

Determiner and Quantifier Systems in Contemporary English

Richard Milne Hogg

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Summary

This thesis is a study of the grammar of determiners and quantifiers, as defined in the Introduction, in Contemporary English and it attempts, within the theory of transformational grammar, to determine the basic grammatical systems which control their linguistic behaviour. The thesis is divided into four parts.


Part I is an historical and critical survey of earlier grammarians. In Chapter 1 the work of grammarians within the 'parts of speech' tradition is surveyed. It is concluded that that theory is inadequate for a proper analysis of determiners, although individual grammarians show remarkable linguistic insight. In Chapter 2 the work of notionalist grammarians is discussed; it is concluded that they provide a useful foundation for further work, despite the lack of a formal approach. In Chapter 3 structuralist analyses, including early transformationalist analyses, are examined. Much of this, it is concluded, is of little value, but the work of later structuralists is seen to be most relevant.

Part II contains an evaluation of recent quantifier theory, and in Chapter 4 the grammar of both is considered from one current point of view. Despite theoretical inadequacies, it is seen that both contains elements

usually associated with all, the and two. In Chapter 5 the theories of Lakoff and Carden are analysed and it is concluded that their proposal that quantifiers are underlying predicates is incorrect. In Chapter 6 a rival theory proposed by Jackendoff is also found to be inadequate.

In Part III new proposals are made for the source of quantifiers. In Chapter 7 it is claimed that simple existential quantifiers, e.g., some, are derived from an NP whose verb is the abstract form EXIST. After further remarks on some, Chapter 8 claims that compound existentials, e.g., many, have a source similar to some, but with a nonrestrictive clause dependent on the quantifier-noun and referring to quantity. In Chapter 9 it is argued that the universal quantifier all has a source in a rather different higher sentence than that for some, where the predicate is quantity-referring, but there is no higher verb EXIST. Each is seen to have a similar source but every is more closely related to the compound existentials. In Chapter 10 quantifier-partitive constructions and the status of any are discussed and a modified analysis of both is also given. It is concluded that it is correct to postulate a higher sentence source for quantifiers, but that the rejection of a purely predicate source is also correct. The analyses given are a partial resolution of these claims.

Part IV is chiefly concerned with a and the. In Chapter 11 it is shown that a is best regarded as a morphological realisation of countability, rather than as an 'article' or numeral. In Chapter 12 the pronominal source for the suggested by Sommerstein is largely accepted, but cataphoric the is seen to be derived from a relativisation transformation. It is concluded that there is no justification for a grammatical category 'article'.



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Introduction

This thesis represents an attempt to provide a satisfactory analysis of the semantic and syntactic behaviour of determiners and quantifiers in Contemporary English, especially British English. However there is no claim that the analysis below is exhaustive and this is for two reasons. Firstly, there is the purely practical point that the range of behaviour found even within such a relatively restricted set of grammatical categories such as we propose to discuss is much too large for a work of this size; secondly, the aims of the thesis are not identifiable solely with providing a set of rules which will generate all and only all grammatical occurrences of determiners and quantifiers. Rather, they are to determine exactly a broad typological classification of the items under discussion and to examine what grammatical differences there may be between the types which are established. At the same time it is hoped that sufficient evidence will be discovered to enable us to decide at least approximately what kind of grammar is most likely to permit us to establish the most useful generalisations about the behaviour of determiners and quantifiers.

Before we elaborate on these theoretical points it is necessary, however, to clarify exactly what is meant by the terms 'determiner' and 'quantifier'. As might be

expected, there is considerable divergence of opinion amongst grammarians as to what words fall under each class, especially as to what words are determiners, and therefore part of our task must be to evaluate the adequacy of competing classifications, but this ought not to deter us from beginning with a quite simple definition which may be modified, if necessary, at a later stage. There is one great advantage in that determiners and quantifiers constitute a 'closed' set, or at least a set which is nearly closed; in other words the items so classified can be enumerated. We shall not attempt to do so, since it seems undeniable that, say, a new determiner could be added to the language, or that an existing one could become obsolete, as did, at an earlier stage of the language, fela, fele, meaning "many". But like other closed sets, determiners can be defined by their surface position, which simplifies matters considerably.

Rather than engage now upon a discussion of the various merits of competing descriptions of determiners and quantifiers, let us simply accept a definition of determiners given in a straightforward practical grammar of English, where theoretical considerations are minimised. Thus Christophersen and Sandved (1969:69) state:

"Determiners are words (or word-groups) that can occur in the positions occupied by the words the, a/an, my, our, your, their in

utterances like

The

A/An

My

etc. ('old) 'man ('men) 'died. "

Amongst the more important determiners which Christophersen and Sandved then list, there are, apart from the above, every, each, one, those, both, many, much, this, his, John's, no, all, some, any. We may dispute one or two of their inclusions and omissions, but basically the class of determiners is thus satisfactorily delimited.

In this thesis we make the further distinction that those determiners which contain a clear semantic component referring to number or quantity are named quantifiers. Thus we may extract from the list above the following quantifiers: every, each, one, both, many, much, no, all, some, any. It is hoped that this semantic distinction will be shown at a later stage to be completely justified.

Although the classification of determiners by Christophersen and Sandved, with our later subclassification of quantifiers, is accepted here, we shall in fact subtract a further group of items which will not be discussed within the main body of the thesis. This group contains the possessive pronouns my, our, etc. and the genitive noun type exemplified by John's, above. In the latter case there is clear evidence that such

constructions are not determiners proper, for example, the possibility of sequences such as the man's where the genitive noun is preceded by the suggests that these constructions are more profitably analysed as nouns which may end up in surface structure in the same position as a determiner and that to search for further connections would be misleading. Our exclusion of possessive pronouns is on rather different grounds. It is undeniable that the grammar of possessive pronouns is intimately connected with that of personal pronouns, and although it is probably the case, as we shall argue in Chapter 12, that the grammar of such pronouns is closely related to at least the amongst the (other) determiners, there are a number of major grammatical areas, such as coreference and pronominalisation, which are central to the grammar of pronouns but by and large peripheral to the grammar of determiners in general. Therefore, for what appear to be fully justifiable reasons of space and time on the one hand and internal coherence of study on the other, we make virtually no reference to the grammar of personal pronouns, and hence possessive pronouns are also rarely mentioned.

As we have already stated, one of our principal aims is to establish a quite basic typological classification of the determiners and quantifiers as defined immediately above. Indeed, we shall attempt to show that there are four major grammatical systems which

account for the differing behaviour of the defined items; further, one such system, we shall claim, contains two major subsystems. In order to examine the nature of each of these systems and subsystems we shall concentrate our attention upon what we shall, it is hoped, show to be the paradigmatic members of each system, that is, those words which most clearly demonstrate the individual characteristics of each system. Therefore, for a large part of this thesis we shall be primarily interested in the following five determiners: some, many, all, a and the. But this does not imply that a quantifier such as few or a determiner such as this will not be discussed; when they are discussed, however, the major concern will be to establish the relation between that determiner and the paradigmatic item in question. For better or for worse we are not investigating individual determiners, we are investigating determiner systems; therefore the principal aims must be the determination of such systems (and their paradigms) and the relation of individual items to a particular system. Furthermore, we do not discuss words such as enough, which although they have a considerable claim to be considered as determiners, are apparently so idiosyncratic that they do not clearly relate to any general system. But until such general systems are agreed upon, there can be little hope that idiosyncratic behaviour can be usefully analysed.

