (c), as well as the original 'each-all' reading, represent optional strengthenings, rather than weakenings, of the default condition. But the problem here is that these strengthenings simply don't yield the right truth conditions. To get the right truth conditions for (533), one *must* use a condition as weak as the 'each-some' condition. Of course, some sentences, such as:

(FN 219) Most philosophers and most linguists hate each other. will be true when there are maximal sets of the right size meeting the stronger 'each-all' condition, but that's just because (a) any pair maximal by the 'each-all' condition is maximal by the 'each-some' condition, and (b) the 'most' cardinality constraint used in (FN 219) will continue to be met even if more philosophers and linguists are thrown in when the condition is weakened to 'each-some'.

complete and unrelativised; or we can view a whole cluster of domains at once, in their mutual relationships. [402]

Sher's semantics for complex dependence, working off of the Barwise approach, we have seen above. For independent branching, she arrives at the following semantic rule:

**(Def. 29)** An independently branching structure of the form:

$$\begin{array}{l} [D1 \ x: \ \psi1(x)] \ | \\ \qquad \qquad \qquad | \ \varphi(x,y)^{400} \\ [D2 \ y: \ \psi2(y)] \ | \end{array}$$

is true iff:

$$\begin{array}{l} [D1 \ x: \ \psi1(x)] [\exists \ y: \ \psi2(y)] \varphi(x,y) \ \& \\ [D2 \ y: \ \psi2(y)] [\exists \ x: \ \psi1(x)] \varphi(x,y)^{401,402} \end{array}$$

Independently branching structures, then, unlike complexly dependent branching structures (under Sher's original semantics) are strictly first-order.

But this semantic rule for independent branching yields some strange results. Consider one of Sher's examples:

(536) Three frightened elephants were chased by twelve hunters.

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<sup>400</sup>In a formal language, of course, we can thus use different syntactic structures to distinguish between complexly and independently branching structures; how we make the distinction in natural language is a difficult question, the answer to which Sher gestures toward.

<sup>401</sup>I restrict myself here to the two-branch case; the necessary generalization is obvious. Also, where Sher makes use of relational quantifiers, I have employed restricted quantifiers to increase uniformity with discussion elsewhere in this paper. The consequent difference between my formulation and hers is purely notational.

<sup>402</sup>Note that this semantics for branching fails dramatically (as does Sher's second-order semantics for complex dependence) when certain types of cumulative action enter the picture. Thus to interpret:

(FN 220) Three men pushed two cars up the hill.

as:

$$\begin{array}{l} (\text{FN 220}') \ [3x: \ \text{man}(x)] [\exists y: \ \text{car}(y)] \text{pushed-up-a-hill}(x,y) \ \& \\ \qquad \qquad \qquad [2y: \ \text{car}(y)] [\exists x: \ \text{man}(x)] \text{pushed-up-a-hill}(x,y) \end{array}$$

is to get things wrong, since no one man pushed any car up the hill. See §3.3.2.2.3.2.1 below for further discussion of the important relations between branching and cumulative quantification and for details on my account's ability to link the two.

Taking this as an example for independent branching, we get the following interpretation:

$$(536') \quad [3x: \text{frightened-elephant}(x)][\exists y: \text{hunter}(y)]\text{chased}(y,x) \ \& \\ [12y: \text{hunter}(y)][\exists x: \text{frightened-elephant}(x)]\text{chased}(y,x)$$

But the truth conditions of (536') are not at all what we might expect. Consider a situation in which some twelve elephants are each being chased by one hunter. In this situation, there are (at least) three elephants being chased by a hunter, and (at least) twelve hunters chasing an elephant, so (536') is true. But this is not a situation in which three elephants are being chased by twelve hunters -- it is instead either a situation in which three elephants are being chased by three hunters or a situation in which twelve elephants are being chased by twelve hunters.

How much of a problem is this? We might suspect that things have gone bad here just because we have used the mon  $\uparrow$  quantifier '(at least) 3' rather than the non-monotonic 'exactly 3'. Were we to take the quantifiers in (536) as precise cardinality quantifiers, it would regiment as:

$$(536'') \quad \begin{array}{l} [\exists!3 \ x: \text{frightened-elephant}(x)] \ | \\ \text{chased}(y,x) \\ [\exists!12 \ y: \text{hunter}(y)] \ | \end{array}$$

and thus interpret as:

$$(536''') \quad [\exists!3 \ x: \text{frightened-elephant}(x)][\exists y: \text{hunter}(y)]\text{chased}(y,x) \\ \ \& \ [\exists!12 \ y: \text{hunter}(y)][\exists x: \text{frightened-} \\ \ \text{elephant}(x)]\text{chased}(y,x)$$

(536''') is not true when twelve hunters chase twelve different elephants; it in fact is not true if more than three elephants are



chased. Thus it might seem to provide a better interpretation of (536) than that allowed by the  $\text{mon } \uparrow$  quantifiers.

Things begin to get a bit muddled here. Consider the following four types of situation:

- (S1) Three elephants are each being chased by four hunters; no other elephants are chased by hunters; no other hunters chase elephants.
- (S2) Three elephants are each chased by four hunters; one other elephant is chased by one other hunter; no other elephants are chased by hunters; no other hunters chase elephants.
- (S3) Four elephants are all chased by all of fifteen hunters; no other elephants are chased by hunters; no other hunters chase elephants.
- (S4) Twelve elephants are each chased by one hunter; no hunter chases more than one elephant; no other elephants are chased by hunters' no other hunters chase elephants.

Now consider which of these four situations makes true which of the following:

- (536) Three frightened elephants were chased by twelve hunters.
- (537) Exactly three frightened elephants were chased by exactly twelve hunters.
- (538) At least three frightened elephants were chased by at least twelve hunters.

Intuitions in these matters are somewhat unreliable, but I find that (536) is definitely true in (S1), (S2), and (S3), and definitely false in (S4); that (537) is definitely true in (S1), may be true in (S2), and is definitely false in (S3) and (S4); and that (538) is true in all four

situations. Sher's formalism allows only (536'), which is true in all four and thus plausible corresponds to (538), and (536'''), which is true only in (S1) and which thus corresponds only imperfectly to the (difficult-to-comprehend) (537). To (536), and the intermediate situations (S2) and (S3), Sher possesses no analog.

To summarize, Sher's attempt to use maximality conditions to unify branching structures containing quantifiers of differing monotonicity contains the seeds of its own destruction. In order to make the approach at all plausible, the maximality condition must be weakened to the point that the second-order semantics for complexly dependent branching structures collapses into the first-order semantics for independently branching structures. Once this collapse has occurred, however, we discover that the first-order semantics gives inadequate interpretations for those branching structures which contain  $\text{mon } \uparrow$  quantifiers, such as (536).<sup>403</sup> It looks, then, as if, should we choose to salvage Sher's approach, we must introduce a separate semantic rule for interpreting those  $\text{mon } \uparrow$  structures. It has a certain ring of familiarity about it. Monotonicity, we are forced to conclude, is somehow deeply imbedded in the nature of branching quantification. To account for this fact, we will require a story about the nature of quantification which shows that monotonicity properties are also deeply imbedded in the nature of all quantification. The neo-Fregean account gives us no such story. The anaphoric account, however, as we saw in §3.3.1.3.1 above, does.

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<sup>403</sup>I assume here that, in the light of the failure of the explicitly non-monotonic (537) to conform to the intuitive truth conditions for (536), we must assume that the quantifiers of (536) are  $\text{mon } \uparrow$ . For discussion of why (538), which also uses  $\text{mon } \uparrow$  quantifiers, differs in truth conditions from (536), see §3.3.1.1.1 above.

### **§3.3.2.2.3 Quantifier Linearity in the Anaphoric Binding Framework**

Having canvassed other attempts to understand the possibility of partially ordered quantifiers, we now turn to the status of such quantifiers under the anaphoric account. Our results here will be rather mixed. On the one hand, many of the lessons learned from our earlier investigations can be profitably and naturally implemented within the anaphoric account. Thus we will show that the anaphoric account gives rise naturally to both ordered and unordered notions of classical and generalized quantification (in a way which satisfies the Contiguity requirement) and that ordered quantification, in deference to the Simplicity requirement, is indeed a more complex beast than unordered quantification. Our conclusion, drawn from consideration of the neo-Fregean work of Barwise and Sher, that branching structures have an important sensitivity to the monotonicity of the involved quantifiers, will be given an adequate (and heretofore lacking) explanation deriving from fundamental properties of anaphoric quantification. Various conclusions about the relation between branching quantification and natural language, such as the needs to avoid 'massive nuclei' readings and to capture Sher's families of readings, will also fall out readily from previously established facts about the anaphoric account.

Nevertheless, the story is not entirely a happy one. We will also find that the notion of quantifier order which we do ultimately derive suffices only to capture a small range of the potential branched readings. To be precise, we will find that no two quantifiers which are unordered with respect to each other can have large scope with respect to any other quantifiers. Pictorially, we are allowed a single initial branching, but any subsequent quantification must be linear:

$$(539) (Q_{n+1} x_{n+1}) \dots (Q_{n+m} x_{n+m}) \dots \left( \begin{array}{c} (Q_1 x_1) \backslash \\ \dots \\ \dots \\ (Q_n x_n) / \end{array} \right) \phi$$

We will close our consideration of partially ordered quantification with some speculation on how dissatisfying we ought to find these limitations.

### §3.3.2.2.3.1 Order Independence of Simple Distribution

In §3.3.1.1 we considered a few examples of variable distribution in action, but all our examples involved only monadic cases, so we have yet to examine interaction among quantifiers under the anaphoric account. Now we will look at a couple of examples of distribution with multiple quantifier blocks, and in the process make a surprising discovery. We start with the sentence:

(540) Some girl danced with every boy.

which we can represent symbolically as:

(540-AA)  $[girl(y)]_y [boy(x)]_x \exists y \forall x \text{ danced-with}(x,y)$

where  $'[girl(y)]_y'$  and  $'[boy(x)]_x'$  act to restrict the terminal occurrences of  $x$  and  $y$ , respectively, and  $'\exists y'$  and  $'\forall x'$  serve to distribute those variables once bound by the restrictors. Through the restriction, the variable  $y$  comes to refer to all girls, while the variable  $x$  comes to refer to all boys. Put more formally, we have:

(541)  $\text{ref}('y') = \{z \mid z \text{ is a girl}\}$

(542)  $\text{ref}('x') = \{z \mid z \text{ is a boy}\}$ <sup>404</sup>

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<sup>404</sup>Where, again, the use of the set-theoretic terminology here is a mere convenience. Despite appearances, the restricted variables are taken to refer plurally to objects, rather than singularly to sets of objects.

These references are first distributed. We know, from above, that:

$$(543) \exists y \forall x \text{ danced-with}(x,y)$$

is true if:

$$(544) \forall x \text{ danced-with}(x,y')$$

is true for some  $y'$  in the distributed reference of  $y$ . By the distribution rule for ' $\exists$ ', we know:

$$(545) D_{\exists}(\text{ref}('y')) = \{\{Y\} \mid Y \in \text{ref}('y')\}^{405}$$

We can thus equate (543) with the disjunction:

$$(546) \bigvee_{Y \in \text{ref}(y)} \forall x \text{ danced-with}(x, \underline{Y})^{406}$$

Similarly, the distribution rule for ' $\forall$ ' tells us that:

$$(547) D_{\forall}(\text{ref}('x')) = \{\text{ref}('x')\}$$

so we can simplify the above disjunction to:

$$(548) \bigvee_{Y \in \text{ref}(y)} \text{danced-with}(\underline{X}, \underline{Y})$$

where  $\underline{X}$  refers to all boys. This disjunction will be true if at least one disjunct is true -- if, that is, there is at least one girl such that the disjunct corresponding to that girl is true. A particular will be true if the girl corresponding to that disjunct danced with all boys. Thus the formal (540-AA) will be true iff some girl danced with every boy -- exactly the result desired.

Now let's look at (540) with the scopes reversed -- the English sentence:

$$(549) \text{Every boy danced with some girl.}$$

formalized as:

$$(549-AA) [\text{boy}(x)]_x [\text{girl}(y)]_y \forall x \exists y \text{ danced-with}(x,y)$$

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<sup>405</sup>I assume here that we are interpreting ' $\exists$ ' according to the strong distribution law. Nothing in the subsequent discussion depends on this assumption.

<sup>406</sup>Where, as in the discussion of §3.3.1.1,  $\underline{Y}$  is a new lexical item referring to  $Y$ .

As before, we have:

$$(541) \text{ ref('y')} = \{z \mid z \text{ is a girl}\}$$

$$(542) \text{ ref('x')} = \{z \mid z \text{ is a boy}\}$$

and by the distribution rule for ' $\forall$ ' we have:

$$(547) D_{\forall}(\text{ref('x')}) = \{\text{ref('x')}\}$$

so (549-AA) is equivalent to the disjunction:

$$(550) \bigvee_{x=\text{ref('x')}} \exists y \text{ danced-with}(\underline{x}, y)$$

which, since the disjunction has only a single element, is itself equivalent to:

$$(551) \exists y \text{ danced-with}(\underline{x}, y)$$

Applying the same procedure to the existential distributor, we conclude that (551) is equivalent to the disjunction:

$$(552) \bigvee_{y=\text{ref('y')}} \text{danced-with}(\underline{x}, \underline{y})$$

But this is the same disjunction as (548), so we have reached the surprising conclusion that (549) receives the same truth conditions as (540)!

Of course, the English sentences (540) and (549) are not equivalent, so it is to the detriment of the anaphoric account that it judges them so. Shortly we will offer some diagnostic remarks and thus develop new insight into distribution, but first I want to explore further the underlying problem. The equivalence of (540) and (549) is a particular manifestation of the following fact:

**Claim:** If  $D_1, \dots, D_n$  are distributors;  $\psi(x_1, \dots, x_n)$  is a formula with  $x_1, \dots, x_n$  free;  $\phi_1(x_1), \dots, \phi_n(x_n)$  are formulae with only  $x_i$  free, for the relevant  $i$ ; and  $p$  is a permutation on  $\{1, \dots, n\}$ , then:

$$(A) [\varphi_1(x_1)]x_1 \dots [\varphi_n(x_n)]x_n (D_1x_1) \dots (D_nx_n) \Psi$$

$(x_1, \dots, x_n)$  is equivalent to:

$$(B) [\varphi_1(x_1)]x_1 \dots [\varphi_n(x_n)]x_n (D_{p(1)}x_{p(1)}) \dots (D_{p(n)}x_{p(n)}) \\ \Psi(x_1, \dots, x_n)$$

**Proof:** The variables  $x_1, \dots, x_n$  are initially assigned through restriction the referents  $\text{ext}(\varphi_1), \text{ext}(\varphi_2), \dots, \text{ext}(\varphi_n)$ . Let  $R_1, \dots, R_n$  be referring expressions with these referents. In (A), we then apply the first distributor, which acts on  $x_1$  to create:

$$D_{D_1}(R_1) = \{X_1^1, \dots, X_n^1, \dots\}^{407}$$

(A) is thus equivalent to the disjunction:

$$(A') \bigvee_{X_i^1 \in D_{D_1}(R_1)} (D_2x_2) \dots (D_nx_n) \Psi(\underline{X_i^1}, R_2, \dots, R_n)$$

Similarly,

$$D_{D_2}(R_2) = \{X_1^2, \dots, X_n^2, \dots\}$$

So each (j<sup>th</sup>) disjunct of (A') can be further analyzed into:

$$(C) \bigvee_{X_i^2 \in D_{D_2}(R_2)} (D_3x_3) \dots (D_nx_n) \Psi(\underline{X_j^1}, \underline{X_i^2}, \dots, R_n)$$