Having now defined the area of English grammar which is to be studied, it would now be useful to say a few words about our theoretical approach, of which there are two fundamental components. The first of these is that a grammar of (a particular part of) a language ought to deal with both semantics and syntax and that, further, no clear distinction can be drawn between the two. This is not to claim that there are no areas which are exclusively syntactic, e.g., affix-moving rules, or that there are no areas which are exclusively semantic, e.g., selectional restrictions (perhaps), but that the number of areas where the two are intermingled, as is even the case in the relatively basic instance of concord, is so great and the methodology required to solve the different problems so similar, that a separation of syntax and semantics would lead to undue complication of and a loss of adequacy in the grammar. Throughout our thesis we shall attempt to justify this claim in more detail.

The second component of our theoretical approach is that we accept that some variant of generative transformational grammar is most likely to permit an adequate account of determiners and quantifiers. To a very large extent the justification for this is presented in Part I of our thesis, and therefore we need not now discuss the merits and demerits of transformational grammar except to state that it ought to be evident by the end of this

thesis that an analysis of the surface structure of determiners and quantifiers is alone quite insufficient to permit useful claims and generalisations to be made about the items concerned. Transformational grammar may well have its defects, but it is at present the most promising of theories which can be used to investigate further into the grammar of a language. The particular variant of transformational grammar which is used here is largely a combination of that presented by Katz and Postal (1964) and the more recent theory of 'generative semantics' proposed in various papers by, amongst others, Bach, Lakoff, McCawley, Postal and Ross. In more detail, it is claimed here that base rules generate underlying semantic representations from which surface structures are derived by meaning-preserving transformations. But many of the more recent accretions to transformational theory, such as global rules and the use of logical notation, are not used here. It is clear that if we can do without such apparatus, and it is argued at several different points in this thesis that this is the case, then we have a grammatical model which is more strictly constrained and hence, if it performs the same tasks, more adequate. Similarly, although in fact this follows from our use of meaning-preserving transformations only, we do not use rules of semantic interpretation of the type proposed by Chomsky, Jackendoff and others. It is clearly in the interests of linguistics to restrict as far as possible the power of specific grammatical models

and therefore it is one of the aims of this thesis to show that such recent additions to the power of transformational grammar are by no means as justifiable as has been thought.

Finally, it may be in order to say a little about the plan of this thesis. In Parts I and II we discuss the work of previous and contemporary grammarians respectively on the subject of determiners and quantifiers, while in Parts III and IV we present our own theories. This is done not simply because it is useful to know the intellectual background in which a study is formulated, but also because, inevitably, many of the ideas in this work itself cannot claim to be totally original. Therefore it is only proper that we first acknowledge the debts to others. A secondary factor is that by first noting the mistakes of others we may then eliminate them and proceed to our own suggestions, having, hopefully, profited from those mistakes. One possibly unfortunate consequence of this approach, it will be discovered, is that the 'articles' are the primary topics of discussion in Parts I and IV, the quantifiers in Parts II and III. This arises from purely practical considerations. For a variety of reasons which will be discussed in Part I, earlier grammarians concentrated their attention to a very great extent on the 'articles', but this situation has been reversed by contemporary writers, who have seen quantifiers as the more interesting and more important

group of determiners. We agree with this latter opinion, as should be clear from the arguments in Part III and, especially, Part IV. Therefore it is perhaps not unfitting that a discussion of quantifiers should occupy the central sections of this work, with the other determiners in a rather more peripheral position. In one respect it may seem unfortunate that virtually all discussion of the 'articles' is dropped for seven chapters, only to be resumed in the final stages, but in another respect it usefully reflects the central priorities and conclusions of this study.

Part I

An Historical Survey

Chapter 1

The 'classical' tradition

1.1 Introductory remarks

Almost every aspect of linguistic investigation can be found to have its source in the works of the Greek and Latin grammarians. There is no need for us to ask why this is so; rather, what we must be aware of is the vast accumulation of linguistic knowledge which is the indisputable result of the work of these grammarians. Therefore the following pages are devoted to an examination of the wildly oscillating status of the grammatical items which we have defined in the Introduction within the theoretical framework first provided by the Greeks and then only slightly modified by the Romans.

If we were to confine our study to classical grammarians proper, that is, to commence with Plato and Aristotle in Greece and end with Donatus and Priscian in Rome, then we would seriously distort the true perspective. For the classical tradition did not die with the decline of the Roman Empire, but continued on through the Middle Ages into more modern times. It may even be claimed that it is still alive today, and it is certainly true that 20th century grammarians such as Poutsma and Krusinga owe their theoretical framework to the early Greek and Latin linguists, even if that theory is

more than a little modified. This persistence of the classical tradition is clearly seen in much of European, and especially Dutch, linguistics, and can be related to more general aspects of European culture.

Therefore we cannot define the classical tradition by chronology, but must define it by a common set of theoretical principles. Nevertheless, we still run into trouble, for there is no one set of such principles to which we can confidently claim that all classical grammarians have adhered, or must adhere. For example, Aristotle, and to a slightly lesser extent the Stoics, used semantic or notional criteria to determine their analyses; on the other hand, the late Latin grammarians made greatest use of morphological criteria, thus reflecting a totally different philosophy of science. But perhaps all classical grammarians have in common what we may term the 'parts of speech' approach, which, although it may have been present in the writings of Aristotle, was established by, above all, the Stoics, and which with surprising rapidity approached the status of dogma. The dogma was not purely linguistic, it reached further: Michael (1970:51) reports one medieval scholar as claiming that:

"The whole church, however, holds that there are only eight [parts of speech:RMH], and I have no doubt that this is divinely inspired."

As we shall see, such reliance on dogma could lead to gross absurdities, but it would not be fair to accuse all, or even most, classical grammarians of following this line. Most accepted that languages consisted of a possibly universal set of categories (all too often based on Latin), and that the description of the function of these categories was the task of the linguist. Thus the 'parts of speech' theory was best equipped for a consideration of discrete parts of sentences, rather than sentences as a whole. This is perhaps the major distinction which we can make between the classical tradition and other traditions of linguistic analysis; the distinction between a notional approach and a formal or morphological approach, cf. Chapter 2, is here essentially subsidiary.