Proceeding in this manner, we see that (A) can be fully analyzed into:

$$(A'') \bigvee_{X_i^1 \in D_{D_1}(R_1)} \bigvee_{X_i^2 \in D_{D_2}(R_2)} \dots \bigvee_{X_i^n \in D_{D_n}(R_n)} \Psi(\underline{X_{i_1}^1}, \underline{X_{i_2}^2}, \dots, \underline{X_{i_n}^n})$$

or, in expanded form,

$$(A''') \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_1^n}) \vee \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_1^{n-1}}, \underline{X_2^n}) \vee \dots \vee \Psi \\ (\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_1^{n-1}}, \underline{X_m^n}) \vee \dots \vee \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_2^{n-1}}, \underline{X_1^n}) \vee \\ \dots \vee \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_{m_1}^{n-1}}, \underline{X_{m_2}^n}) \vee \dots \vee \Psi \\ (\underline{X_2^1}, \underline{X_1^2}, \dots, \underline{X_1^n}) \vee \dots \vee \Psi(\underline{X_{m_1}^1}, \underline{X_{m_2}^2}, \dots, \underline{X_{m_n}^n}) \vee \dots$$

Since ' $\vee$ ' is associative, (A''') can be rearranged into the form:

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<sup>407</sup>Where, of course, the sequence of distributed references may be infinite.

$$\begin{aligned}
(B''') \quad & \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_1^{p(n)}}, \dots, \underline{X_1^n}) \vee \\
& \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_1^{p(n)-1}}, \underline{X_2^{p(n)}}, \underline{X_1^{p(n)+1}}, \dots, \underline{X_1^n}) \vee \dots \vee \\
& \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_1^{p(n)-1}}, \underline{X_m^{p(n)}}, \underline{X_1^{p(n)+1}}, \dots, \underline{X_1^n}) \vee \dots \vee \\
& \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_2^{p(n)-1}}, \dots, \underline{X_1^n}) \vee \dots \vee \\
& \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_{m_1}^{p(n)-1}}, \dots, \underline{X_{m_2}^{p(n)}}, \dots, \underline{X_1^n}) \vee \dots \vee \\
& \Psi(\underline{X_1^1}, \underline{X_1^2}, \dots, \underline{X_2^{p(1)}}, \dots, \underline{X_1^n}) \vee \dots \vee \\
& \Psi(\underline{X_{m_1}^1}, \underline{X_{m_2}^2}, \dots, \underline{X_{m_n}^n}) \vee \dots
\end{aligned}$$

which may then be more succinctly stated as:

$$(B'') \quad \bigvee_{X_{i\rho(1)}^{p(1)} \in \mathcal{D}_{D\rho(1)}(R_{\rho(1)})} \bigvee_{X_{i\rho(2)}^{p(2)} \in \mathcal{D}_{D\rho(2)}(R_{\rho(2)})} \dots \bigvee_{X_{i\rho(n)}^{p(n)} \in \mathcal{D}_{D\rho(n)}(R_{\rho(n)})} \Psi(\underline{X_{i_1}^1}, \underline{X_{i_2}^2}, \dots, \underline{X_{i_n}^n})$$

which is, in turn, equivalent to (B).

What this proof shows is that quantification, as I have defined it, is not, despite syntactic appearances, linearly ordered. In fact, quantifier prefixes are completely unordered. Given this, it follows immediately that:

$$(540-AA) \quad [\text{girl}(y)]_Y [\text{boy}(x)]_X \exists y \forall x \text{ danced-with}(x,y)$$

and:

$$(549-AA) \quad [\text{boy}(x)]_X [\text{girl}(y)]_Y \forall x \exists y \text{ danced-with}(x,y)$$

are equivalent, since we are free to swap the order of the universal and existential distributors.

### §3.3.2.2.3.2 Simple Distribution and Cumulative Quantification

Linear ordering is a feature of quantification as we usually understand it, so at some point we will have to explain how it enters the picture. However, if we want to understand branching quantifiers, then a picture of quantification which imposes the *minimal partial order* on quantifier prefixes seems like a good place to start.



And, in fact, an interesting array of putative branching cases are well analyzed by the distributive framework as it now stands. These cases are not those most commonly associated in the literature with issues of branching, but I think that, nonetheless, they are the clearest cases of the phenomenon. Some explicit discussion of the type of case which interests me here occurs in [Sher 1990]; I will start by considering one of her cases. Consider the sentence:

(536) Three frightened elephants were chased by twelve hunters.

There are two linear readings available for this sentence:

(536a) [3x: frightened-elephant(x)] [12y: hunter(y)] chased(y,x)

(536b) [12y: hunter(y)] [3x: frightened-elephant(x)] chased(y,x)

On (536a), there are three elephants, *each of which* was chased by twelve hunters -- thus there are (up to) 36 hunters involved. On (536b), there are twelve hunters, each of which chased three elephants, and thus (up to) 36 elephants. (536a) and (536b) are perfectly legitimate readings of (536), but there is yet another reading available, on which there are not 36 hunters or 36 elephants, but just three elephants and twelve hunters. If anything, this is the preferred reading. It is also a reading which cannot be captured using linear quantification.<sup>408</sup>

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<sup>408</sup>One can, I suppose, write a linear sentence like:

(FN 221)  $(\exists x_1)(\exists x_2)(\exists x_3)(\exists y_1)(\exists y_2)(\exists y_3)(\exists y_4)(\exists y_5)(\exists y_6)(\exists y_7)(\exists y_8)(\exists y_9)(\exists y_{10})(\exists y_{11})(\exists y_{12})(\text{frightened-elephant } x_1 \text{ \& frightened-elephant } x_2 \text{ \& frightened-elephant } x_3 \text{ \& hunter } y_1 \text{ \& hunter } y_2 \text{ \& ... \& hunter } y_{12} \text{ \& chased } y_1, x_1 \text{ \& ... \& chased } y_{12}, x_3)$

to capture (536) (but are the chasing relations as desired here?). However, this 'decompositional' strategy is not available with all determiners: compare:

(FN 222) Finitely many frightened elephants were chased by uncountably many hunters.

(FN 223) Several frightened elephants were chased by many hunters.

(FN 224) Most frightened elephants were chased by most hunters.

Now consider how (536) analyzed using the distribution framework developed above. We have a regimentation of the form:

$$(536c) \text{ [frightened-elephant}(x)\text{]}_x \text{ [hunter}(y)\text{]}_y (3x)(12y) \\ \text{chased}(y,x)$$

(recall that the order of the determiners doesn't matter). The (undistributed) 'x' then refers, after restriction, to all frightened elephants, while (undistributed) 'y' refers to all hunters. After distribution, (536c) is thus equivalent to:

$$(536d) \quad \bigvee_{\substack{X=\{x_1, x_2, x_3\}, \\ Xx_i \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \bigvee_{\substack{Y=\{y_1, y_2, \dots, y_{12}\}, \\ Yy_i \text{ a hunter for } i=1,2, \dots, 12}} \text{chased}(Y,X)$$

which is true just in case there are some three frightened elephants and some twelve hunters such that the latter chased the former.

The (partially ordered) quantification thus allows us to pick out three elephants and twelve hunters -- *simpliciter*, not per hunter/elephant. What we have here is a method of accommodating within a project of branching quantification what has in the literature generally been called cumulative or collective quantification.<sup>409</sup> Thus sentences such as:

(553) Three professors graded five exams.

(554) 300 companies ordered 5000 computers.

also analyze easily using the unordered distribution provided by the anaphoric account. In (552), we distribute the professors into groups of three, and the professors into groups of five, and look for a grading of a second group by a first group. Similarly, in (553) we distribute the

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<sup>409</sup>See, e.g., [Scha 1984] or [Davies 1982].

companies into groups of 300 and the computers into groups of 5000, and look for orderings of a second group by a first group.<sup>410</sup>

### §3.3.2.2.3.2.1 Cumulative Quantification and Plural Reference

The real strength of the anaphoric's account of cumulative quantification (in the guise of minimally ordered quantification), however, only becomes apparent when we consider how the account interacts with earlier observed features of polyadic plurally referring sentences. Consider sentence (536) again, with the preferred reading calling for only three elephants and only twelve hunters. *Prima facie*, the linear readings (536a) and (536b) are *compatible* with such readings -- the linearity only allows that there be more than three elephants/more than twelve hunters, it doesn't require such superfluity. Thus when there are only three and only twelve, (536a) and (536b) are both true (but, of course, they are not only true then).

However, there are other ways (536) can be true which are *strictly* incompatible with linear analysis. Consider the following set of circumstances:

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<sup>410</sup>There is a potential further difficulty introduced here by the possible non-extensionality of the second argument position in 'ordered'. Thus it seems *prima facie* possible that companies might order computers even if (a) there are no particular computers they want or request, or even (b) there are no computers at all. The difficulty here is a specific instance of the general problem of verbs which generate non-extensional contexts without the presence of explicit sentential operators (like 'that'-clauses) which allow the use of scope distinctions in analysis. The general problem is a well-known one, and shows up as well in sentences such as:

(FN 225) Bob is looking for a unicorn.

(FN 226) Albert studies tigers.

(FN 227) Fred wants a sloop.

I have no insight into the proper treatment of such contexts (the three immediate alternatives -- denying the apparent non-extensionality, searching for hidden or implicit 'that'- and other sentential operators, or explaining ambiguities of quantified noun phrases in non-extensional contexts through other than scope mechanisms -- all strike me as importantly flawed, although a full discussion is not possible here). See [Quine 1956] and [Montague 1973] for further discussion.

(555) Elephant  $E_1$  is chased by four hunters; elephant  $E_2$  is chased  
by seven hunters; elephant  $E_3$  is chased by one hunter.

Intuitively, this looks like a situation in which three elephants are chased by twelve hunters, so (555) ought to entail the truth of (536). But clearly no linear (first-order) analysis of (536) will bring out that entailment relationship. Using the distribution framework, however, the relation between (555) and (536) is seen easily: we take  $E_1$ ,  $E_2$ , and  $E_3$  as our three elephants, and collect up the four hunters chasing  $E_1$ , the seven chasing  $E_2$ , and the one chasing  $E_3$  to get twelve hunters. We then note that the latter are chasing the former, so (536) is true.

What we see here, then, is a profitable interaction between the distributive quantificational apparatus and the array of readings for plural referring sentences. The distribution performed on (536) gives us two plural referring expressions related by the predicate 'chased'; that distributed claim then has as many readings available as the similar:

(556) Albert, Beth and Charles were chased by Dianne, Egbert,  
Francine, George, Hilbert, Iona, Jasmine, Kristen, Laura,  
Mark, Norbert, and Occam.

Thus, for example, each elephant could be chased by all twelve hunters (individually), the three elephants could, en masse, be chased by each of the twelve hunters, the twelve hunters could collectively chase each of the three elephants, the three could collectively be chased by the twelve, each elephant could be chased by four hunters (en masse or individually), or the first two elephants could (collectively) be chased (collectively) by the first eight hunters while the second and third

elephants were (collectively) chased (collectively) by hunters three through twelve.<sup>411</sup>

This interaction between the distribution mechanisms and the plural readings allows us to reproduce, in a more satisfying way, Sher's derivation of a 'family' of branching readings. Thus consider again Barwise's:

(557) More than half the dots and more than half the stars are all connected by lines.

we have the regimentation:

(558) [dots  $x$ ] <sub>$x$</sub>  [stars  $y$ ] <sub>$y$</sub>  ( $>\frac{1}{2} x$ ) ( $>\frac{1}{2} y$ ) connected-by-lines  $x, y$

which, through the normal distribution procedure, will be true if there is some  $R_1$  referring to more than half of the dots and some  $R_2$  referring to more than half of the stars such that:

(559)  $R_1$  and  $R_2$  are connected by lines.

is true. As we saw in §3.2.1.1.2.2 (especially in §3.2.1.1.2.2.2), there are many ways in which (559) could be true. One of them is what I above called the 'each-all' reading, in which each object to which  $R_1$  refers is connected by a line to each object to which  $R_2$  refers. It is this reading which Barwise focuses on in his paper<sup>412</sup>, giving rise to his problem with 'massive nuclei'. However, we can also have perfectly valid 'each-some' readings, in which each dot in  $R_1$  is connected to some dot

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<sup>411</sup>It is perhaps not entirely clear how elephants could be chased collectively who are not also chased individually or how hunters could chase collectively who do not also chase individually. I take it, however, that with sufficient imagination convincing scenarios can be designed. I leave the imagination as an exercise for the reader.

<sup>412</sup>The 'all' in:

(557) More than half of the dots and more than half of the lines are *all* connected by lines.

creates, as [Sher 1990] notes, a preference for the 'each-all' reading. Again, whether that preference is semantic or pragmatic I leave an open question.

in  $R_2$  (and vice versa), or 'one-one' readings, in which each dot in  $R_1$  is connected to some unique dot in  $R_2$  (and vice versa). See the diagrams in §3.3.2.2.2.3.1 for illustrations of the first two of these three readings.

More generally, we see that the independently motivated functioning of polyadic plurally referring sentences generates the full range of Sher's family of branched readings. Recall from §3.2.1.1.2.2.2 above the following distinguished 'fundamentally singular' readings:

- (1) The all-all-...-all readings.
- (2) The 1-1-...-1 readings.
- (3) The each - two-or-more - two-or-more - two-or-more - ... - two-or-more readings.
- (4)  $p(1)$ -each -  $p(2)$ -more-than- $k_{p(2)}$  -  $p(3)$ -more-than- $k_{p(3)}$  - ... -  $p(n)$ -more-than- $k_{p(n)}$  readings.
- (5)  $\sum_{i,j=1}^n$  i-each - j-some readings ( $i \neq j$ ).
- (6)  $\sum_{i,j=1}^n$  i-each - j-at-least-half readings ( $i \neq j$ ).
- (7)  $p(1)$ -most -  $p(2)$ -most - ... -  $p(n)$ -most readings.

Various of these readings, when imposed on the PPR sentences of the form:

(560)  $R_1$   $\varphi$ 's  $R_2$ .

will produce various relations in Sher's family:

- (561) Most of the stars and most of the dots are all connected by lines.
- (562) Most of my right hand gloves and most of my left hand gloves match one to one.

(563) Most of my friends saw at least two of the same few Truffuat movies.

(564) The same few characters appear in many of her early novels.

(565) Most of the boys and most of the girls in this party are such that each boy has been chased by at least half the girls, and each girl has been chased by at least half the boys.<sup>413</sup> [417]

Each of these sentences has the same basic logical structure -- in essence, a long disjunction whose disjuncts consist of, say, most of my right hand gloves and most of my left hand gloves, or most of my friends and a few Truffuat movies, related by some predicate. How they differ is in what reading of those PPR sentences making up the disjuncts is preferred. (561), for example, prefers an 'each-all' reading. (562) prefers a 'one-one' reading of the PPR disjuncts (corresponding to reading type (2) from above), while (563) asks for a 'each - two-or-more' reading, corresponding to type (3) above. Similarly, (564) and

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<sup>413</sup>These sentences are, obviously enough, not particularly pretty English. In some of them, the use of additional markers to indicate the type of plural reading desired is so obtrusive as to require, in my opinion, the introduction of further (possibly 'second-order' or set-theoretical) logical mechanisms for their analysis. My claim is that the readings which Sher wants to highlight should all be available from the 'cleaner':

(FN 228) Most of my right hand gloves and most of my left hand gloves match.

(FN 229) Most of my friends saw a few Truffuat movies.

(FN 230) A few characters repeatedly appear in many of her early novels.

(FN 231) Most of the boys and most of the girls in this party chased each other.

How available the reading is depends on how natural the practice of looking for such a reading is (various contextual clues further influence availability). As a rough guide, I find that (FN 228) and (FN 229) can relatively easily be read as Sher desires, (FN 230) only with some difficulty, and (FN 231) only with extraordinary mental contortions.

(565) request readings types (4) and (6), respectively. Our original (536) probably favors a type (5) 'each-some' reading, and with some ingenuity, type (7) readings can also be devised.