1.2 The Greek grammarians

Although the foundations of the classical tradition were laid by Plato in his dialogue The Cratylus, it is only in the work of Aristotle that we first glimpse an account of those items which are the object of our

study.¹ Aristotle believed that there were three major syntactic categories, which he called ὄνομα ("onoma"), ῥῆμα ("rhema") and συνδέσμος ("syndesmos"). These terms are often translated, rather dubiously, as 'noun', 'verb' and 'conjunction'. The basis for this tripartite division is both notional and morphological. As far as our interests are concerned, it is the category of syndesmos which is the most important, since it was there that articles and pronouns were placed. It is not clear into which category the quantifiers fall, but Aristotle's system allows them to be considered either as rhemata or syndesmoi. This is because quantifiers appear to have a number of predicative features, as will be discussed most especially in Chapter 5, and the rhemata are essentially predicates, cf. Robins (1967: 26-27). On the other hand, quantifiers share a number of features with pronouns, and this has led the most recent scholars within this tradition to class them as pronouns, see below §1.5. Therefore it might not be totally foolish to conclude that quantifiers, like articles and pronouns, ought to be classed as syndesmoi within an Aristotelian framework.

¹ The best accounts of the Greek and Latin grammarians are to be found in Robins (1951; 1967:9-65), Michael (1970) and Dineen (1967), to all of which the reader is referred for further information. The discussion in §§1.2 - 1.4 is in large measure based upon these works.

Nevertheless, it is over-ambitious to attempt to draw any serious conclusions from Aristotle's own writings, because of the scantiness of the evidence, and we must look to the Stoics for a more complete exemplification of the early classical analyses. The Stoic's most important innovation, as far as we are concerned, was the division of the syndesmoi into two categories: syndesmos and ἄρθρον ("arthron"). Syndesmos now covers only conjunctions and prepositions; the arthra are what we call articles and pronouns. It is interesting to note here that the term arthron looks as if it covers almost exactly those elements which would be dominated by a Determiner node in at least the early formulations of transformational grammar, which in essence means those items which we defined as determiners in the Introduction, cf. too Chapter 3. Quantifiers, however, still present major problems, and to claim that quantifiers are Stoic arthra is certainly a case of ex post facto rationalisation made on the evidence of 20th century analyses. But there is scarcely any other problem of general principle, especially if we note Sommerstein (1972), where it is claimed, as we shall see below, that articles are underlying pronouns.

But it is totally false to read into the Stoic classification the implications of Sommerstein's work, for the very simple reason that the Stoics relied on quite different criteria. Their main reason for

separating out articles and pronouns from the other syndesmoi was morphological: the arthra were to be defined as the inflected members of the original class of syndesmoi. Nor must we forget the very close etymological relation between the Greek definite article and the relative pronoun; the former was $\delta, \eta, \tau\acute{o}$ and the latter was $\delta\grave{\epsilon}, \eta\grave{\epsilon}, \acute{o}$. This relation is common in other languages too, of course, cf. the German forms der, die, das. Therefore it can be seen that the Stoic categorisation relied to a very little extent on semantic or syntactic criteria, but was based primarily on morphological and etymological evidence. It may well be that the former criteria lead to the same conclusion, but that is a matter of pure coincidence unless it can be proven that there is a logical relation between the two different types of evidence. The assumption of such a relation without sufficient evidence can lead to quite unfortunate results, as is most easily observed in the work of a number of mid-19th century linguists. Some of the best examples of the school referred to here are to be found in the Proceedings of the Philological Society, which were published between 1842 and 1853, and of course that work has its intellectual origins in the materialistic theories of Horne Tooke (1798), which must often seem absurd to us.

One important distinction, at least historically, which the Stoics introduced was that between definite

and indefinite articles. The former category included personal pronouns; the latter included what is known today as the 'definite article' and also the relative pronouns. And so we find a state of utter confusion, which, it must be supposed, is amusing to the disinterested observer, where what is now called the 'definite article' was in Stoic terminology an indefinite article.² The terminological switch appears to have taken place in about the 18th century, so we shall consider the reasons (mainly dogmatic) behind it in §1.4. The reason for the Stoic contrast of definite vs. indefinite was semantic: the definite arthra referred specifically to one of first, second or third persons, as is implicitly stated in the modern term 'personal pronouns'; which person an indefinite arthron referred to had to be determined on each occasion by looking at the context, since all such arthra could refer to any of the three persons, cf. Robins (1951:30).

The distinction between personal pronouns on the one hand and relative pronouns and articles on the other

² In an attempt to avoid the confusion I shall use single quotes whenever the modern usage is intended, e.g., "The is the English 'definite article'." It should also be remembered that neither Greek nor Latin had any equivalent of our 'indefinite article'.

was characterised in rather different terms by the Alexandrian grammarian Dionysius Thrax, who called the former ἀντωνυμία ("antonymia") and the latter arthra. The former part of speech is defined by Thrax as "a part of speech substitutable for a noun and marked for person", the latter as "a part of speech inflected for case and preposed or postposed to nouns" (Robins, 1967:34).

Thrax probably represents the peak of the Greek grammatical tradition, especially because it was he who originated the classification of speech into eight parts, which, as we have seen, was later to be theologically approved. Therefore it is reasonable at this point to consider the merits of these first steps in the classical framework. The most obvious point is that the Greek grammarians had virtually nothing to say about quantifiers, whose status in the grammars of the time is highly obscure. This is in fact a recurrent inadequacy in the classical tradition and it extends right up to the present day. On the other hand, the special status of 'articles' and pronouns is recognised. We have already noted, however, that the emphasis on morphological criteria is unsatisfactory, even if, as it happens, it does lead to some interesting speculation, since syntactic criteria are underestimated. The reliance on morphological criteria is greatest in Thrax's grammar, and this probably accounts for the sharp distinction which that grammarian makes between arthra and antonymia,

which ought to be considered a retrograde step.

The danger of failing to appreciate the historical perspective is, nevertheless, always present, and therefore if it does not seem to us that the early Greek grammarians have many insights to offer us in a study of determiner systems, we ought to remember that they were taking the first essential steps. Even if a morphological bias tended to muddy the waters of linguistic inquiry, it is apparent that semantic evidence was also used and points such as the anaphoric use of the 'definite article' were noticed, cf. Robins (1951:43). Nor was the theory of 'parts of speech' yet fully hardened into dogma, which was later to lead to the absurdities found especially in the medieval-classical grammarians. Within a theory which is itself inadequate, the Greeks probably accomplished rather more than did any of their followers for some time to come.

1.3 The Latin grammarians

The most radical difference between the Latin and the Greek grammarians is not to be found in any theoretical divergence, but in the data which Latin, as opposed to Greek, provided. In Latin there was no equivalent of the Greek 'definite article', still less of Modern English a. Therefore Thrax's classification of arthron amongst his eight parts of speech was no longer applic-

able, at least by reference to the immediately observable data. The most detailed Latin grammar, that of Priscian, omits arthron - Latin articulus - and to preserve the number of parts of speech at eight, a new part of speech, interjectio ("interjection") is created. This desire to keep the number of parts of speech fixed at eight is symbolic of the derivative status of most Latin grammars, which provide us with no deeper insights into the status of either quantifiers or 'articles'. Indeed, Priscian himself appears to have been in error about the semantic uses of pronouns, see Robins (1967: 57-58). Amongst the more important Latin grammarians only Varro has a separate part of speech articulus, by which he means a case-inflected word which is not a noun, cf. Michael (1970:67). However, this retention of articulus looks very much like a matter of purely terminological dispute which is without linguistic significance. We are, in fact, back to the Stoic position where article and pronoun combine as one part of speech. It is interesting, nevertheless, to note that for Varro hic ("this") is an articulus finitus, whereas quis ("who") is an articulus infinitus.