However, the anaphoric account's derivation of this family of reading allows more options than Sher's reading. All of the above options are drawn only from the fundamentally singular readings of the relevant PPR sentence of the form (560). There are, of course, many other readings which are not fundamentally singular. Thus consider a situation in which twelve hunters act collectively, and then chase three separate elephants. This is clearly a situation in which (536) is true, but none of Sher's readings will capture that truth, since none of the twelve hunters is (on his own) engaged in chasing elephants. Any relation condition which we require between the sets of hunters and the sets of elephants will fail, because on the singular level the 'chasing' relation has an empty intersection with the product space of hunters and elephants.

In addition to capturing collective as well as distributive readings of simple branched sentences, the way in which the anaphoric account through its minimally ordered quantifiers derives the phenomenon of branching enjoys a second advantage over Sher's story. Unlike Sher, we have a ready explanation of what the exact range of the family of readings of branched sentence is, and why it is what it is. Sher merely postulates that we can in various ways weaken Barwise's 'each-all' condition relating the sets of objects picked out by the two coordinate NPs. She has thus has no account of exactly how that condition can be weakened, and in particular no explanation of the fact that there are certain minimal points beyond which the conditions cannot be weakened.



Inspection of cases will reveal that the weakest possible correlating condition is an 'each-some/some-each' condition. To see this, consider again:

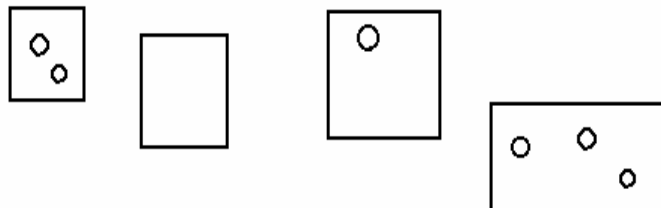
(536) Three frightened elephants were chased by twelve hunters. Imagine we pick some three elephants and some twelve hunters, and then claim that they are correlated via the 'chased' relation in such a way that only two of the three elephants were chased by any of the hunters, or in such a way that only eleven of the twelve hunters did any of the chasing. Neither of these correlations will suffice for the truth of (536). But why? Sher has no answer, but we can see that this fact follows from the truth conditions determined above for PPR sentences. Thus given a sentence of the form:

(556) Albert, Beth and Charles were chased by Dianne, Egbert, Francine, George, Hilbert, Iona, Jasmine, Kristen, Laura, Mark, Norbert, and Occam.

it follows from the Involvement Principle that all of Albert, Beth, and Charles must be chased and that all of Dianne, Egbert, Francine, George, Hilbert, Iona, Jasmine, Kristen, Laura, Mark, Norbert, and Occam must chase.<sup>414</sup> Much of the complexity of those minimal cases of branching

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<sup>414</sup>Note, thus, that corresponding to those cases -- such as those discussed in §3.2.1.1.2.1.3 -- which provide prima facie violations of the Involvement Principle, there are branched sentences which prima facie have readings weaker than the 'each-some/some-each' condition. Thus consider again the diagram:



and consider whether we can truly say:  
(FN 232) Four squares contain all the circles.

quantification which are discussed in the literature on cumulative quantification, then, come not from complexities of the quantification, but from complexities of plural reference. Only an account of quantification which explains why plural reference comes into the picture in the first place can successfully place the complexities where they belong.

### §3.3.2.2.3.2.2 Simple Distribution and Quantifiers of Mixed Monotonicity

All the branching cases we have considered so far use only monotone increasing quantifiers. This focus on positive monotonicity should come as no surprise, given my system's built-in bias in favor of  $\text{mon} \uparrow$  distribution. We now turn to the effect of introducing negation into branching structures in an attempt to see how well we can accommodate  $\text{mon} \downarrow$  and non-monotonic quantifiers in such structures.

Note first that the order-independence of distribution is lost when negations are added. Thus there is a truth-conditional difference between:

$$(566) [\text{frightened-elephant}(x)]_x [\text{hunter}(y)]_y (3x) \neg (12y) \\ \text{chased}(y,x)$$

and:

$$(567) [\text{frightened-elephant}(x)]_x [\text{hunter}(y)]_y (12y) \neg (3x) \\ \text{chased}(y,x)$$

In (566), the first distribution creates a disjunction of the form:

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(FN 232) strikes me as acceptable here, even though there will be no correlating condition which has all four squares involved in the containing. Similar the following appear to have acceptable branched readings:

(FN 233) Any three philosophers can outwit any five linguists.

(FN 234) Any two European nations are wealthier than any seven African nations.

even when not all of the philosophers are involved in the outwitting, or not all of the European nations are involved in the out-earning.

$$(568) \quad \bigvee_{\substack{X=\{x_1, x_2, x_3\}, \\ Xx_i \text{ a frightened elephant} \\ \text{for } i=1,2,3}} [\text{hunter}(y)]_y \neg(12y) \text{ chased}(y, \underline{X})$$

The second distribution then creates a further disjunction inside the scope of the negation:

$$(569) \quad \bigvee_{\substack{X=\{x_1, x_2, x_3\}, \\ Xx_i \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \neg \bigvee_{\substack{Y=\{y_1, y_2, \dots, y_{12}\}, \\ y_i \text{ a hunter for } i=1,2, \dots, 12}} \text{ chased}(\underline{Y}, \underline{X})$$

which, via an application of the DeMorgan laws, is equivalent to:

$$(570) \quad \bigvee_{\substack{X=\{x_1, x_2, x_3\}, \\ Xx_i \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \bigwedge_{\substack{Y=\{y_1, y_2, \dots, y_{12}\}, \\ y_i \text{ a hunter for } i=1,2, \dots, 12}} \neg \text{ chased}(\underline{Y}, \underline{X})$$

By similar reasoning, (567) is equivalent to:

$$(571) \quad \bigvee_{\substack{Y=\{y_1, y_2, \dots, y_{12}\}, \\ y_i \text{ a hunter for } i=1,2, \dots, 12}} \bigwedge_{\substack{X=\{x_1, x_2, x_3\}, \\ Xx_i \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \neg \text{ chased}(\underline{Y}, \underline{X})$$

(566), then, amounts to the assertion:

(572) Some three elephants were chased by no twelve hunters.

while (567) amounts to:

(573) There are some twelve hunters who chased no three elephants.

Sentences (566) and (567) thus do a reasonably good job of capturing the intuitive truth conditions of the following apparently branched constructions with quantifiers of mixed monotonicity:

(574) Three elephants were chased by at most eleven hunters.

(575) Twelve hunters chased at most two elephants.

Note, however, that it is important on my analysis that the monotone increasing quantifier come first in logical form of the sentence. This is because the following two sentences are equivalent:

$$(576) \quad [\text{frightened-elephant}(x)]_x [\text{hunter}(y)]_y \neg(3x)(12y) \text{ chased}(y, x)$$

(577)  $[\text{frightened-elephant}(x)]_x [\text{hunter}(y)]_y \neg(12y)(3x)$

$\text{chased}(y,x)$

-- where, of course, these two correspond to:

(576a) At most two elephants were chased by twelve hunters.

(577a) At most eleven hunters chased three elephants.

The distributors are not order-independent across the scope of a negation, but once both are within the scope of a negation operator, they are again order-independent with respect to each other. Note that the one set of truth conditions generated by the equivalent (576) and (577) is a poor match for both (576a) and (577a). We have (576)/(577) equivalent to:

$$(578) \neg \bigvee_{\substack{X=\{x_1, x_2, x_3\}, \\ x_i \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \bigvee_{\substack{Y=\{y_1, y_2, \dots, y_{12}\}, \\ y_i \text{ a hunter for } i=1,2, \dots, 12}} \text{chased}(\underline{Y}, \underline{X})$$

which, via the DeMorgan laws, is equivalent to:

$$(579) \bigwedge_{\substack{X=\{x_1, x_2, x_3\}, \\ x_i \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \bigwedge_{\substack{Y=\{y_1, y_2, \dots, y_{12}\}, \\ y_i \text{ a hunter for } i=1,2, \dots, 12}} \neg \text{chased}(\underline{Y}, \underline{X})$$

(579) asserts that given any three elephants and any twelve hunters, the hunters did not chase the elephants. But neither (576a) nor (577a) require anything this strong.<sup>415</sup>

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<sup>415</sup>Actually (as emphasized below) (576a) and (577a) are difficult to interpret at all. Of course, we don't want the linear readings of either:

(FN 235) [at most 2x: elephant x][12y: hunter y] chased y,x

(FN 236) [at most 12y: hunter y][3x: elephant x] chased y,x

(although these are permissible readings of the English sentences). But grasping an appropriate branched reading is difficult. It is tempting, I find, to hear (576a) and (577a) as both equivalent to:

(FN 237) At most three elephants were chased by at most twelve hunters.

which would accord with the truth conditions which my account (seemingly incorrectly) assigns to them. Giving into this temptation, however, amounts to denying the apparent synonymies between (575) and (576a) and between (574) and (577a). The intended readings ought to require (a) in the case of (576a) that there are some twelve hunters who (taken together) chased at most two elephants and (b) in the case of (577a)

A point on syntax is in order here. The negations in (566), (567), (576), (577) above are introduced on the level of analysis because my system allows only  $\text{mon } \uparrow$  distributors primitively, and thus understands  $\text{mon } \downarrow$  distributor as implicitly negated. When the  $\text{mon } \downarrow$  distributor has wide scope, we can no longer keep that implicit negation appropriately connected to its corresponding  $\text{mon } \uparrow$  distributor, because the two adjacent distributors are semantically indifferent to their scope orderings. One might think, however, that there is an easy solution to this problem. If this is a genuinely branched structure, then the underlying syntax ought to be (e.g.):

$$\begin{array}{r}
 \neg(3x) \backslash \\
 \phantom{\neg(3x)} \phantom{\backslash} \\
 (576b) \text{ [frightened-elephant}(x)\text{]}_x \text{ [hunter}(y)\text{]}_y \quad \text{chased}(x,y) \\
 \phantom{\neg(3x)} \phantom{\backslash} / \\
 \phantom{\neg(3x)} \phantom{\backslash} \\
 (12y) /
 \end{array}$$

and there would be no opportunity for the order-independence of the (3x) and (12y) distributors to allow the (12y) distributor to come under the influence of the negation.

However, I deny that (576) (or the corresponding English (576a)) has such a branched syntax. My approach to quantifier branching will be to deny that there is *syntactic* branching at all, and then to derive the necessary partial ordering of quantifiers purely from semantic properties of those quantifiers. Syntactically, then, a branched English construction like (576a) has the usual 'linear' phrase-structure tree form, and in such a form, when the  $\text{mon } \downarrow$  quantifier has wide scope, its

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that there are some three elephants who (taken together) were chased by at most eleven hunters.

implicit negation also has scope over the second  $\text{mon } \uparrow$  quantifier, and the undesired reading is generated. While my insistence that branching be confined to the semantics and derive from an underlying linear syntactic base creates difficulties here, we will see shortly that it also serves to provide natural constraints on the total range of permissible branchings.

My account thus predicts that branched constructions with quantifiers of mixed monotonicity will be acceptable only when the monotone increasing quantifier has largest scope. Of course, this restriction does not immediately rule out branched interpretations of (576a) and (577a), since on the level of logical form the quantifier which has largest surface form scope may end up with narrow scope. Thus we may have the following logical forms for (576a) and (577a):

(576c)  $[_S [_{NP} \text{ twelve hunters}]_{t_1} [_S [_{NP} \text{ at most three elephants}]_{t_2} [_S [_{NP} t_1] [_{VP} \text{ chased } t_2]]]]]$

(577c)  $[_S [_{NP} \text{ three elephants}]_{t_1} [_S [_{NP} \text{ at most twelve hunters}]_{t_2} [_S [_{NP} t_2] [_{VP} \text{ chased } t_1]]]]]$

in which case the usual branched reading goes through unproblematically. If, however, we genuinely want to hear (576a) and (577a) with the  $\text{mon } \downarrow$  quantifiers receiving wide scope, then we must accept as a weakness of my account the fact that it assigns truth conditions (579) to both.<sup>416</sup> Since, with Barwise, I find branched constructions with quantifiers of mixed monotonicity difficult to interpret, however, I am not inclined to take this weakness as decisive.

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<sup>416</sup>Although see again the suggestions in footnote 414 above that these truth conditions may be as good as any in such situations.

We have now seen that my account handles at least some constructions which mix monotone increasing and monotone decreasing quantifiers, as a result of the order-dependence induced by the implicit negation in the  $\text{mon} \downarrow$  distributor. I want to turn now to consideration of branched constructions with dual  $\text{mon} \downarrow$  quantifiers, such as:

(580) At most three elephants were chased by at most twelve hunters.

Prima facie, my account does quite poorly with such constructions, since they appear to be equivalent to:

(581)  $[\text{frightened-elephant}(x)]_x [\text{hunter}(y)]_y \neg(3x)\neg(12y)$   
 $\text{chased}(y,x)$

(with both primitive  $\text{mon} \uparrow$  distributors carrying a negation). But (581) is equivalent to:

(582)  $\neg \bigvee_{\substack{X=\{x_1,x_2,x_3\}, \\ X_{x_i} \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \neg \bigvee_{\substack{Y=\{y_1,y_2,\dots,y_{12}\}, \\ Y_{y_i} \text{ a hunter for } i=1,2,\dots,12}} \text{chased}(\underline{Y}, \underline{X})$

which is in turn equivalent to:

(583)  $\bigvee_{\substack{X=\{x_1,x_2,x_3\}, \\ X_{x_i} \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \bigwedge_{\substack{Y=\{y_1,y_2,\dots,y_{12}\}, \\ Y_{y_i} \text{ a hunter for } i=1,2,\dots,12}} \text{chased}(\underline{Y}, \underline{X})$

which requires that some three elephants are chased by every group of twelve hunters, i.e.:

(584) Three elephants were chased by all the hunters.<sup>417</sup>

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<sup>417</sup>Note, furthermore, that again the ordering of the two distributors matters here. Had we taken (580) as equivalent to:

(FN 238)  $[\text{frightened-elephant}(x)]_x [\text{hunter}(y)]_y \neg(12y)\neg(3x)$   
 $\text{chased}(y,x)$

our final truth conditions would have been:

(FN 239)  $\bigvee_{\substack{Y=\{y_1,y_2,\dots,y_{12}\}, \\ Y_{y_i} \text{ a hunter for } i=1,2,\dots,12}} \bigwedge_{\substack{X=\{x_1,x_2,x_3\}, \\ X_{x_i} \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \text{chased}(\underline{Y}, \underline{X})$

which is equivalent to:

(FN 240) Twelve hunters chased all the elephants.

Such truth conditions could hardly be farther from what is desired.

The desired truth conditions, of course, require that given any group of three elephants and any group of twelve hunters, there is no chasing of the first by the second. That is:

$$(579) \quad \bigwedge_{\substack{X=\{x_1, x_2, x_3\}, \\ X_{x_i} \text{ a frightened elephant} \\ \text{for } i=1,2,3}} \bigwedge_{\substack{Y=\{y_1, y_2, \dots, y_{12}\}, \\ Y_{y_i} \text{ a hunter for } i=1,2, \dots, 12}} \neg \text{chased}(\underline{Y}, \underline{X})$$

Such truth conditions will result from the starting analysis:

$$(576) \quad [\text{frightened-elephant}(x)]_x [\text{hunter}(y)]_y \neg (3x)(12y) \text{ chased}(y, x)$$

My suggestion, then, is that branched constructions with dual mon  $\downarrow$  distributors should be understood as having logical forms on analogy with (576). The idea here is that the single negation, respecting the fact that the two subsequent mon  $\uparrow$  distributors are order-independent, merges with both equally to form two mon  $\downarrow$  distributors -- since merging with either over the other would create a misleading impression of order-dependence. Branched dual mon  $\downarrow$  quantifiers, then, are not doubly negated, but doubly marked as singly negated.

The final issue to consider, then, is the presence of non-monotonic quantifiers, as in:

$$(585) \quad \text{Exactly three elephants were chased by exactly twelve hunters.}$$

I admit to finding such constructions extraordinarily difficult to understand (see §3.3.2.2.2.3.1.5 for further discussion), so to some extent I am willing simply to dismiss them.

I think, however, that my account will also explain some of the particular oddity of branched constructions with non-monotonic quantifiers. (585), to the extent that it is interpretable, clearly



requires that there be some three elephants and some twelve hunters such that the latter chased the former, none of the former were chased by any other hunters, and none of the latter chased any other elephants. What is not clear, however, is whether (585) permits any other elephant-chasing to occur outside these groups of three and twelve. On my understanding of (585), both non-monotonic distributors contain both negated and unnegated components. As we saw when considering the interaction of non-monotonic quantifiers and 'any' in §3.3.1.3.1 above, the behaviour of the internal negation in non-monotonic distributors is somewhat ambivalent -- it is not entirely clear to what extent that negation can 'escape' from the narrow confines of the non-monotonic distributor itself and influence the larger structure of the sentence. It is these dual ambiguous negations, I suggest (combined with the previously noted ambiguity between dual negation and dually marked single negation) which give rise to the ambiguous extension of the ban on chasing beyond the core three elephants and twelve hunters.

In summary, we make the following observations about branched constructions with quantifiers of mixed monotonicity. First, like Sher and unlike Barwise, we are able to interpret constructions which mix  $\text{mon } \uparrow$  and  $\text{mon } \downarrow$  quantifiers. Unlike Sher, however, our account is sensitive to differences in reading which emerge as the scopes of the  $\text{mon } \uparrow$  and  $\text{mon } \downarrow$  quantifiers are swapped. Like both Sher and Barwise, we can interpret constructions with dual  $\text{mon } \downarrow$  quantifiers. Like Barwise, we impose a different semantic mechanism in such constructions (in our case, the use of dually marked single negation). Finally, constructions with non-monotonic quantifiers are rejected, with at least the suggestion that they ought to be rejected anyway and that the anaphoric

account, unlike those of Barwise or Sher, can explain why they ought to be rejected. Throughout, the sensitivities to quantifier monotonicity first observed by Barwise and unsuccessfully rejected by Sher are preserved and expanded, but importantly they are further *explained*, by being situated in an account of quantification which has a previous bias in favor of  $\text{mon } \uparrow$  over  $\text{mon } \downarrow$  quantification.

### §3.3.2.2.3.3 Complex Distribution and Scope

It has now been shown that the mechanisms of distribution give rise to a highly successful account of branching quantifier sentences of the form:

$$(586) \begin{array}{l} (D_1 x_1) \quad | \\ \dots \quad | \quad \psi(x_1, \dots, x_n) \\ (D_n x_n) \quad | \end{array}$$

where  $\psi$  is quantifier-free -- an account with greater flexibility than any currently available. However, two shortcomings remain in the theory of distribution. First, we are still considerably short of a general explanation of branching structures, since we at the moment can handle only *fully* unordered quantifier prefixes. Second, and more importantly, we have no explanation as yet of the possibility of linearly ordered quantification.<sup>418</sup> To complete the story, we need some way to introduce ordering relations among distributors, giving rise to the full array of partially ordered prefixes -- and, in particular, allowing an explanation of the classical case of the linearly ordered quantifier prefix.

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<sup>418</sup>Of course, these two shortcomings are both manifestations of the same underlying problem: that we have no way to impose any ordering relation, whether wholly linear or partial (but nor degenerate) on distributional prefixes.

The key to achieving this completion of the story lies in considering the grammar of 'distribute'. We have been assuming that our distributors take a plural referring expression and distribute it into smaller packages. But distribution must be distribution *among* or *over* something. We must, that is, say to *what* these new plural referring expressions are being distributed. In the discussion above, we used as a default answer: 'the predicate containing the variable'. Thus when we had:

(587) (3x)(12y)chased(y,x)

the newly created referring expressions, referring to some three elephants or to some twelve hunters, are distributed to the predicate 'chased(y,x)' -- it is this predicate which must be true of the three elephants or of the twelve hunters. But this is not the only available option.

We might, for example, distribute the groups of three elephants to the open formula:

(588) (12y)chased(y,x)

By doing so, we would be asking for some group of three elephants of whom (588) was true. Three elephants, that is, who are such that 'were chased by twelve hunters' was true. But if E1 was chased by twelve hunters, and E2 was chased by twelve other hunters, and E3 was chased by yet another twelve, then we have three elephants who satisfy the predicate 'were chased by twelve hunters' -- but we also have a situation which supports the truth of the linear:

(536a) [3x: frightened-elephant x] [12y: hunter y] chased y,x

and not (necessarily) that of:

(536c)  $[\text{frightened-elephant}(x)]_x [\text{hunter}(y)]_y (3x)(12y)$   
 $\text{chased}(y,x)$

We are thus on our way to linearity.

### §3.3.2.2.3.3.1 Complex Distribution and Order-Dependence

We now want to make more precise the way in which we have a choice about what to distribute to. We will make use of the notion of  $\lambda$ -abstraction.

$\lambda$ -abstraction is the process of transforming formulae into predicates, as follows:

**( $\lambda$ -Abstract)** If  $\varphi(x_i)$  is an open formula such that, for all models  $M$ ,  $\Sigma_M$  is the set of sequences satisfying  $\varphi$  in  $M$ , then:

$$\lambda x_i(\varphi(x_i))$$

is a predicate whose extension in  $M$  is:

$$\{x \mid \exists \sigma \in \Sigma_M, \sigma(i) = x\}$$

Thus, for example, if we have the formula:

$$(589) (\forall y)Fxy$$

we can use  $\lambda$ -abstraction on  $x$  to create the new predicate:

$$(590) \lambda x((\forall y)Fxy)$$

which will be true of an object just in case every object bears the relation  $F$  to that first object.

We can thus distinguish two forms of distribution, simple and complex:

**(Simple Distribution)**  $D$  simply distributes a plurally referring ' $x$ ' in ' $(D x)\varphi$ ' if:

$$(D x)\varphi \equiv \bigvee_{Y \in D_D(x)} \varphi(x/Y)$$

**(Complex Distribution)**  $D$  complexly distributes a plurally referring ' $x$ ' in ' $\{D x\}\varphi$ ' if:

$$\{D x\}\varphi \equiv \bigvee_{y \in D_D(x)} [\lambda x(\varphi(x))]Y$$

I will use curly brackets -- '{ }' -- to distinguish complex from simple distribution.<sup>419</sup> Note that in the case where  $\varphi$  is quantifier free, simple and complex distribution trivially yield the same results.

Of course, it is not immediately clear that simple and complex distribution will ever yield different results. In classical logical systems, whenever we have a singular term  $t$ , the following are equivalent:

$$(591) [\lambda x(\varphi(x))]t$$

$$(592) \varphi(x/t)^{420}$$

This equivalence would seem to entail the further equivalence of simple and complex distribution, since it would allow us to move between the  $\lambda$ -extracted ' $[\lambda x(\varphi(x))]Y$ ' and the simple  $\varphi(x/Y)$ .

The classical equivalence between (591) and (592) ceases to hold, however, when we introduce *plural* referring terms. To say of John either that there is some woman such that that woman and he stand in the loving relation (' $(\exists x)Lxj$ ') is no different than saying of John that he possesses the property of being loved by some woman (' $[\lambda y(\exists x)Lxy]j$ ').

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<sup>419</sup>I leave an open question whether in natural language there is any syntactic distinction between simple and complex distribution, although I suspect that there is not. If there is not, then quantified claims in natural language will be uniformly ambiguous among ordered and un-ordered readings, although certain constructions may require unordered readings, such as those with coordinate NPs:

(FN 241) Several philosophers and several linguists argued.

<sup>420</sup>Were we so foolish as to consider, say, definite descriptions to form singular terms, this claim would not hold when intensional operators existed in the language. Thus, for example, the following are not equivalent:

(FN 242)  $[\lambda x(\text{odd}(x))](ix)(\text{number-of-planets}(x))$

(FN 243)  $\text{odd}((ix)(\text{number-of-planets}(x)))$

For similar reasons, the equivalence claim given in the main text is at least suspect when the language contains such hyperintensional operators as the psychological verbs.

But if Albert also possesses the property of being loved by some woman, then we can say that Albert and John possess the property of being loved by some woman:

$$(593) [\lambda y(\exists x)Lxy]J^{421}$$

but not that there is some woman such that that woman and John and Albert stand in the loving relation:

$$(594) (\exists x)LxJ$$

Since variable restriction will generally introduce plural referring expressions, there is a genuine distinction between simple and complex distribution.<sup>422</sup>

Having seen that simple and complex distribution offer two distinct alternatives, we can now prove the following crucial theorem, establishing the link between complex distribution and linear quantification:

**Linearity Theorem:** If  $Q_1, \dots, Q_n$  are determiners,  $\phi_1, \dots, \phi_n$  are formulae,  $\chi_1, \dots, \chi_n$  are variables, and  $\psi$  is a formula in  $\chi_1, \dots, \chi_n$ , then:

$$\begin{aligned} & [\phi_1(\chi_1)]\chi_1 \dots [\phi_n(\chi_n)]\chi_n \{Q_1 \chi_1\} \{Q_2 \chi_2\} \dots \{Q_n \chi_n\} \psi(\chi_1, \dots, \chi_n) \\ & \equiv [Q_1 \chi_1: \phi_1(\chi_1)] [Q_2 \chi_2: \phi_2(\chi_2)] \dots [Q_n \chi_n: \phi_n(\chi_n)] \psi(\chi_1, \dots, \chi_n) \end{aligned}$$

**Proof:** We proceed by induction on  $n$ . When  $n=0$ , then the two sentences are obviously equivalent. Assume then that for any  $m < n$ , we have established:

$$\begin{aligned} & [\phi_1(\chi_1)]\chi_1 \dots [\phi_m(\chi_m)]\chi_m \{Q_1 \chi_1\} \{Q_2 \chi_2\} \dots \{Q_m \chi_m\} \\ & \psi(\chi_1, \dots, \chi_m) \end{aligned}$$

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<sup>421</sup>Where 'J' is a term referring plurally to John and Albert.

<sup>422</sup>And, furthermore, in those cases in which there is only a single object in the extension of the restricting predicate, there is no distinction between branched and ordered readings.

$$\equiv [Q_1 \chi_1: \varphi_1(\chi_1)] [Q_2 \chi_2: \varphi_2(\chi_2)] \dots [Q_m \chi_m: \varphi_m(\chi_m)] \\ \Psi(\chi_1, \dots, \chi_m)$$

Consider first a model in which:

$$[\varphi_1(\chi_1)]\chi_1 \dots [\varphi_n(\chi_n)]\chi_n \{Q_1 \chi_1\} \{Q_2 \chi_2\} \dots \{Q_n \chi_n\} \\ \Psi(\chi_1, \dots, \chi_n)$$

is true. Then:

$$\lambda x_1 ([\varphi_2(\chi_2)]\chi_2 \dots [\varphi_n(\chi_n)]\chi_n \{Q_1 \chi_1\} \{Q_2 \chi_2\} \dots \{Q_n \chi_n\} \\ \Psi(\chi_1, \dots, \chi_n))\Phi$$

is true, for some  $\Phi$  referring to  $Q_1$  objects which are  $\varphi_1$ . This  $\lambda$ -  
extracted expression will be true just in case, for every  $\alpha$  in  
the referent of  $\Phi$ , the following is true:

$$[\varphi_2(\chi_2)]\chi_2 \dots [\varphi_n(\chi_n)]\chi_n \{Q_1 \chi_1\} \{Q_2 \chi_2\} \dots \{Q_n \chi_n\} \\ \Psi(\alpha, \dots, \chi_n)$$

By the inductive hypothesis, we then have the truth of all  
sentences of the form:

$$[Q_2 \chi_2: \varphi_1(\chi_1)] [Q_3 \chi_3: \varphi_3(\chi_3)] \dots [Q_n \chi_n: \varphi_n(\chi_n)] \Psi(\alpha, \dots, \chi_n)$$

for all  $\alpha$  in the referent of  $\Phi$ . But since  $\Phi$  refers to  $Q_1$  objects,  
the truth of all such sentences guarantee the truth of:

$$[Q_1 \chi_1: \varphi_1(\chi_1)] [Q_2 \chi_2: \varphi_2(\chi_2)] \dots [Q_n \chi_n: \varphi_n(\chi_n)] \Psi(\chi_1, \dots, \chi_n)$$

Similar considerations will show that any model which makes:

$$[Q_1 \chi_1: \varphi_1(\chi_1)] [Q_2 \chi_2: \varphi_2(\chi_2)] \dots [Q_n \chi_n: \varphi_n(\chi_n)] \Psi(\chi_1, \dots, \chi_n)$$

true also makes:

$$[\varphi_1(\chi_1)]\chi_1 \dots [\varphi_n(\chi_n)]\chi_n \{Q_1 \chi_1\} \{Q_2 \chi_2\} \dots \{Q_n \chi_n\} \\ \Psi(\chi_1, \dots, \chi_n)$$

true, so the two are equivalent. Thus by induction, the Linearity Theorem is established.<sup>423</sup>

Complex distribution thus gives us linearly ordered quantifier prefixes, while simple distribution gives us partially ordered prefixes. Note that we have now satisfied both Contiguity, since both the simple and complex notions of distribution arise naturally from the general restriction-and-distribution model advanced by the anaphoric account of variables, and Simplicity, since linear quantification results from the imposition of the *additional* mechanism of  $\lambda$ -abstraction onto the distributive process invoked in branching structures.

To see how the mechanisms of complex distribution ensure linearity, let us return to the earlier (§3.3.2.2.3.1) problematic:

(549) Every boy danced with some girl.

The universal quantifier in 'every boy' distributes the reference provided by 'boy' over the  $\lambda$ -abstracted predicate 'danced with some girl' to give rise to:

$$(549a) \bigvee_{x \in D_V(x)} [\lambda x((\exists y)D(x,y))]X$$

which, since  $D_V(x)$  has only a single member, is equivalent to:

$$(549b) [\lambda x((\exists y)D(x,y))]X$$

where 'X' refers to all boys. One way (549b) can be true is if X is given the fully distributive reading, thus taking it as:

$$(549c) \bigwedge_{b \in ref(X)} [\lambda x((\exists y)D(x,y))]b$$

which is in turn equivalent, since each 'b' refers singularly, to:

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<sup>423</sup>I assume throughout that  $Q_1, \dots, Q_n$  are all monotone increasing determiners. Other quantifiers derivatively expressible in my system are Boolean combinations of mon  $\uparrow$  determiners, so the Linearity Theorem as proven will immediately entail the appropriate linearity result for them as well.





[MOST w: Rwy]/

What we want, then, is to have the two 'most' distributors employ simple distribution, and the two 'every' quantifiers employ complex distribution over the 'most' quantifiers:

(597)  $[Tx]_x[Vy]_y[Rzx]_z[Rwy]_w \{ \forall x \} \{ \forall y \} (Mz)(Mw)(Hzw \ \& \ Hwz)$

(597) will then be equivalent to:

(598)  $(\lambda x)(\lambda y) \bigvee_{\substack{Z \subseteq EXT(R) \\ |Z| > EXT(R)}} \bigvee_{\substack{W, W \subseteq EXT(R) \\ |W| > EXT(R)}} (Hzw \ \& \ Hwz) \underline{XY}$

where X refers to all townsmen and Y refers to all villagers. What is required here is that, for any choices of villager and townsman, there be some collection of most relatives of the villager and some collection of most relatives of the townsman such that the first hate the second -- exactly the desired reading. In general, we can use simple distribution to create an unordered quantifier prefix, and then use subsequent complex distribution to further linearly quantify that initial unordered sentence.

**§3.3.2.2.3.3.2.1 Limitations of the Theory**

The interaction between simple and complex distribution in partially ordered quantifier prefixes is not a complete success, however. While we can properly analyze:

(595) Most relatives of every villager and most relatives of every townsman hate each other.

we cannot capture:

(599) Some relative of most villager and some relative of most townsmen hate each other.