Medieval Latin grammarians almost exclusively follow Priscian in not including articles as one of the parts of speech, and we can reasonably suspect a fossilisation of the theory. Even the most perceptive grammarians, finding the particular facts of Latin in

conflict with the widespread linguistic presence of equivalents for the and a elsewhere, can find space within their theory and practice, both of which are language-dependent, for little other than regret. Thus Roger Bacon admits that there is a strong case for establishing 'article' as a part of speech, for:³

"Almost every people has them [articles:RMH],
and the French language has in that category
li, le, las and so on."

Bacon compromises by calling hic, haec and hoc "pronomina articularia" (Michael, 1970:68). We may conclude that if this reflects a general state of linguistic thought, there is little to be gained from a fuller exposition of Roman and medieval theories about 'articles' or determiners in general.

1.4 The vernacular grammarians

The early grammarians of English who followed the classical tradition were faced with the grave difficulty that the surface structure of English was radically different from that of Latin; yet for the most part they wished to retain the broad outlines of the Latin grammars. As far as we are concerned, this adherence to a

³ Quoted and translated from Michael (1970:68):

"Omnes fere nationes habeant eos, et lingua gallica habet eos ut li le las et huius modi..."

theoretical position insufficiently supported by the surface evidence is most relevant to the status of the 'articles'. It will be recalled from §1.3 that Latin grammarians did not in general classify the 'article' as a separate part of speech, and that this continued to be the case through the Middle Ages, despite the conflicting evidence of the developing or developed vernacular languages which Roger Bacon, at least, noticed. When the writing of English grammars became a fit study for a linguist to undertake, this conflicting evidence could no longer be ignored.

But the question remained of how the and a could be systematised within the theory, and, of course, a presented a further problem in that it had existed in neither Greek nor Latin. Some grammarians attempted to classify a and the as signs of cases, which is interesting only for the absurdities which followed, and not for any insights. So we find that the 17th century grammarian Jeremiah Wharton quotes the following declension of book (1654:35), cf. Michael (1970:350):

Singularly,	N.	a book,	Plurally,	books,
	G.	of a book,		of books,
	D.	to a book,		to books,
	A.	the book,		the books,
	V.	o book,		o books,
	Ab.	in a book.		in books.

Even Wharton, however, finds it difficult to justify the

statement that a is the sign of the nominative and the of the accusative; but "there is no better way to distinguish them in declining".

More interesting are those grammarians who do not create a new part of speech 'article', but attempt to include the 'articles' within other classes. Especially so is the claim made by William Turner (1710:7) and reported in Michael (1970:221, 354). Turner claims that the is a pronoun and a an adjective. It is not clear from Michael's comments exactly what justification, if any, Turner gives for this, but it is certainly quite remarkable in the light of recent linguistic research. Turner is making exactly that claim for the which is made by Sommerstein (1972); in the case of a we need to make only one jump in the argument. As we shall see below, within the classical tradition the status of quantifiers is always dubious, but let us, with some justification, assume that for Turner they would be adjectives - or, more properly, a subtype of noun, since, in keeping with the prescriptions of Latin grammars, he does not recognise adjectives as forming a separate part of speech. Now let us accept that a is more quantifier-like than 'article'-like (where the is the paradigmatic 'article'). This seems reasonable enough, although it cannot be justified at this point; note, however, the claims of Perlmutter (1970), which will be discussed at length in Chapter 11. The next step is to recognise

that Lakoff (1970b, 1970d) and Carden (1968) have proposed that quantifiers are syntactically related to adjectives. If this is in any measure correct, notwithstanding the criticisms which are made of the Lakoff-Carden hypothesis in Chapter 5, then we can say that there is in recent transformational writings some evidence that transformationalists too would class the as a pronoun, a as an adjective. Whatever the rights and wrongs of that claim, and however plausible or implausible Turner's own arguments may be, it can hardly be denied that he has some right to claim that he introduces an element of déjà vu into even the most recent and (apparently) original claims.

It was only by the end of the 18th century, cf. Michael (1970:355) that classical grammarians of English generally accepted that the 'article' was a separate part of speech. The tardiness of this acceptance must be attributed to the fact that 'article' as a category had no traditional status in the descendants of the Latin grammars, for the reasons which we discussed in §1.3, above. Further, even when it was conceded that 'article' was one of the parts of speech, classical grammarians tended to concentrate on syntactic criteria to a rather greater extent here than was the case with the other, morphologically-established, parts of speech. From the modern point of view this is a theoretical improvement, but we should not over-value the change.

Essentially it is one forced upon the vernacular grammarians by the inadequacy of the classical theory, and it fits badly, if at all, into the classical tradition.

To summarise the results achieved by the grammarians who considered the 'article' to be one of the parts of speech in English would be impossible within the space which we can permit ourselves here, and therefore the following is only an attempt to highlight a few points of theoretical interest. The first point which we should notice is that even when the 'article' is classed as a separate part of speech, there is still a tendency to regard the members of that category as related to some other category as well. Thus Wallis (1653:71, 72) states:

"A ... est articulus Numeralis; atq; idem
omino significat ac one unus, sed minus
emphatice."

"The est articulus Demonstrativus, idemq;
significat ac that illud sed minus emphatice."

Wallis' terms "numerical article" and "demonstrative article" serve also as a reminder that the opposition 'definite' vs. 'indefinite' as we know it today has not always been in common use amongst vernacular grammarians. Michael (1970:361-62) states that the first recorded use in an English grammar dates back only to 1662, and that the present-day usage becomes common only towards the

latter part of the 18th century.⁴ It is fascinating to speculate upon the usage of 'definite' and 'indefinite', especially when we remember that we have found a startling reversal of Greek terminology, cf. §1.2, but we can conclude little else than that the usage has changed, either through a misunderstanding of Greek descriptions, a subject about which the vernacular grammarians were, according to Michael (1970:350), rather less than well-informed, or through simple processes of semantic change.

The 18th century classical grammarian James Harris is worthy of attention in that he provides one of the clearest explanations of a well-known contrast between the and a. Harris writes (1771:215-6):

"(A) respects our primary Perception, and denotes Individuals as unknown; (THE) respects our secondary Perception and denotes Individuals as known. To explain by an example - I see an object pass by, which I never saw till now. What do I say? "There goes a Beggar with a long beard." The Man

⁴ What Michael does not note, however, is that the opposition of 'definite' vs. 'indefinite' is to be found in Palsgrave's grammar of French, dated 1530, in other words over 130 years earlier than the first recorded use in a grammar of English.

departs and returns a Week after. What do I say then? "There goes the Beggar with the long Beard." The Article only is changed, the rest remains unaltered."