(599) ought to correspond to a structure represented by:

$$(600) \quad \frac{[\text{MOST } x: Vx] \setminus [\exists z: Rzx][\exists w: Rwy](Hwz \ \& \ Hzw)}{[\text{MOST } y: Ty]}$$

Here the two 'most' quantifiers need to be unordered with respect to each other, but they need to have scope over the 'some' quantifiers. The only way to do this is to have each 'MOST' quantifier complexly distribute over the formula:

$$(601) \quad [\exists z: Rzx][\exists w: Rwy](Hwz \ \& \ Hzw)$$

The difficulty, however, is that the two 'MOST' quantifiers fail to join up with each other. One generates a disjunction of the form:

$$(602) \quad \bigvee_{\substack{X, X \subseteq \text{EXT}(V), \\ |X| > |\text{EXT}(V) - X|}} [(\lambda x) \exists z: Rzx][\exists w: Rwy](Hwz \ \& \ Hzw)$$

and the other of the form:

$$(603) \quad \bigvee_{\substack{Y, Y \subseteq \text{EXT}(T), \\ |Y| > |\text{EXT}(T) - Y|}} [(\lambda y) \exists z: Rzx][\exists w: Rwy](Hwz \ \& \ Hzw)$$

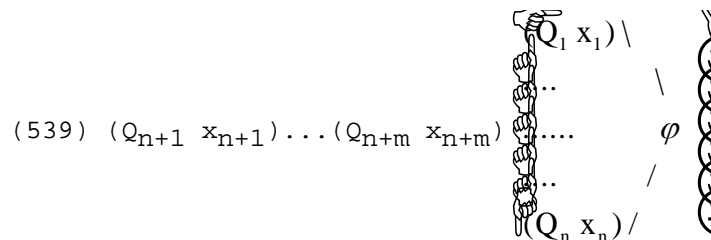
but these two disjunctions are not connected in any way. And they must be connected to get out a sensible reading, since each disjunction on its own contains free the variable  $\lambda$ -extracted in the other disjunction.<sup>424</sup>

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<sup>424</sup>Note that it won't suffice simply to form a double disjunction doubly  $\lambda$ -extracted, since this amounts to making one 'most' quantifier complexly distribute over the other, and gives us a fully linear, rather than a branched, reading.

The underlying difficulty here is that my account in a way only simulates partiality of quantifier order from an underlying linear base. I assume that the syntax of natural language is linear (more properly, tree-like), and that semantic rules work compositionally from the inside out on that syntax. Even where, through simple distribution, quantifiers are wholly unordered, it is a purely semantic lack of ordering. There is still a determinate order of processing fixed by the syntax, it's just that (due to the nature of the distribution) that order makes no difference to the outcome. Thus when we have a sentence like (101) above, there will be some syntactically provided ordering of the quantifiers. One of the two 'most' quantifiers will be in the wide-scope position, and if both are interpreted as distributing complexly, then that one will take semantic wide scope over the other, creating a linear reading.

In general, then, what my account permits is an arbitrary block of distributors, each one interpreted simply or complexly. All those which are interpreted simply effectively take small scope, and those which are interpreted complexly then form a linear prefix on the block of unordered simple distributions. I allow, then, structures which in a branching syntax) would be of the form:



Any branching after the initial branch is forbidden.

Obviously, this is not a wholly desirable outcome. Sentences of the form (599) are not the most elegant constructions around, but I think I can understand them in the way called for by, e.g., Barwise or Sher. The fact that my account cannot capture such readings, then, is decidedly to its detriment. My suspicion, however, is that the defect lies not in the story about distribution, but in the underlying syntax on which that story is imposed. I take it to be one of the advantages of my approach that its exploitation of an independently motivated syntax allows it to place natural limits on the range of branching structures to be permitted, whereas other accounts merely stipulate the range of structures to be interpreted. That we find the limits here placed too conservatively here is, I think, no reason to abandon the methodology. Instead, we should take it as a sign that perhaps the tree-like structure we have assumed for sentences is not the ultimate story. I have earlier (§2.3.3.2) indicated one way in which I would diverge from that story. I do not at the moment see the way to expand our syntactic worldview to remedy the current difficulty, but at least, I think, I know which way we ought to look.

### **§3.3.3 A Brief Note on Polyadic Quantification**

Given the neo-Fregean perspective on quantifiers, according to which they are predicates of predicates, monadic quantifiers (binding a single variable) generalize naturally to polyadic quantifiers (binding multiple variables). Just as the *predicate* 'is red' can satisfy the (second-order) predicate 'is instantiated', thus giving rise to:

(604)  $(\exists x)Rx$

the relation 'is less than' can satisfy the (second-order) predicate 'is each-some instantiated', thus giving rise to:

$$(605) (\forall \exists x, y) L_{xy}^{425}$$

The status of polyadic quantification on my account, however, is more complicated. We now have to address two separate issues: (a) can a single restrictor bind multiple variables, and (b) can a single distributor distribute multiple variables. The first of these questions is discussed tangentially in footnote 275 and §3.2.1.2 above. In short, I find it difficult to see how simultaneous restriction of multiple variables by a single restrictor could be plausible. Imagine we have a proposed such construction, such as:

$$(606) [\text{less-than } x, y]_{x, y} (\text{prime } x \wedge \text{even } y)$$

in which the relational predicate 'less-than' binds both the 'x' and the 'y' (for simplicity, I am considering a case with no subsequent distribution of the bound variables). What sort of semantic value is the relation to pass on to the variables it binds? In keeping with the discussion of relativized restriction given in §3.2.1.2 above, it seems 'x' ought to refer relatively to all those things less than y, for any choice of y, and similarly 'y' ought to refer relatively to all those things greater than x, for any choice of x. But now our relativized reference is ungrounded -- in order to find a determinate reference for 'x', we need first to have a determinate reference for 'y' so that we can relativize x appropriately. But in order to have a determinate

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<sup>425</sup>One can also, of course, have quantifiers which (in neo-Fregean terminology) are (second-order) relations of predicates -- thus giving rise to n-ary quantification, as opposed to polyadic quantification. Thus given the predicates 'is a man' and 'is mortal', we can have the second-order relation 'universally instantiates' to give rise to:

$$(\text{FN } 244) (\forall x)(\text{man } x, \text{mortal } x)$$

See §1.2 for more details.

reference for 'y', we first need a determinate reference for 'x', so that we can relativize y appropriately. The process is never able to get off the ground.<sup>426</sup> I don't intend these considerations against polyadic variable restriction to be completely definitive. I see no way to make the process work, but am not closed to the possibility of someone of greater ingenuity finding the key.

The other type of polyadicity possible under my system involves single distributors acting on multiple variables. Here there seems to be much less conceptual bar to implementing the polyadicity. Just as we can have, say, sentential operators which act on more than one sentence, we might be able to have distributors which act on more than one noun phrase. Thus we might have an ' $\forall\exists$ ' distributor, which, when applied to the two noun phrases in:

(607) John and Albert wrote to Mary and Sarah.

produces the disjunction:

(608) (John and Albert wrote to Mary)  $\vee$  (John and Albert wrote to Sarah)

or an ' $\exists\forall$ ' distributor, which, when applied to (607) produces:

(609) (John wrote to Mary and Sarah)  $\vee$  (Albert wrote to Mary and Sarah)

I intend to leave the possibility of polyadic distributors largely an open issue here, but I want to close this discussion with one source of worry about the potential logicality of such distributors. I am quite

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<sup>426</sup>Note, however, that it is unproblematic to have a single variable restricted by multiple restrictors -- here the semantic value of the variable simply compounds, and the restrictors act as if joined by conjunctions. Thus certain (but not all) types of n-ary (as opposed to polyadic) quantification are possible. In many cases of 'donkey' anaphora, for example, there will be binding of variables by multiple restrictors.

uncertain how much weight ought to be placed on this worry; as I mention above (§3.3.1.2), while it seems likely to me that natural language determiners are characteristically logical (in the permutation-invariance sense of logicality), I place no theoretical importance in my account on such logicality.

[Higginbotham & May 1981] suggests that we should introduce dyadic quantifiers -- binding two variables -- for the analysis of certain multiple-wh question forms and of Bach-Peters type sentences involving crossing co-reference:

(610) Which pilot shot which plane?

(611) The pilot who shot at it hit the Mig that chased him.

They want, furthermore, to extend the notion of logicality to such dyadic quantifiers. In the monadic case, they follow the standard permutation invariance line of logicality, and thus require that a function which is to serve as a quantifier be preserved under automorphisms of the domain.<sup>427</sup> When they turn to binary quantifiers, they need to decide how to extend this condition to functions representing binary quantifiers.

A function representing a binary quantifier must be, in species, a map from the product space of the domain,  $D \times D$ , to the set of true and false, such that the quantifier  $Q$  returns a truth just in case the relation to which it is being applied (considered as a subset of the product space) is such that the quantifier function  $f$  returns a true when applied to it. In order to ensure that the putative quantifier be a

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<sup>427</sup>Higginbotham and May share in the neo-Fregean concept of quantification, but they use the trivial variant of treating quantifier denotations not as sets of predicate denotations but rather as characteristic functions of such sets -- functions from predicate denotations to truth values.



logical one, we want to insist that this function  $f$  be invariant under some type of automorphism of the domain. But what type?

Higginbotham and May have a *prima facie* odd response to this question. They want to allow any  $f$  which is invariant under what they call a 1-automorphism, where:

**(Def. 30)** A 1-automorphism is a mapping  $m: D \times D \rightarrow D \times D$  such that, for any two points  $(a,b)$  and  $(a',b')$  in  $D \times D$ , with  $m(a,b) = (\alpha,\beta)$  and  $m(a',b') = (\alpha',\beta')$  the following condition holds:

$$a = a' \text{ if and only if } \alpha = \alpha'$$

A 1-automorphism, then, is an automorphism which never maps a first coordinate to different first coordinates as the second coordinate changes, and which never maps different domain first coordinates to the same image first coordinate.

The insistence on 1-automorphisms is odd because of the asymmetry of the definition. Why should first coordinates, and not second coordinates, get this kind of protection under quantificational operations? Higginbotham and May realize that this is strange, and have a brief discussion of the matter:

Before proceeding we may comment on a peculiarity of our definition above, namely the bias that it shows toward the first coordinates of relations (there is a dual definition, using second coordinates). The reason for this bias is that it is necessary to encode the 'direction' in which we think of a relation as going. Specifically, if  $m$  is a 1-automorphism, and  $R$  a relation on  $D$ , then the restriction to  $m$  of  $R$ , but not in general its restriction to the converse of  $R$ , will have the property (i). [56]

Higginbotham and May's reasoning here is rather hard to fathom. We don't necessarily think of a relation as 'going in a particular direction'. Of course one needs to keep straight the first and second objects in the

relation, but this isn't to assign a *direction* to the relation. And in any case, nothing about a symmetric automorphism requirement would threaten our ability to keep track of which was the first-positioned object and which the second-positioned.

One would thus suspect that Higginbotham and May intend further to clarify this comment with the subsequent sentence starting 'Specifically,...'. But nothing could be further from the truth. First, the feature of 1-automorphisms they indicate here seems completely irrelevant both to considerations of the 'direction' of the relation and to any plausible motive for preferring the asymmetric 1-automorphism requirement. Second, the claim being made is patently false.

Assume the claim were true. Then we would have to have the following. Pick a 1-automorphism  $m$ . For any relation  $R$  in  $D \times D$ ,  $m|R$  has the property (i), but there is some relation  $R$  such that the converse of  $R$ <sup>428</sup> does not have  $R$ . But the converse of  $R$  -- call it  $R'$  -- is itself a relation on  $D$ , since a relation is just a set of ordered pairs from  $D$ , and we just said that any relation has property (i) hold on the restriction of  $m$  to itself. Contradiction.

Setting aside such broadly philosophical justifications for the bias in favor of 1-automorphisms, Higginbotham and May also have specific examples which they think show that alternative definitions either rule out good quantifiers which the 1-automorphism standard

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<sup>428</sup>It is unclear whether Higginbotham and May mean, by the 'converse' of a relation either:

(FN 245)  $D \times D - R$

or:

(FN 246)  $\{ \langle b, a \rangle \mid \langle a, b \rangle \in R \}$

My objection runs either way.

allows in, or let in bad quantifiers which their definition keeps out. Thus they consider at one point the rather natural looking definition:

**(Def. 31)**  $f$  is a quantifier if it remains invariant under any automorphism of  $D \times D$ .

This definition has the advantage of being exactly parallel to the unary quantifier definition. However, Higginbotham and May draw our attention to the following quantifier 'AE':

(612)  $AE(R) = 1$  iff for every  $a \in D$ ,  $\langle a, b \rangle \in R$  for some  $b \in D$ .

AE thus creates a quantifier which is true of any relation whose domain is all of the things in the model. They now rightly observe that AE is not preserved under general automorphisms of  $D \times D$ <sup>429</sup>, but that it is preserved under 1-automorphisms of  $D \times D$ . They feel further that AE should be a genuine quantifier, since it looks only at the size of the domain of  $R$ , not at the identity of any particular objects.

However, there is an obvious dual to the AE quantifier:

(613)  $EA(R) = 1$  iff for every  $b \in D$ ,  $\langle a, b \rangle \in R$  for some  $a \in D$ .

This is the quantifier which is true when applied to any relation whose *range* is the entirety of the model's domain. There can be no reason for considering AE a genuinely logical quantifier but not EA, but it's quite easy to see that EA is ruled out by the 1-automorphism standard. To see this, consider a domain with three objects,  $a$ ,  $b$ , and  $c$ . Let the relation  $R$  be defined by the following set of ordered pairs:

(614)  $R = \{ \langle a, a \rangle, \langle a, b \rangle, \langle b, c \rangle \}$

$R$  does in fact have a range covering the entire domain, so  $EA(R) = 1$ .

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<sup>429</sup>If, for example, the domain were the positive integers, and all and only ordered pairs of the form  $\langle n, 1 \rangle$  were elements of  $R$ , and we applied an automorphism to  $D \times D$  which mapped  $\langle x, y \rangle$  to  $\langle y, x \rangle$ , then the relation  $R'$  induced by the automorphism would have domain 1, rather than a total domain of the positive integers.

However, we can easily construct a 1-automorphism  $m$  of  $D \times D$  such that  $EA(m(R)) = 0$ . We use the following definition:

$$\begin{aligned} \text{(Def. 32)} \quad m(x,y) &= (x,y) \text{ if } x = a \text{ or } x = c \\ &= (b,c) \text{ if } x=b,y=a, \\ &= (b,b) \text{ if } x=b,y=b, \\ &= (b,a) \text{ if } x=b,y=c \end{aligned}$$

$m$ , then, is the function which keeps all points in  $D \times D$  fixed, except for swapping  $(b,a)$  and  $(b,c)$ . Note first that  $m$  is a 1-automorphism. Things starting with  $a$  are always mapped to things starting with  $a$ , and similarly with  $b$  and  $c$ . However, note that:

$$(615) \quad m(R) = \{(a,a), (a,b), (b,a)\}$$

and that this set does not satisfy EA. Thus EA is, by Higginbotham and May's standards, not a genuinely logical quantifier.

Since I am inclined to agree with Higginbotham and May that AE ought to be a logical quantifier, I agree that generic automorphisms of the product space  $D \times D$  cannot be the standard bearers of logicity. Since EA strikes me as every bit as logical as AE, however, I find their proposal of 1-automorphisms equally unacceptable.<sup>430</sup> We need, it would seem, some even more tightly constrained class of automorphisms. However, it is unclear what that class would be. The most obvious next class to consider is 1,2-automorphisms -- automorphisms which preserve identity and distinctness facts in both the first and the second

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<sup>430</sup>2-automorphisms, defined in the obvious way, will uphold the logicity of EA but reject that of AE, and thus are equally unacceptable.

coordinate.<sup>431</sup> A little thought will confirm that 1,2-automorphisms are the same as the unary induced automorphisms, where:

**(Def. 33)** A unary induced automorphism is a mapping  $m: D \times D \rightarrow D \times D$  such that there is some automorphism  $\mu$  on  $D$  such that  $m(\langle x, y \rangle) = \langle \mu(x), \mu(y) \rangle$ .

Higginbotham and May argue, however, that certain quantifiers are incorrectly judged logical by the standard of unary-induced automorphism invariance. Thus consider the quantifier  $Q^\circ$ :

(616)  $Q^\circ(R) = 1$  iff for all  $x \in D$ ,  $\langle x, x \rangle \in R$ .

$Q^\circ$  thus holds of all universally reflexive relations.  $Q^\circ$  is preserved under unary induced automorphisms, but not under 1- or 2-automorphisms. Higginbotham and May suggest (with, I think, some plausibility) that  $Q^\circ$  ought not be judged wholly logical because it is not completely indifferent to the identity of individuals -- in particular, identity facts between the two relata in the quantified relation matter to the applicability of  $Q^\circ$ .

While these considerations are not wholly definitive, both because not all potential types of permutation invariance have been canvassed and because the logical status of the EA, AE, and  $Q^\circ$  quantifiers is not incontrovertibly clear, I am inclined to suspect that there is no unproblematic standard for logicality of polyadic quantifiers. If that is true, I am further inclined to take it as a reason for suspicion of the very project of defining polyadic distributors.

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<sup>431</sup>The notion of logicality induced through application of such automorphisms is, in fact, the standard adopted by, e.g., [Van Bentham 1989] and [Sher 1991].

### **§3.4 Anaphoric Binding and Compositionality**

According to the anaphoric account, quantification is a non-compositional affair. One cannot proceed straightforwardly 'from the inside out' in determining the meaning of a whole sentence; the restricting process requires that at times one also work from the outside in. In this last section, I want to consider some implications of this noncompositionality. I begin by discussing exactly what sort of constraint the notion of compositionality provides, using as a springboard the recent claim by [Zadrozny 1995] that compositionality is a formally empty principle. I draw from this discussion some methodological lessons which must be heeded if compositionality is to be of any use. I then suggest that standard Tarskian semantics for quantified languages do not heed these lessons, and that, properly construed, Tarskian quantification is no more compositional than mine. I close by considering why compositionality might be important to us and showing that all the available reasons judge the particular noncompositionality of my account equally acceptable.

#### **§3.4.1 Compositionality as a Methodological Constraint**

A compositional meaning theory is, very roughly, a meaning theory which shows how the meanings of wholes depend upon the meanings of component parts. Compositionality is, for various reasons, often taken to be a desirable trait in a meaning theory, and, moreover, a trait which is not always easy to come by. Considerable ink has been spilled in debating whether certain features of English, or other natural languages, obviate the possibility of a compositional semantics for the language.

### §3.4.1.1 A Challenge to Compositionality

Recently it has been suggested that this ink has been spilled in vain.

In his 'From Compositional to Systematic Semantics', Wlodek Zadrozny proves that, given any set  $S$  of lexical strings and any meaning function  $M$  on  $S$ , we can construct a new meaning function  $\mu$  which matches the original  $M$  but which is compositional. That is, we will have:

$$\mu(s.t) = \mu(s)(\mu(t))$$

and:

$$\mu(s)(s) = M(s)^{432}$$

where '.' is the concatenation operation in the syntax. Zadrozny proves this result by observing that the set of equations:

$$\mu(s) = \{ \langle s, M(s) \rangle \} \cup \{ \langle \mu(t), \mu(s.t) \rangle, t \in S \}$$

for all  $s$  in  $S$  always has a solution, and thus by constructing a meaning function  $\mu$  through what he calls 'an extreme example of defining a function by cases' [333].<sup>433,434,435</sup>

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<sup>432</sup>Alarm bells ought to go off at this point. Why does Zadrozny impose the requirement that  $\mu(s)(s) = M(s)$ , rather than the more natural requirement that  $\mu(s) = M(s)$ ? He claims that:

Since elements of  $M$  can be of any type, we do not automatically have  $m(s.t) = m(s) \# m(t)$ , where  $\#$  is some operation on the meanings. To get that kind of homomorphism, we have to perform a type raising operation that would map elements of  $S$  into functions and then the functions into the required meanings. ... The meaning function  $\mu$  that we want to define will provide compositional semantics for  $S$  by mapping it into a set of functions in such a way that  $\mu(s.t) = \mu(s)(\mu(t))$ , for all elements  $s, t$  of  $S$ . [330]

The requisite type raising, then, force us to take as meanings in our new semantics functions from terms to the meanings of the old semantics, and thus forces the new semantics to match the old by assigning a function which itself assigns the right meaning to the term in question. Given this shift in the kind of thing we are taking as semantic value, one might wonder whether Zadrozny is engaged in at all the same project as the traditional semanticist. One can take the subsequent technical discussion of this paper, especially the distinction made between occult and manifest meanings and the illustration of Zadrozny's reliance on occult meanings, as an attempt to provide firm formal support for this initial skepticism.

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<sup>433</sup>Zadrozny proves his theorem using the resources of AFA, a set theory which denies the Foundation axiom of ZFC and thus allows sets containing themselves as members. AFA proves the Solution Lemma which Zadrozny uses in the proof of his Theorem 1. While Zadrozny notes that AFA and ZFC are equiconsistent, it's not clear why this equiconsistency should justify his use of this nonstandard set theory. The two set theories decidedly do not prove the same theorems (as is obvious from the fact that the Foundation axiom is a theorem of ZFC but not of AFA). In particular, ZFC does *not* prove Zadrozny's Theorem 1. If our grammar has any strings  $s$  and  $t$  which can be concatenated in either order (i.e., both  $s.t$  and  $t.s$  are grammatical), we will need  $\mu$  such that:

$$\mu(s)(\mu(t)) = \mu(s.t)$$

$$\mu(t)(\mu(s)) = \mu(t.s)$$

Considering  $\mu(s)$  as a set of ordered pairs, we see that it must contain some ordered pair with  $\mu(t)$  in the first position. Similarly,  $\mu(t)$  must contain some ordered pair with  $\mu(s)$  in the first position. Thus  $\mu(s)$  must contain, buried two levels down,  $\mu(s)$  itself -- which violates the Foundation axiom.

The question for Zadrozny thus becomes whether AFA is an appropriate theory in which to formulate one's semantic theories. I do not take this question on here, in part because, as I show below, there is a simple method for obtaining something sufficiently similar to Zadrozny's result using only the resources of ZFC.

<sup>434</sup>Note that Zadrozny's Theorem 1 also suffices to provide a meaning function which is *strongly compositional* in the sense of [Larson & Segal 1995]:

**Strong Compositionality:**  $R$  is a possible semantic rule for a human natural language only if  $R$  is strictly local and purely interpretive. [79]

A rule is strictly local if it 'interprets a syntactic node  $[X Y_1 \dots Y_n]$  ... in terms of its immediate subconstituents  $Y_1, \dots, Y_n$ '; it is purely interpretive if it 'interprets only structure given by the syntax'. Clearly a theorem which yields a meaning function meeting the condition:

$$\mu(s.t) = \mu(s)(\mu(t))$$

where '.' is a concatenation relation given by the syntax, yields a strongly compositional meaning function.

<sup>435</sup>Zadrozny's proof of his Theorem 1, as it is given in the text of his paper, is not adequate to establish his result. He claims that "clearly,  $\mu$  is a function, because it is a collection of ordered pairs" [332]. But of course merely being a collection of ordered pairs does not suffice to show that  $\mu$  is a function. While this stronger fact about the form of  $\mu$  is easily read off of the construction of  $\mu$  as well, a further worry lurks. We need to know not just that  $\mu$  is a function, but also that  $\mu(s)$  is a function for every  $s \in S$ . Without such assurance, the construction given for  $\mu$  is ill-formed. But why should we believe that each  $\mu(s)$  has such a property? In order for the claim to hold, we need to know that no  $\mu(s)$  contains two ordered pairs with the same first element and differing second elements. But since  $\mu(s)$  will in general contain ordered pairs of the form:

$$\langle \mu(t), \mu(s.t) \rangle$$

for every  $t \in S$ , what we need to know is that there are no  $t_1, t_2$  such that:

$$\mu(t_1) = \mu(t_2)$$



### §3.4.1.2 A Challenge to a Challenge

I found Zadrozny's result somewhat surprising -- especially since I took myself to have a reasonably straightforward *proof* that not all semantics can be made compositional. If compositionality is, as Zadrozny asserts, "the property that the meaning of the whole is a function of the meaning of its parts" (329), then the following is certainly a necessary condition for a meaning theory to be compositional:

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$$\mu(s.t_1) \neq \mu(s.t_2)$$

Is there any good reason to think that there will in fact be no such  $t_1, t_2$ ?

As it turns out, there is (although neither the worry nor the corresponding reason are mentioned by Zadrozny). In general, given distinct  $t_1, t_2$  we can never have:

$$\mu(t_1) = \mu(t_2)$$

This is because  $\mu(t_1)$  contains an ordered pair of the form  $\langle t_1, m(t_1) \rangle$  while  $\mu(t_2)$  contains an ordered pair of the form  $\langle t_2, m(t_2) \rangle$ . Since  $t_1$  and  $t_2$  are distinct, these two ordered pairs are distinct. Furthermore,  $\langle t_1, m(t_1) \rangle$  cannot appear in  $\mu(t_2)$ , and  $\langle t_2, m(t_2) \rangle$  cannot appear in  $\mu(t_1)$ . This is a consequence of the fact that the construction of  $\mu$  is such that given any  $s \in S$ , there is one and only one ordered pair in  $\mu(s)$  such that an actual lexical item (rather than  $\mu$  applied to some lexical item) appears as the first element of that ordered pair.

However, while Theorem 1 stands as stated, matters are less clear when we come to Zadrozny's Proposition 3. In the construction used in the proof of Proposition 3, we solve the set of equations of the form:

$$\mu(s) = \{ \langle \$, m(s) \rangle \} \cup \{ \langle \mu(t), \mu(s.t) \rangle : t \in S \}$$

Here the ordered pair  $\langle s, m(s) \rangle$ , which previously served to guarantee the uniqueness of  $\mu(s)$  from  $\mu(t)$  for other  $t$ , has been replaced by the ordered pair  $\langle \$, m(s) \rangle$ , which will be common to  $\mu(t)$  for all  $t$ . There is thus no longer any reason to think that  $\mu(t_1)$  and  $\mu(t_2)$  will in general be distinct for distinct  $t_1, t_2$ , and if there is no reason to believe there will be such distinctness, there is no longer reason to think that  $\mu(s)$  will be a function. In particular, if we take a simple grammar whose only lexical items are  $s$  and  $t$ , composable in either order, and assume that  $m(s) = m(t) = m(s.t) = m(t.s) = \dots$ , we will find that  $\mu(s) = \mu(t)$  and thus that the construction is ill-founded.

While the proof of Proposition 3 thus founders, we can fix it easily by changing slightly the set of equations to be solved for  $\mu$ . We simply add an 'identifier' tag to each  $\mu(s)$ :

$$\mu(s) = \{ \langle s, s \rangle \} \cup \{ \langle \$, m(s) \rangle \} \cup \{ \langle \mu(t), \mu(s.t) \rangle : t \in S \}$$

I leave it an open question to what extent such an ad hoc solution to the technical difficulty undermines the degree to which Proposition 3 ought to interest those concerned with the methodological force of compositionality. (I am particularly grateful to Theo Janssen for discussion on the subject of this note.)

**N-Comp:** A meaning theory  $M$  is compositional only if, given any  $s, t$  in  $S$  such that  $M(s) = M(t)$ , if  $u$  and  $v$  differ only in that some occurrences of  $s$  have been replaced with occurrences of  $t$ , then  $M(u) = M(v)$ <sup>436,437</sup>

If the meaning of the whole is a function of the meanings of the parts, then there can be no change in the meaning of the whole where there is no change in the meanings of the parts.

However, it's well-known that we can construct languages which fail to obey N-Comp in their semantics. One such language is one which perversely combines the Fregean notion that propositional attitude contexts fail to respect the principle of substitution of coreferential terms with the direct reference notion that names carry referents as their sole semantic content. Of course, no serious semantic theory *would* combine these two notions, precisely because (a) the resulting

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<sup>436</sup>A fully adequate statement of the N-Comp condition will be more complicated than that given in the text. Once we start to consider semantic phenomena such as intensionality and context sensitivity, we may well be forced to replace the simple idea of a single meaning for each lexical item with a more complex system assigning levels of meaning, such as one embodying Frege's sense/reference distinction or Kaplan's character/content distinction. If we do move to multiple levels of meaning, we will have to decide on which levels of meaning the N-Comp constraint is to be imposed in order to yield compositionality (in the case of Kaplan, presumably on the level of content, and in the case of Frege explicitly, although problematically, on the levels of both sense and reference). Since the main thrust of this paper is independent of these concerns, I speak from here on under the fiction that there is a single level of meaning to which N-Comp can unproblematically be applied.

<sup>437</sup>The condition N-Comp is a well-known consequence of compositionality and certainly is not unique to me. It is, for example, essentially this condition which drives [Frege 1892] to conclude that coreferential proper names must have differing senses. Note that N-Comp, as a direct consequence of compositionality, is a property held by all compositional meaning theories. The restriction to meaning functions satisfying N-Comp is thus *not*, as is Zdrozny's proposed restriction to systematic meaning functions, an attempt at further narrowing of the core notion of compositionality (it is in fact a trivial consequence of Zdrozny's own formal definition in terms of type-lifted syntax-semantics homomorphisms).

combination is noncompositional and (b) compositionality is considered a desideratum for a good semantic theory. I don't intend here to endorse this meaning theory for the attitudes -- merely to observe that it is a *possible* theory and a noncompositional one.<sup>438</sup>

We would then have a meaning function M for (a fragment of) English which is such that:

(617) Albert believes that Hesperus is a planet.

(618) Albert believes that Phosphorus is a planet.

are assigned different meanings by M (perhaps 'true' for (617) and 'false' for (618))<sup>439</sup>, but which is also such that:

$$M('Hesperus') = M('Phosphorus') = \text{Venus}$$

Clearly this meaning function M does not obey the necessary condition N-Comp for being compositional. Moreover, it should be obvious that, *contra* Zadrozny, there can be no 'compositional semantics ... which agrees with the function M' [332]. No matter what new meaning function  $\mu$  we devise, if it agrees with M to the extent that:

$$\mu('Hesperus') = \mu('Phosphorus')$$

and:

$$\mu((1)) \neq \mu((2))$$

then it cannot meet N-Comp and thus cannot be compositional. Again, the point here is not that natural language actually is noncompositional, but just that not every analysis of every semantic phenomenon can be made compositional.

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<sup>438</sup>Nor, of course, does the existence of one noncompositional meaning function for attitude contexts mean that there is no other compositional theory for the same constructions -- although any such compositional theory will of necessity differ from at least some of the assignments made by the noncompositional function given in the main text.

<sup>439</sup>Although nothing in the example depends on taking truth values as the meanings of sentences.

### §3.4.1.3 Some Belated Preliminaries

Zadrozny, then, has a proof that all semantics can be made compositional, and I have one that some semantics cannot be made compositional. Something is clearly amiss. In order to see what, we need to step back a bit and state clearly what we're trying to do. Suppose we have a grammar  $G$  which generates a set  $S$  of well-formed expressions (the maximal of which will be 'sentences'). It's no task simply to create a compositional meaning function on  $S$ . We could, for example, map every element in  $S$  to the same meaning (perhaps to the value 'true'). Then there would be a simple function giving the meaning of the whole from the meanings of the parts -- the identity function. In order to provide an interesting result, we must show that there is always a compositional meaning function *which meets certain constraints*.