It may well be that Harris' description and exemplification is over-crude, but it is important in so far as it establishes that the demands, in many cases, previous reference of some kind. Harris (1771:217-8) also makes the important point that the lack of an 'article' with plural nouns shows that reference is to an unspecified set. As we shall see at a later stage, this is most relevant for an attempt to understand the function of quantifiers, and we need only take issue with Harris when he claims in the same passage that a always has the task of showing that reference is to a one-member set. There seems to be rather more to a than that.

After Harris has concluded his discussion of the and a, he continues (1771:233-34):

"The ARTICLES already mentioned are those strictly so called; but besides these are the PRONOMINAL ARTICLES, such as This, That, Any, Other, Some, All, No or None, &c. Of these we have spoken already in our Chapter of Pronouns, where we have shewn, when they may be taken as Pronouns, and when as Articles. Yet in truth it must be confessed, if the Essence of an Article be to define and

ascertain, they are much more properly
Articles than anything else, and as such
should be considered in Universal Grammar."

This is but one example of the vacillation which is common amongst classical grammarians when faced with the problem of quantifiers. The classical theories offered no obvious compartment into which quantifiers could be pushed, and therefore many grammarians categorised them as two, or even three, different parts of speech simultaneously. Further, it is rather unfortunate that the demonstratives this and that are separated by Harris (and others) from the, with which they have much in common, and instead allied to some, etc., with which they have much less in common. But it is difficult to see how the classical theories could be modified to provide a more adequate framework for description. Even if, for example, the category 'article' were subsumed under a more general 'pronoun' category, the only result might be an unwieldy and undifferentiated mass of highly varied items.

In contrast to the Latin and medieval grammarians, these early students of English certainly made a substantial contribution to our knowledge of the semantics and syntax of determiners and quantifiers. But all the time it has to be recognised that they were working within a framework which had two major disadvantages. Firstly, it was conceived originally only as a theory

for the description of Greek, and so it was unsuited for either the description of English or Harris' "Universal Grammar". Secondly, and relatedly, it was essentially a surface structure theory and therefore ignored the underlying semantic and syntactic regularities. In fact this latter point is not always true, for a number of linguists from different ages went far beyond the surface. Harris is one such example, and Chomsky (1966) is partly a description of a similar group of linguist-philosophers, but it seems fair to say that he does not give sufficient acknowledgement to the predecessors of the Cartesians, cf. Salmon (1969).

1.5 Recent classical grammarians

The classical tradition has continued up to the present day, although perhaps in a modified form. Most importantly, many of the more recent 'classicalists' have been greatly influenced by the work of scholars such as Jespersen, and indeed it is often difficult to see what distinguishes the two types of linguist. Nevertheless we shall delay any discussion of Jespersen and other linguists with a similar theoretical background until Chapter 2, and at this point concentrate our attention on what can reasonably be regarded as the paradigmatic classical grammars of recent times. The major grammars to which we shall refer are those of Kruisinga (1932a), Maetzner (1874a, 1874b) and Poutsma

(1914). They, and other perhaps less interesting grammars, follow in outline the classical tradition in that they too consider nouns, verbs, etc. separately within their different functions as discrete members of the set of 'parts of speech'. Their aims are also similar to the early vernacular grammarians in that these later linguists also attempt a complete survey of the English language, although often with the marginal distinction that there is some emphasis on the teaching of English as a foreign language. Of course, this description also fits a large part of the work of Jespersen too, but here we have to differentiate between the theoretical demands of one particular hypothesis and the general aims of any linguist. I believe that it is fair to say that Jespersen was attempting to reach a goal which every linguist must wish to reach in the end, but that on the other hand the classical grammarians were fulfilling an essential demand of the theory to which they adhered. A 'parts of speech' grammar must deal with the whole language, albeit discretely.

As in earlier studies, the status of 'articles' and quantifiers varies from grammar to grammar. Thus Poutsma (1914) considers that the 'articles' are a separate part of speech, but that this and that and all the other items relevant to our study are to be regarded as subtypes of pronouns. These subtypes include demonstratives, determinatives (same and such), interrogatives,

relatives and indefinites (most of the quantifiers). Kruisinga (1932a) regards all the determiners and quantifiers, including the 'articles', as belonging to various subclasses of pronouns, and thus follows the pattern first set down in the Stoic grammars, see above, §1.2. Maetzner (1874a) is rather unusual in claiming that there are only three primary parts of speech; these are "noun", "verb" and "particle". The first two of these categories resemble the categories of noun phrase and verb phrase within transformational grammar; "particle" is apparently a cover term used to describe all those elements which cannot reasonably be described as "noun" or "verb", for example, conjunctions and interjections; a similar system is to be found in Wallis (1653). Within Maetzner's system determiners and quantifiers are classed in the category "noun". But Maetzner makes numerous subclassifications which have the result that his system is not as radically different as it first appears to be. Thus the 'articles' are given a secondary classification which is different from that for all the other items, which are considered as various types of pronouns. More recent classical grammars, for example Zandvoort (1957) and Scheurweghs (1959), tend to separate the 'articles' from the other determiners at an early stage, and so clearly follow the example of Poutsma, and, in a less apparent manner, that of Maetzner too.

The reason for the separation of the 'articles' from the other determiners is that the 'articles' cannot be used without a noun, whereas the others can be used nonattributively and are thus termed 'pronouns', given Maetzner's definition to be correct (1874a:290):

"The pronoun, which represents a noun in the sentence, or, more correctly, has the nature of a noun, and has thence its name, is, by its value and idea, distinguished from a mere sign for a substantive or adjective, although it partly serves to avoid the repetition of the same noun."

But this is hardly a position with which we can agree, for several reasons. It is not the case that all of the 'non-article' determiners can stand alone in a noun phrase. Maetzner himself concedes that this is not true in the case of every and there are alternations such as no/none which clearly demonstrate that some determiners when used nonattributively are subject to morphological change, cf. Maetzner (1874a:308-15). There is also the similar alternation in possessives, e.g., my/mine. The argument would appear to be at its strongest with personal pronouns, but even there it is doubtful, since, at the very least, you cooccurs with nouns, as in you boys. However in this last case we must beware of accepting as a fact the analysis suggested by Postal (1966), about which there are grave reservations. See the discussion of this point in §12.1.