One rough constraint is that the compositional meaning function respect our independent judgments about the meanings of the parts -- whatever source those judgments may have. Of course, our naive intuitions about meaning may be insufficiently well-formed to support, on their own, a theory of meaning, so it seems acceptable that there be some regimentation of semantic values as we move from intuition to theory. Nevertheless, our starting intuitions must be recognizable in the final result. In particular, I want to insist that the following synonymy constraint be met:

**Synonymy:** An interesting compositional meaning theory must be such that it assigns the same semantic value to any two

terms which we took as synonymous prior to the theory construction.<sup>440</sup>

I don't intend that this be a sufficient condition for isolating the interesting compositional meaning theories, but it will suffice for our current purposes.

#### §3.4.1.4 The Occult, and its Omnipotence

Let's return to the Zadrozny result. Given a meaning function  $M$ , call that function derived from  $M$  using the methods Zadrozny sets out in his proof of Theorem 1 the  $z$ -function for  $M$ . Simplifying the attitude context case set out above, assume we have a language which has three lexical items: the names 'h' and 'p' and the predicate 'B' (interpreted as 'is such that Albert believes it to be a planet'<sup>441</sup>). Assume also that we start with a meaning function  $M$  for this language which yields the following result:

$$M('h') = M('p') = \text{Venus}$$

$$M('B') = \text{Albert}^{442}$$

$$M('hB') = \text{true}$$

$$M('pB') = \text{false}$$

Zadrozny shows us that we can obtain a  $z$ -function for  $M$  by solving the following system of equations:

$$\mu('h') = \{ \langle 'h', \text{Venus} \rangle, \langle \mu('B'), \mu('hB') \rangle \}$$

$$\mu('p') = \{ \langle 'p', \text{Venus} \rangle, \langle \mu('B'), \mu('pB') \rangle \}$$

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<sup>440</sup>The same cautionary remarks made regarding the N-Comp constraint (see footnote 435) apply to the interpretation of the Synonymy constraint.

<sup>441</sup>Where, despite the topicalization, the attitude report is intended to be read as *de dicto*.

<sup>442</sup>Obviously, this is not a plausible semantic value for  $B$ . I use a (short) implausible value simply because the value of  $B$  does not feature in the subsequent discussion. Replace mentally with your favorite value for predicates if you prefer.

$$\mu('B') = \{\langle 'B', \text{Albert} \rangle\}$$

$$\mu('hB') = \{\langle 'hB', \text{true} \rangle\}$$

$$\mu('pB') = \{\langle 'pB', \text{false} \rangle\}$$

Performing the appropriate substitutions, we obtain:

$$\mu('h') = \{\langle 'h', \text{Venus} \rangle, \langle \langle 'B', \text{Albert} \rangle, \langle 'hB', \text{true} \rangle \rangle\}$$

$$\mu('p') = \{\langle 'p', \text{Venus} \rangle, \langle \langle 'B', \text{Albert} \rangle, \langle 'pB', \text{false} \rangle \rangle\}$$

$$\mu('B') = \{\langle 'B', \text{Albert} \rangle\}$$

$$\mu('hB') = \{\langle 'hB', \text{true} \rangle\}$$

$$\mu('pB') = \{\langle 'pB', \text{false} \rangle\}$$

$\mu$  is indeed compositional. We have the desired results:

$$\mu('pB') = \mu('p')(\mu('B'))$$

$$\mu('hB') = \mu('h')(\mu('B'))$$

Moreover,  $\mu$  meets the necessary condition N-Comp. However, note how it is able to do so. It avoids the earlier problems with N-Comp precisely because it denies that 'h' and 'p' have the same meaning. That is, it violates the Synonymy constraint, and thus fails to provide an *interesting* compositional semantics for our language.

How can this be? Hasn't Zadrozny promised that we will obtain a compositional meaning function which "agrees with" [330] our original (non-compositional) meaning function in what it assigns to strings? Let's take a closer look at what Zadrozny's result actually delivers. What we are guaranteed by Theorem 1 is, given a meaning function  $M$ , a compositional Z-function  $\mu$  for  $M$  which is such that:

$$(*) \mu(s)(s) = M(s)$$

It is (\*) which implements the requirement that the new compositional meaning function agree with the old non-compositional meaning

function.<sup>443</sup> Let's say that a meaning function  $\mu$  z-matches M just in case (\*) holds.

Now (\*) does hold in the above case. We have  $\mu('p') = \{ \langle 'p', Venus \rangle, \langle \langle 'B', Albert \rangle, \langle 'pB', true \rangle \rangle \}$ , so  $\mu('p')('p') = Venus = M('p')$ . Similarly we have  $\mu('h') = \{ \langle 'h', Venus \rangle, \langle \langle 'B', Albert \rangle, \langle 'hB', true \rangle \rangle \}$ , so  $\mu('h')('h') = Venus = M('h')$ . Nevertheless, we do not have  $\mu('p') = \mu('h')$ . I thus want to distinguish two components of meaning, which I will call *manifest* and *occult* meaning, provided by the z-matching  $\mu$  function Zadrozny shows us how to construct:

- **Manifest Meaning:** the portion of  $\mu$  constrained by the original meaning function M, which we are to think of as showing what the term 'really' means, and which is accessed by considering  $\mu(s)(s)$  for any s.
- **Occult Meaning:** those ordered pairs in the value assigned to s which do not have s as their first component and which thus are not constrained by the z-matching requirement; the occult meanings tell how the term acts when combined with any other terms in the language.

Once we make this distinction, we can see that the occult is all-powerful -- the occult meanings in Zadrozny's system are doing all the work. Consider an arbitrary string:

$$s = (s_1.(s_2.(... (s_n.s_{n+1})))...)^{444}$$

and think about how, using a z-function  $\mu$  for the intuitive meaning function M, we would go about calculating the value of s. We will have:

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<sup>443</sup>Without some such requirement, of course, Zadrozny's result would be no more interesting than the trivial result observed earlier that any grammar can be given a compositional meaning theory by uniformly assigning some arbitrarily chosen meaning to all strings.

<sup>444</sup>I assume here without loss of generality that we have a right-branching tree.

$$\mu(s_n \cdot s_{n+1}) = \mu(s_n)(\mu(s_{n+1}))$$

Note here that we appeal to part of the occult meaning of  $s_n$ : we use the ordered pair:

$$\langle \mu(s_{n+1}), \mu(s_n \cdot s_{n+1}) \rangle$$

which appears in the extension of  $\mu(s_n)$ . This occult meaning, of course, is unconstrained by the z-matching condition and thus by the nature of M. Having calculated the value of  $\mu(s_n \cdot s_{n+1})$ , we go on to calculate:

$$\mu(s_{n-1} \cdot (s_n \cdot s_{n+1})) = \mu(s_{n-1})(\mu(s_n \cdot s_{n+1}))$$

Again, we appeal only to the occult meaning of  $s_{n-1}$ . The same holds all through the final calculation of the meaning of  $s$ . At every stage, the manifest meanings of the terms are inert, while the occult meanings fully determine the value at the next stage.

What this shows is that the manifest meanings are purely epiphenomenal. We could change every manifest meaning in the system, and, while losing formal compositionality, we would not alter in the slightest the computational procedure (or its result) through which we determine the semantic value of wholes from their parts. Since only the manifest meanings of terms are constrained by the original meaning function, we see that the z-matching condition provides only the weakest of connections with our original intuitions on meaning. It is no wonder, were we willing to give up so much, that we could obtain compositionality in return.

#### **§3.4.1.5 A Strengthened Compositionality Result**

In fact, if we do make this sacrifice -- if we do accept z-matching as the appropriate level of respect for our starting semantic intuitions and thus hand compositional meaning over to the occult, we can get more



than compositionality in return. We can even get the systematicity that Zadrozny sees as the way out of the 'somewhat disturbing' result he adduces.

Zadrozny suggests that 'by requiring that the meaning function belong to a certain class' [335] we can avoid the ubiquitous compositionality of his Theorem 1. As an example, he gives the following grammar:

**Grammar DN:**

$$N \rightarrow DN$$

$$N \rightarrow D$$

$$D \rightarrow 0|1|2|3|4|5|6|7|8|9$$

This is the grammar which assigns the counterintuitive left-branching tree structure to numerals, in which the ones-place digit has scope over the tens-place digit, the tens-place digit scope over the hundreds-place digit, and so on. Zadrozny then proves the following theorem:

**Theorem 6:** There is no polynomial  $p$  in two variables  $x,y$  such that:

$$\mu(DN) = p(\mu(D), \mu(N))$$

and such that the value of  $\mu(DN)$  is the number expressed by the string  $DN$  in base 10.

Theorem 6 is thus intended to show that systematicity -- here realized in the requirement that the meaning function be a polynomial -- provides constraints which cannot be provided by mere compositionality. But the theorem makes a crucial illicit move. What Zadrozny requires here is not just that the new meaning function  $\mu$  z-match the intuitive meaning

function<sup>445</sup>, but that in fact  $\mu(\text{DN})$  just be the intuitive meaning of DN. Zadrozny has thus, in this example, abandoned the distinction between manifest and occult meaning, adopting a matching condition stricter than that of z-matching, one which requires *all* meaning to be manifest. Were he to remain consistent with his earlier reasoning, all he would require of  $\mu$  is that:

$$\mu(\text{DN})(\text{DN}) = \text{the number expressed by the string DN in base 10}$$

thus allowing for an additional layer of occult meaning to the term 'DN'.

Two points are important here. First, had Zadrozny required so strict a matching condition when considering the introduction of (non-systematic) compositional meaning functions, we would not have been able to prove his Theorem 1 (as we saw from the failure of the attitude-context example to meet the N-Comp requirement). Thus he has not shown that systematic semantics places any more constraint on the theorist than mere compositional semantics. Second, if we do weaken the requirement for a successful systematic semantics to mere z-matching of the starting meaning function, then we can easily provide a systematic z-function for DN.

In order to do so, let  $M$  be the (non-systematic) function which assigns to each string in DN the appropriate number. Now let  $D_1, \dots, D_n, \dots$  be a list of all the grammatical strings of DN, and define  $\mu$  as follows:

$$\mu(d) = \{ \langle D_1, M(D_1) \rangle, \langle D_2, M(D_2) \rangle, \dots, \langle D_n, M(D_n) \rangle, \dots \}$$

for all  $d$  in DN. Clearly this  $\mu$  z-matches ML for any  $d$  in DN, we have:

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<sup>445</sup>That is, the meaning function which assigns to each DN 'the number expressed by the string DN in base 10'.

$$\mu(d)(d) = M(d)$$

Moreover, it's obvious that there is some polynomial  $p$  such that:

$$\mu(DN) = p(\mu(D), \mu(N))$$

Since all terms have the same semantic value (although not the same manifest value) under this system, we can take the polynomial 'x' (or 'y') to provide our systematic composition method.

In fact, this general method can be employed to obtain Zadrozny's own results much more simply than he does. Given a grammar  $G$  generating strings  $S_1, \dots, S_n, \dots$  and a meaning function  $M$  for those strings, we define a new function  $\mu^*$  as follows:

$$\mu^*(s) = \{ \langle S_1, M(S_1) \rangle, \dots, \langle S_n, M(S_n) \rangle, \dots \}$$

for all  $s$  generated by  $G$ .  $\mu^*$  is compositional in the most trivial sense: the meaning of the whole is simply identical to the meanings of the parts.<sup>446,447</sup> Similarly, it is systematic in the most trivial sense.

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<sup>446</sup>Note, however, that the  $\mu^*$  thus generated is not compositional in Zadrozny's strict sense -- it is not such that  $\mu(s.t) = \mu(s)(\mu(t))$ . This is to be expected, since a  $\mu^*$  of the form I set out here can always be obtained using only the resources of ZFC, whereas a  $\mu$  meeting the Zadrozny compositionality constraint is, as we saw above, frequently incompatible with the Foundation axiom.

My  $\mu^*$  does, however, respect the more general conception of compositionality (endorsed by Zadrozny) of "a functional dependence of the meaning of an expression on the meanings of its parts" [329-330]. My failure to meet the particular Zadrozny construal of compositionality, then, can only be grounds for objection should we have some reason to favor his construal over others. The only possible such ground I see is that Zadrozny's technical notion of compositionality is intended to spell out the idea that compositionality requires "the existence of a homomorphism from syntax to semantics" [329].

The introduction of homomorphisms into the discussion raises complications. In its strictest use, a homomorphism is a mapping  $f$  from one group  $G_1$  (with group operation  $\bullet_1$ ) to a second group  $G_2$  (with group operation  $\bullet_2$ ) such that:

$$f(x \bullet_1 y) = f(x) \bullet_2 f(y)$$

However, neither syntax nor semantics obviously have a group structure (note, for example, that the concatenation operation in syntax is not associative). Thus we will (following [Janssen 1986, 1997]) need to rest with the weaker approximation of a homomorphism as a mapping which preserves structure, in the sense that:

$$f(x \otimes y) = f(x) \bullet f(y)$$

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where  $\otimes$  is some operation on the domain space and  $\bullet$  is some operation on the range space. Our particular problem, then, lies in locating the relevant operations whose structure we wish to preserve. In the domain space of syntax, the operation of concatenation is perhaps an obvious candidate. However, when we turn to semantics things are less clear. What general operation on semantic values do we want respected by the compositional homomorphism?

Zadrozny's method of obtaining compositionality takes functional application to be the relevant operation to be preserved; mine takes set union to be the favored structure. I see no grounds for preferring either of the two. In fact, I see considerable grounds for skepticism that the notion of a general operation on n-tuples of semantic values is a useful or even sensible one. If, for example, semantic values in natural language run the gamut from objects to events to properties to intensions to truth values to propositions to truth functions, why should we think that there is any global operation to be performed on those semantic values, let alone an operation whose structure mirrors the structure of syntactic concatenation? In any case, at least some authors (e.g., Davidson) want both to reject the very notion of semantic objects and to endorse compositionality as a methodological principle. It would be unfortunate if an insistence on compositionality as a syntax-semantics homomorphism were to reduce the position of such authors to incoherence. On the strength of such considerations, I am inclined to take the identification of compositionality with the existence of an appropriate homomorphism more as an enlightening metaphor and less as a working definition.

I will thus continue to treat compositionality as a functional dependence of the meanings of wholes on the meanings of parts, leaving open the exact type of functional dependence. In subsequent discussion, I will frequently make reference to a compositionality principle for a particular meaning function. That principle is intended to show *how*, in that particular case, the meaning of the whole depends on the meanings of the parts. I make no attempt to say which compositionality principles are the right ones (or even if there is a right principle).

Thus, for example, one compositionality principle for the meaning function  $\mu^*$  described above in the main text is:

$$\mu^*(s.t) = \mu^*(s) = \mu^*(t)$$

The meaning of the whole, that is, is determined by the meanings of the parts in the particularly strict sense of simply being the meanings of the parts.

<sup>447</sup>In addition, the analog to Zadrozny's Proposition 4:

If the set of expressions  $S$  and the original meaning function  $m(x)$  are computable, then so is the meaning function  $\mu(x)$ .

holds for the compositional meaning function  $\mu^*$  I describe above. Clearly if we have "a Turing machine,  $T_1$ , that prints all elements of  $S$ , and another Turing machine,  $T_2$ , that takes an element  $s$  on the output tape of  $T_1$  as input and produces as output  $m(s)$ " [334], we can construct a third Turing machine which will construct the set of the successive ordered pairs of outputs of  $T_1$  and  $T_2$  -- which is just what is required as the semantic value of every term in  $S$ .

Similarly, if, for the original meaning function  $M$ , there was some finitely axiomatizable theory  $T$  proving every sentence of the form:

$$M(s) = x$$

where 's' is replaced with a (name of a) grammatical element of  $S$  and 'x' is replaced with (a name of) the appropriate semantic value of  $s$ , then that same finite theory, in conjunction with the axiom:

### §3.4.2 Test Cases in Compositional Semantics

One certain lesson of the above is that systematicity is *not* the route to real constraints on the range of available meaning theories. What we need instead are constraints on what meanings are assigned to component parts. Without such constraints, both compositionality and systematicity are always available. With such constraints, even the weaker constraint of compositionality places considerable restrictions on what semantics are at our disposal.

If we want compositionality to be a methodological principle with content, then we must impose enough constraints on meaning that we avoid the sorts of dodges -- through 'occult' meanings and the like -- that I sketch above. The semanticist who would make use of compositionality, then, must shoulder the additional burden of motivating and obeying substantive requirements on the meanings of lexical items.

#### §3.4.2.1 Compositionality and the Context Principle

How heavy exactly is that burden? A full answer to that question exceeds the bounds of this work, but I want to examine some cases in order to give a partial answer. First, consider the position of the semanticist who, for whatever reason, feels that the semantic properties of whole sentences enjoy some sort of conceptual priority over those of component lexical items, and thus that the semantic properties of subsentential parts are mere theory-internal constructs, which have no obligation to

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$(\forall t)(\forall s)(\exists!x)(x = \langle s, M(s) \rangle \ \& \ x \in \mu^*(t))$   
will entail every sentence of the form:

$$\mu^*(t)(s) = M(s)$$

where 's' and 'x' are as above 't' is an arbitrary element of S.

Note in addition that Zadrozny's proof of his Proposition 4 for his  $\mu$  function is not obviously successful./

the data.<sup>448</sup> To what extent can such a person, in light of the results discussed above, use compositionality as a real constraint on theory formation?

The situation we face here is not quite that which I exploit above. Here, instead of having a pre-theoretical position that constraints the manifest semantic value of all terms and the occult values of none, we have a position which fully determines the semantic values of some terms (the sentences) and says nothing about others. Assume that  $S_1, \dots, S_n, \dots$  list all of the sentences in the language, and that the function  $M$  codifies the pre-theoretic assignment of semantic

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<sup>448</sup>Beyond, of course, their obligation to contribute to the construction of the correct semantic value for the sentence containing them. The paradigm case of the philosophical position I sketch here is Frege; see for example [Frege 1884]:

In the enquiry that follows, I have kept to three fundamental principles: ... never to ask for the meaning of a word in isolation, but only in the context of a proposition. [X]

More recently, Davidson has endorsed the idea that the data for the semantic theorist is on the level of complete sentences:

Theories of another kind [from his] start by trying to connect words rather than sentences with non-linguistic facts. This is promising because words are finite in number while sentences are not, and yet each sentence is no more than a concatenation of words. ... But such theories fail to reach the evidence, for it seems clear that the semantic features of words cannot be explained directly on the basis of non-linguistic phenomena. The reason is simple. The phenomena to which we must turn are the extra-linguistic interests and activities that language serves, and these are served by words only in so far as the words are incorporated in (or on occasion happen to be) sentences. [Davidson 1984a, 127]

or, more succinctly,

Words have no function save as they play a role in sentences; their semantic features are abstracted from the semantic features of sentences. [Davidson 1984b, 221]

Davidson holds that the publicly observable data forces onto the semantic theorist a set of T-sentence constraints of the form:

$s$  is true iff  $p$

thus dictating the semantic value of sentences (although this language is somewhat misleading in Davidson's case), but also holds that the corresponding R-sentences of the form:

$t$  refers to  $x$

have no empirical content, and are to be endorsed or rejected purely on the grounds of how well they advance the theory construction.

values to these sentences. How free are we to write a compositional semantics for this grammar which agrees (fully) with M on  $S_1, S_2, \dots$ ?

We can no longer use the move made above, in which each term in the language is given enough semantic content to allow it to determine the meaning of any meaningful string, and in which compositionality is reduced to sameness of meaning between part and whole. If we use occult meanings at the lower levels, we need to flush them out as we reach the sentential level. So long as the syntax of the language is reasonably well-behaved, however, we can always perform this flushing, and construct a compositional  $\mu$  which exactly matches M on the sentential level.<sup>449,450</sup>

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<sup>449</sup>Two methods for performing this flushing: the brute-force method is to devise a function SENT which is true of a string just in case that string is a sentence, and then define the compositional nature of  $\mu$  as follows:

$$\begin{aligned}\mu(s.t) &= \mu(s) \text{ if } \text{SENT}(s.t) = \text{false} \\ &= \mu(s)(s.t) \text{ if } \text{SENT}(s.t) = \text{true}\end{aligned}$$

Somewhat more elegantly, we can define the meaning function so that it, at each level in composition, 'dumps' all (now) unnecessary meaning. The composition principle would then look like:

$$\mu(s.t) = \{ \langle x, y \rangle \mid \langle x, y \rangle \in \mu(s) \ \& \ (\exists z)((s.t).z=x \vee z.(s.t)=x) \}$$

Once we reach the sentential level, then, all but the meaning of the sentence (according to M) will have been discarded (although see the next footnote for a more careful discussion of this point).

<sup>450</sup>The one caveat to this claim regards sentence which themselves contain sentences as subcomponents. If our philosophical concerns require us to assign meaning  $M_1$  to sentence  $S_1$  and meaning  $M_2$  to sentence  $S_2$ , and if our syntax recognizes another sentence of the syntactic form:

$$C(S_1, S_2)$$

for some sentential connective C, then our options for removing certain types of noncompositionality are extremely limited. If, for example, there is another sentence  $S_4$  which also has meaning  $M_1$ , and which is such that:

$$M(C(S_1, S_2)) \neq M(C(S_4, S_2))$$

(thus violating the condition N-Comp), then we will be unable to construct a new meaning function  $\mu$  which agrees with M on the sentential level but which is compositional, no matter how much occult meaning we build into the connective C. We might attempt to avoid this problem by treating  $S_1$  etc. *qua constituent of  $S_3$*  not as a sentence subject to pre-theoretic meaning constraints but as a freely assignable subsentential component.

Constraining the theory only on the sentential level, then, does not allow for the use of compositionality as a strong constraint on theory formation. Now consider what happens if we take, instead, pre-theoretic constraints to determine what values the meaning function must assign to the atomic lexical items. Even if we allow the introduction of occult meanings on the higher syntactic levels, so long as we have pre-theoretic constraints on the manifest meanings of those levels we will not always be able to obtain compositional meanings functions -- our impoverished starting point will make it impossible to build up the occult meanings necessary to work around any *prima facie* noncompositional obstacles.<sup>451</sup> But if we loosen our pre-theoretic constraints even slightly, by allowing even one atomic term in the grammar to carry occult meaning, then compositionality loses force in any sentence in which that term appears -- since it can then infest all higher levels with that occult meaning.<sup>452</sup>

The use of compositionality as a methodological constraint, then, requires assiduous attention to the semantic details of every lexical

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<sup>451</sup>Given a sufficiently well-behaved meaning function. Here Zadrozny's remarks on 'systematicity' of meaning functions may have some bite. If my lexicon has atomic units  $a_1, \dots, a_n$ , and I want to obtain manifest meanings  $M_1, \dots, M_m, \dots$  for complex strings  $S_1, \dots, S_m, \dots$ , I can simply impose a compositionality principle of the form:

$$\mu(s.t) = \mu(s) \cup \mu(t) \cup \{ \langle S_1, M_1 \rangle, \dots, \langle S_m, M_m \rangle, \dots \}$$

Clearly what we want (and what seems to be embodied, albeit vaguely, in the notion of compositionality) is for the meaning of the whole to be *fully provided* by the meaning of the parts -- without the introduction of new information in the process of composition.

<sup>452</sup>More precisely, the unconstrained term gives us the power to assign, in a compositional manner, arbitrary manifest meanings to all nodes dominating that term in the sentential tree. If we have particularly strong pre-theoretic constraints, which fully constrain meaning on all levels, but which still leave the unconstrained term unconstrained, we can eliminate noncompositional behavior (a) at nodes immediately dominating the unconstrained term, in a strongly compositional manner, and (b) at all nodes dominating the unconstrained term, in a weakly compositional manner.



item. Any time a semantic theory assigns to lexical items meanings which 'look outward' by, e.g., having several levels (manifest and occult) which have the potential to interact differently with different imbedding contexts, we should be suspicious of claims that that theory is any useful sense compositional. Of course, those suspicions can be allayed, but only by showing that the outward looking meanings are independently motivated by concerns other than mere compositionality.<sup>453</sup>

#### §3.4.2.2 Quantification Tarskian and Anaphoric

As mentioned above, my account of quantification uses a non-compositional semantics. Thus in a sentence like:

(619) Most philosophers know logic.

analyzed as:

(620) [philosopher x]x (MOST x)(x knows logic)

we cannot determine the meaning of the subcomponent 'x knows logic' purely from the inside out, since the meaning of the variable 'x' will be influenced by the meaning of the restricting term attached to it.

Of course, there are compositional ways of formulating my semantic project. The easiest is perhaps to have all variables initially carry all possible plural references as their semantic values. In (620), all of those plural references would then undergo distribution by the distributor MOST, and then this array of distributed references would then be screened by the restrictor 'philosopher' to allow through only those which resulted from the distribution of the appropriate plural

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<sup>453</sup>Thus, for example, Frege's sense/reference distinction, while prima facie of the form of a 'Zadrozny dodge' by giving terms two levels of meaning -- one to interact with extensional contexts and one to interact with non-extensional psychological contexts -- has, in the thought that there is a necessary distinction between what we think and the way we think it, a plausible motivation for positing those two levels which is independent of mere concerns about compositionality.

reference to all philosophers. In this way the process of meaning building would be strictly compositional, from the inside out. However, such a reformulation is clearly a version of the 'Zadrozny dodge'. The underlying truth of the semantic mechanism is the non-compositional story I have relied on throughout, and then through the use of occult meanings we encode all potentially needed meanings into each variable term and allow the later lexical context to 'screen out' those which are not actually needed.

My suggestion, however, is that the standard Tarskian semantics for quantified logic relies on exactly the same dodge. In a Tarskian semantics, a sentence like (619) above will be analyzed as:

(621) [most x: philosopher x](x knows logic)

but here 'x knows logic' will be given a *context-independent* meaning, in terms of satisfaction conditions. Using double bars ('||') to indicate denotation, we can say:

(622) ||x knows logic|| = { $\sigma$  |  $\sigma(x) \in$  ||knows logic||}

The meaning of the open sentence, then, is a set of sequences which satisfy the relevant predicate(s). That set of sequences is then further screened by the quantifier, and we get:

(623) ||[most x: philosopher x](x knows logic)|| = { $\sigma$  | (MOST  $\sigma'$ ) ( $\sigma'(x) \in$  ||philosopher|| &  $\sigma' \in$  ||x knows logic|| & ( $\forall y$ ) ( $y \neq x \rightarrow \sigma'(y) = \sigma(y)$ )}<sup>454</sup>

Thus we take the meaning of 'x knows logic' and consider only those components of that meaning which are located sufficiently close in sequence-space to sequences about philosophers who know logic. The same

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<sup>454</sup>There is a slight use-mention confusion in this formulation. I take it the appropriate complication is obvious.

meaning could also combine -- but in a different way -- with any number of other quantifiers, each of which would exploit some other structural feature of the class of sequences giving the meaning.

The Tarskian semantics, then, strikes me as a paradigm of the 'Zadrozny dodge'. What meaning the variable needs to contribute ought to depend on the quantifier binding that variable, and that quantifier will, of course, be outside the scope of the variable, necessitating an 'outside in' semantic composition. But we get around this difficulty by assigning, prior to consideration of the quantifier, classes of infinite sequences as meanings, so that no matter what quantifier we actually encounter, we will have the information necessary to see how the variable ought to interact with that quantifier. If this is right, then the Tarskian semantics is really no more compositional than my own.

#### **§3.4.3 Some Reasons For Compositionality**

Of course, showing that the Tarskian semantics is compositional only through the use of technical trickery does not exonerate my account from the charge of non-compositionality. At best, it shows that the two approaches are equally guilty. In this last section, I want to consider two reasons for wanting a compositional semantics in the first place, and show that both of these reasons are equally satisfied by the particular species of non-compositionality my account enjoys.

One argument for compositionality derives from considerations of learnability by speakers of finite cognitive resources. This argument is most famously pursued in [Davidson 1965]. The difficulty is to see how we, with our finite resources and finite exposure to the language, could have the capacity to understand a potential infinity of sentences. The

suggestion is that a compositional semantics will account for this capacity, since we need only grasp (a) the meanings of a finite collection of semantic primitives and (b) the compositional semantic rules.

The second argument for compositionality derives from considerations of the syntax-semantics interface. The thought here is simply that since natural languages have a compositional syntax, in which sentences are represented by hierarchical tree structures, the simplest picture of the connection between syntactic inputs and semantic outputs will call for a compositional semantics which starts with semantic assignments to lexical items at the lowest nodes in the syntactic tree, and then composes meaning upward through the tree according to some rule or rules.

Neither of these considerations, however, tells against the particular non-compositionality we find in my account. Note first that what the learnability considerations really call for is a *computable* semantic theory. As long as there is some computable function  $f$  mapping syntactic inputs to semantic outputs, finite speakers will be able to learn that function and thus master the totality of the language. Compositionality, then, is of interest simply because having a compositional meaning function is one particularly easy way to guarantee that the meaning function be computable as well.<sup>455</sup> But my semantics for quantification, while not compositional, is clearly computable. There is a simple and computable process for determining the meanings of the variables given the meanings of all the other lexical items. That

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<sup>455</sup>Although we depend here on the further assumption that the syntax is also computable.

process is not a strictly 'inside out' process, but that fact need not interfere with its suitability for explaining the semantic competence of speakers.<sup>456</sup>

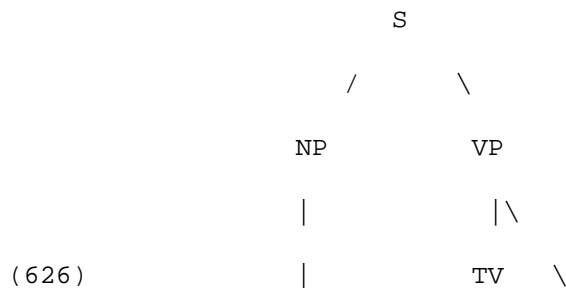
That the simplicity of the syntax-semantic interface does not necessitate a strictly compositional semantics can be seen by considering simple pronominal anaphora. In a sentence like:

(624) John likes his car.

with its hierarchical structure:

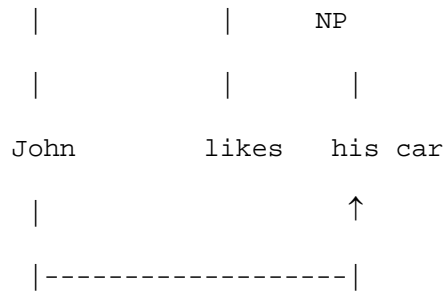
(625) [<sub>S</sub> [<sub>NP</sub> John] [<sub>VP</sub> [<sub>TV</sub> likes] [<sub>NP</sub> his car]]]

we cannot work strictly from the inside out, because the fact that 'his' refers to John must be transmitted down the syntactic tree, rather than up. Similarly, under the anaphoric account, facts about variable reference are transmitted down the syntactic tree. The key to understanding these occasional departures from compositionality lies in realizing that in addition to the tree-like hierarchical relations between terms in syntax, there are also 'horizontal' coindexing relations along which semantic information can be passed. Thus (624) above is best represented as:




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<sup>456</sup>Contrast this computability with the (at least potential) uncomputability of a Fregean sense-based theory which requires iteratedly indirect senses for terms in iteratedly oblique contexts. Here semantic mastery will require mastery of an infinite hierarchy of increasingly indirect senses, and this mastery may be (but is not necessarily) beyond the means of finite beings like us.



with an additional arrow joining 'John' and 'his'. The simplest syntax-semantics interface here will allow 'John' to pass its referent along this arrow to 'his'. Compositionality, then, is merely an accidental byproduct of a certain conception of syntax. Once we see that a more complex notion of syntax is called for, with horizontal as well as vertical organization, we see that the anaphoric account continues to respect, within that new syntactic framework, the goal of a simple syntax-semantics interface.

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