Of course, simply to point out that you is capable of being used attributively, that is, in collocation with a noun, does not, even if it is true, disprove Maetzner's point, but there is an argument deriving from this which does do so. Let us accept that all the so-called pronouns of Maetzner, Poutsma and, indeed, Kruisinga may be used attributively. Given this, can we not suggest that in fact there is always a noun cooccurring in underlying structure but that, in certain cases, this noun may be deleted? This would account for the position in English where most, if not all, pronouns can be used both alone and with a following noun. In what respect do the 'articles' differ? In the fact that they do not occur alone in a noun phrase. Now presumably the fact that they do not do so is in some way connected with the further fact that the 'articles' are proclitic, i.e., unaccented, and this provides us with some evidence of relevance. If the 'articles' were ever to occupy a non-attributive position in a noun phrase, they would perforce carry some measure of stress and thus would assume different phonological forms, from which it might well be possible, and, moreover, reasonable, to derive the proclitic forms.

The evidence from proclitic forms leads us to the real reason for the rejection of any classification which separates the 'articles' from the other determiners and quantifiers, for it seems most likely that if

the and a are proclitic derivatives it is among the other determiners that we shall find their accented equivalents. Indeed, for a there is considerable evidence that the accented equivalent is one, as can be observed from the following remarks:

"The indefinite article is, speaking historically, the weak form of attributive one."

(Kruisinga, 1932a:312)

"Numeral and article are identical in form in OE. The possible difference in stress cannot be documented. About the middle of the 12th century the abbreviated form a began to make its appearance. This is important because it shows that the word had by then lost its inflexional endings and become unaccented."

(Christophersen, 1939:107)

The stressed equivalent of the would appear to be the demonstrative that:

"The name ARTICLE is given to the, weakened from the Anglo-Saxon demonstrative pronoun se, seo, þat..."

(Maetzner, 1874a:315)

"Most attempts to find a semantic distinction between the and that can be reduced to the statement that the is the unstressed form of that ..."

(Thorne, 1972:565)

It would appear very possible, therefore, that any explanation of the reasons underlying the non-occurrence of the and a alone in a noun phrase will involve the hypothesis that these are proclitic forms, possibly derived from that and one respectively, and can thus occur only when there is a non-deleted noun in the same noun phrase which will carry the stress. If this is the case, then there would appear to be at least two possibilities. Firstly, the 'articles' could be derived from that and one by stress and vowel reduction rules; this type of solution has been suggested as suitable for a by Perlmutter (1970) and as suitable for the by Thorne (1972). Secondly, even if such a purely phonological explanation were not accepted, one might still wish to derive the 'articles' from a source nearly identical to the source of the relevant demonstrative or numeral. If either of these solutions were accepted, then it would be clear that there would be no reason to suppose the 'articles' formed a part of speech separate from the other determiners and quantifiers. The only distinguishing factor would be that they are proclitic, and this can hardly be considered to be a sufficient reason for radically segregating the 'articles', although it might be a justification for a minor subclassification. However, in Chapter 11 we shall see that at least in the case of a the above arguments cannot be accepted; but the arguments there can in no way be construed as being

in favour of an 'article' theory, especially since they do not directly relate to the status of the.

Finally, there is a tactical reason for not wishing to keep the 'articles' separate from the other determiners: many classical grammarians, having accepted that the and a are the only members of one word-class, have then assumed that the and a perform roughly the same functions, with only one or two specific features of contrast between them; hence, perhaps, the modern usage of 'definite' and 'indefinite'. Not all classical grammarians do so, and in this context it is worth remembering that the tags 'definite' and 'indefinite' have not always been attached to the 'articles'. For example, the quotation from Wallis (1653) in §1.4 gives a much more apt description of these items. Assuredly, however, the hypothesis is all too tempting. Therefore, it seems wise not to make too early a judgment in favour of retaining the part of speech 'article' with its implicit but doubtful assumptions.

Kruisinga (1932a:315) succinctly shows that it is far from correct to claim that the and a perform very nearly identical duties:

"The two articles have distinct functions that have hardly anything in common. There is nothing in the indefinite article that corresponds with the defining, nor with the anaphoric definite article. Nor is there

any function of the definite article that is similar to the numerical or individualizing indefinite article. In one case, however, the two words have functions that clearly resemble each other; they have been denoted by the same term: classifying."

It can be seen that, apart from the introduction of the term 'classifying', which is a description of the function of the 'articles' in sentences such as:

(1.1) The lion is a dangerous animal

(1.2) A lion is a dangerous animal

Kruisinga's definition does not depart from the other classical descriptions of the 'articles' except in his emphasis on the wide range of differences between them. He suggests that the 'definite article' has three functions: (i) demonstrative; (ii) defining; (iii) classifying (1932a:238), whereas the 'indefinite article' has two functions: (i) individualising; (ii) classifying (1932a:315). That analysis may, for the moment, be accepted as it stands, but it should be recognised that Kruisinga's major achievement was his prudent refusal to regard the 'articles' as being in simple opposition.

However, one other point which we must take note of is that Kruisinga is most insistent about the importance of the deictic function of the, cf. Kruisinga (1932a: 239-41), and he closely relates the functions of the to those of this and that. From what we have said already

about the fact that the is possibly a proclitic form of that, taken together with Krusinga's evidence, it would seem clear that the relation between deixis and 'definiteness' is much closer than any mere accident would produce. It might indeed be that it is reasonable to consider the as the unmarked member of the set of deictics, thus asserting in classical terms that it may well be a 'pronoun'. However this is still rather speculative and cannot be considered seriously until we have looked in rather more detail at the semantic and syntactic behaviour of the. In this respect consider the remarks of Thorne (1974:111, fn. 1) and, more generally, our comments in Chapter 12, below.

With reference to the 'articles', Poutsma makes the following remark, which is even more pertinent when it is extended to a wider field (1914:517):

"The primary and most important function of both the definite and the indefinite article is to indicate that the thing of which we have formed a conception is marked off or defined, i.e., thought of within certain physical or imaginary outlines or limits."

In fact one would wish to quarrel with this statement as it stands, for it seems to be equally true of the other determiners and quantifiers as it is of the and a, for
in:

(1.3) some boys; much milk

some and much 'mark off' or 'define' the reference of boys and milk to the same extent as do a and the in:

(1.4) a boy; the milk

Might not Poutsma's remark be thought of as too general a statement to be of great interest? Hardly, for it is precisely because of this generality that the notion that 'delimitation' is the primary function of determiners and quantifiers is an important one.

Poutsma's claim leads to the suggestion that determiners and quantifiers are associated with the marking of sets, in that their function is to delimit the size of the set to which reference is made, and that perhaps the syntax of these items can be connected with the hypothesis presented in Bach (1968) that the underlying structure of nouns involves variables and predicates rather like those used in symbolic logic, cf. Chapter 7 for further discussion. Further, it perhaps accounts for the normal structure of generics being similar to:

(1.5) Boys are nasty creatures

(1.6) Lions live in zoos

where the noun phrases are not delimited and thus the reference covers the whole class which forms the potential referent, not merely a delimited set within that class.

On the subject of quantifiers, or 'indefinite pronouns', as they are often called, the recent classical grammarians tend to adopt a somewhat defeatist attitude,

as is exemplified by the following statement from Kruisinga (1932a:129):

"Attempts have sometimes been made to improve the classification of pronouns, but the only result has been, at best, to show the grammarian's ingenuity. The indefinite pronouns have especially been the subject of such experiments. And it is perfectly true that no definition has been given that applies to all of them. But there is no reason why we should attempt such a definition; it is enough to have a name to refer to a number of pronouns. The chapter on indefinite pronouns may be considered as the lumber-room of the pronouns; and a lumber-room may be as convenient in grammar as it is in a house."

The major source of the problems seems to lie not in the fact that these grammarians are interested only in the surface structure of language, which is patently untrue, but that they are constrained by their methodology to treat each item in isolation. And so, it is only when they discard such constraints and begin to compare, for example, each and every, that they are able to throw much light on the relevant syntactic problems. For instance, there is general agreement, cf. Poutsma (1914: 1066ff., 1081ff.) and Kruisinga (1932a:274-77), that each is strongly distributive, every weakly distributive, and all nondistributive. Such a distinction helps us to

explain certain features of the syntax of these determiners.

On the other hand, when there is a lack of association, then important insights may be lost. Thus Kruisinga (1932a:260, 267), Zandvoort (1957:172) and Maetzner (1874b:209) all fail to explore the relationship of both to all as fully as they might. Although one can find hints that both may be considered to be a dual form of all, these grammarians are reluctant to pursue the matter systematically.⁵ Similarly, the following quotation from Maetzner (1974b:255) with regard to much only serves to cloud the issues:

"In the positive it is only met with in the singular, and it may nevertheless in many cases stand opposed to the plural many, with which it has of itself nothing in common."

Once more, therefore, we may conclude that it is the theoretical background of these recent classical grammarians which is the main obstacle to a satisfactorily worked-out grammar. Where that theory is at its strongest, that is, in respect of the 'articles', the classical grammarians have produced many important insights which it would be foolish to ignore. However, even in that case the segregation of the 'articles' into

⁵ For a more extended consideration of the relation between all and both see Chapters 4 and 10, below.

a separate category means that the full implications of the best of the proposed analyses cannot be adequately recognised within the theory. And on the subject of quantifiers we are presented yet again with the failure of classical theory to provide a consistent explanation of their syntactic and semantic behaviour. But it is especially notable with the recent classical grammarians that a great deal of semantic evidence is taken into account and that this can be most illuminating. Therefore, in the next chapter we shall look at a group of linguists who recognise to at least the same extent the importance of semantic evidence, but who are not hindered by the same inadequate 'parts of speech' theory.

Chapter 2

Some notionalist theories

2.1 Defining 'notionalism'

It is reasonable to claim that many linguists, diverse both in time and theory, from Aristotle to Jespersen and from Thrax to Chomsky, could with equal justice be called 'notionalists'. We are therefore faced with a large and potentially unwieldy body of thought which we must attempt to define within given limits. And therefore let us immediately accept the following remarks of Jespersen (1924:55) as an adequate statement of notionalist principles:

"... beside, or above, or behind the syntactic categories which depend on the structure of each language as it is actually found, there are some extralingual categories which are independent of the more or less accidental facts of existing languages; they are universal in so far as they are applicable to all languages, though rarely expressed in them in a clear and unmistakable way ... for want of a better common name for these extralingual categories I shall use the adjective notional and the substantive notion. It will be the grammarian's task in each case to investigate the relation between the

notional and the syntactic categories."

Such a statement of principle would be accepted by many grammarians, and this is the root of the difficulty expressed above. Therefore, for heuristic purposes we must further restrict notionalism in the following manner: only if a commitment to notionalism is not accompanied by a previous commitment to some formal system shall we claim that a notionalist theory is being expressed. Thus a classical grammarian such as Poutsma will be excluded, since the basic commitment in his grammar is to a formal system. Similarly, although many transformational grammarians approach a notionalist position - for an early example see Lyons (1966) - they will not be discussed here. The grammarians discussed in this chapter may well have formal systems, but such systems are not prior, as is the case with the examples above.

It therefore follows that we characterise the term 'notionalism' as it is used in this chapter in two ways. Firstly, notionalist grammarians would all agree to disagree with the notorious remark of Katz and Fodor (1963:483) that:

"linguistic description minus grammar equals semantics."

Rather, they regard semantics as inextricably involved with syntax, and maintain that no syntactic description

worthy of the name can afford to ignore the semantic aspects of the subject. Secondly, among the notionalist grammarians discussed below, there is at least a certain eclecticism and at most a severe scepticism about the worth of formal systems. This is in sharp contrast to most of the classical grammarians discussed in Chapter 1 and the structuralists and early transformationalists who will be discussed in Chapter 3.

Yet despite this second characteristic, we can observe a simple division amongst notionalist grammarians of this century when we consider their approaches to 'articles' and quantifiers; this is largely due to the influence of the French linguist Gustave Guillaume. We shall see below that Guillaume's theories, especially in relation to a description of the 'articles', have influenced a number of other notionalist grammarians. We may therefore distinguish between works within a Guillaumiste tradition (although 'tradition' is possibly too strong a word) and other works which remain notionalist but are unconnected with Guillaumisme. This, incidentally, closely relates to a division of interest: within the Guillaumiste tradition attention is focussed exclusively on the 'articles'; when we look elsewhere our attention will be to a large extent focussed on the other members of the determiner and quantifier systems.

2.2 Guillaume and the 'articles'

Despite the brevity of our remarks above, it may already be clear that we have to place strong emphasis on the works of Gustave Guillaume, for his theories concerning the French 'article' system laid the theoretical foundations upon which much of the work that was to be done by notionalist grammarians in the ensuing decades was to be constructed; and this is true despite the scepticism general amongst such grammarians about the worth of formal systems. The definitive work for a study of these theories is Guillaume (1919), but there is further discussion and exemplification in Guillaume (1944, 1945a, 1945b).¹ Guillaume freely acknowledges his debt to psychology, and expressly states that his linguistic work is based on a theory which he calls 'psycho-mécanisme'; as far as we are concerned, we need

¹ For a discussion of Guillaumiste theories and an interesting, if, in the end, unconvincing, attempt to relate them to the theories of transformational grammar, see Toussaint (1967), in which there is a bibliography of work done by the Guillaumiste school. See too Guillaume (1971), especially the introduction by Roch Lavin. Hewson (1972) is an examination of the English 'articles' from a Guillaumiste point of view, but that work adds little of theoretical interest. For a penetrating criticism of Hewson (1972), see Sommerstein (1974).

only explain this theory in terms of Guillaume's description of the 'articles'. According to 'psycho-mécanisme', as advanced by Guillaume, the structure of language has two levels, one of which may be termed the conceptual or abstract level, the other the existential or concrete level. It is claimed that when we use language what in effect we are doing is taking items from the stock of concepts (at the abstract level) and 'conceptualising' them (at the concrete level). The further this process is taken, apparently, the more highly evolved is the language. According to Guillaume's theory, the purpose of the 'articles' is to signify the transition from one level to the other. Guillaume himself explains this as follows (1919:305):

"On passe ... d'un plan où les noms existent virtuellement à un plan où ils se réalisent effectivement. Dénoter les cas généraux de cette transition constitue le rôle de l'article, simple signe de relation entre une idée et un fonds d'idées."

The difficulty with such a thesis is that its acceptance is, as it were, an act of faith. Its basis is a type of hypothesis which one can neither prove nor blandly assume. Certainly, its acceptance might permit the presentation of a consistent theory of the 'articles', but the thesis lacks a clarity which might be more attainable if the theory were based upon, or at least

had a definable connection with, linguistic evidence. All too often Guillaume presents explanations which are both obscure and unenlightening outside the context of his 'psycho-mécanisme'. Even many of those linguists who have found Guillaume's theory a valuable starting point would assent to this criticism; for example, Chrstiphersen (1939:57) says:²

"His [Guillaume's:RMH] style and arrangement are wanting in perspicuity, and he is often so subtle that in spite of his wordiness and frequent repetition of himself, I do not pretend to a full understanding of all his points."

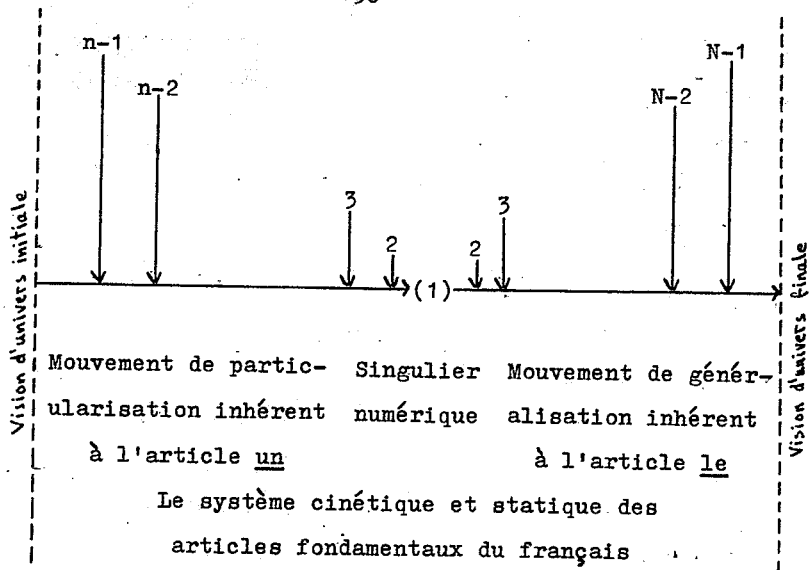
The topic of all the above-mentioned works by Guillaume is the two French 'articles' le (la, les, l') and un (une), and it is rarely that he strays further than the partitives (de, etc.). As we have seen above, in our discussion of some of the classical grammarians, this is almost certainly a mistaken position from which to consider any of the members of the determiner systems, since it sets up, a priori, a false opposition.

² The (anonymous) reviewer of Guillaume (1971) in TLS (1972), who contributes an extreme and passionate defence of Guillaume, also admits that Guillaume is often obscure, although the cause of this is claimed by the reviewer to be "terseness", which seems implausible.

This is patently true of Guillaume, who is to an almost exclusive degree concerned with a postulated opposition between the 'articles'. Thus he states (1944:93):

"L'article un y indique le mouvement par lequel la pensée, prenant de la distance par rapport à l'universel, s'approche par degré du singulier numérique. Autrement dit, l'article un du français symbolise dans la langue le mouvement d'approche du nombre 1, auquel il aboutit et avec lequel il ne se confond pas. L'article le, à l'inverse - une grande symétrie règne dans la partie formelle des langues évoluées - symbolise le mouvement par lequel la pensée prenant son départ au singulier déjà atteint s'en éloigne et tend, sans que dès lors, aucune limitation finale puisse lui être assignée, vers l'infinitude de la vision universelle."

Or, diagrammatically (Guillaume, 1944:97):



Such a false opposition leads inevitably to false conclusions: the 'definite article' is not the obverse face of the 'indefinite article', no more so in French than in English. The 'articles', as was argued above, in §1.5, each fulfil essentially different and not necessarily related functions. Also, although the syntax of number is an inherent part of the syntax of the determiners, and most especially, of course, of the quantifiers, its importance is considerably greater within the syntax of the 'indefinite article' than within the syntax of the 'definite article'. This is a fact which Guillaume cannot allow, see his remarks in Guillaume (1945a:209). It is impossible to discuss the grammar of the 'articles' in a linguistic vacuum; yet this is precisely what Guillaume attempts to do. A

satisfactory account of le and un (or the and a) must be contextualised within a far larger system. And although Guillaume does discuss much else in the grammar of French in other works, see especially Guillaume (1971), it is extremely difficult to claim that the necessary contextualisation ever takes place.

Guillaume's theories have had great appeal, however, to many European linguists, and this, we may speculate, is for the following reasons. Firstly, at one time he was the only linguist who had attempted to construct a comprehensive theory of the 'articles'. Secondly, his theory was flexible in that it was very general and could accommodate apparent contradictions, for example, the anaphoric and generic uses of the 'definite article'. Thirdly, his mentalistic outlook would appeal to that large number of linguists already sympathetic to Saussurean theories (although it would be incorrect to assume that de Saussure and Guillaume shared anything other than a vague similarity of scientific philosophy). Fourthly, there was no viable structuralist model, cf. Chapter 3, which could have pointed out the deficiencies of Guillaume's theories and at the same time provided a usable alternative. Indeed, it might be claimed that it was only after scholars such as Guillaume had achieved a certain amount of pioneering success that it was possible to start constructing a genuine and useful theory of the 'articles'. Therefore, one's criticisms of



Guillaume must be tempered by the knowledge that he was, essentially, attempting to do that which had not been done before, and for this he had to develop a framework of his own. Also, the lack of any rigorous formal apparatus, such as, perhaps, structuralism could later have provided, meant that the excesses to which we are all only too susceptible could not be automatically curbed. Guillaume's success lies not so much in his solving the problems that he set out to solve, which he patently did not do, but in creating an atmosphere in which such problems could profitably be discussed.

Louis Hjelmslev was perhaps the best known linguist to accept Guillaume's theory of the 'articles' without making major modifications. Hjelmslev considered that (1928:337):

"Le rôle grammatical de l'article est ... de concrétiser le sémantème, celui-ci étant par définition abstrait en lui-même. L'article dit défini est un morphème de concrétisation qui indique que l'objet ou sa qualité est supposé connu à l'interlocuteur (à celui à qui on parle). L'article dit indéfini est un morphème de concrétisation qui indique que l'objet est supposé inconnu à l'interlocuteur. L'article zéro est, par opposition aux deux autres articles, un morphème d'abstraction."

This is clearly an advance on Guillaume, for the reason that it is more closely within the context of a rigorously defined linguistic theory, one which is trying to cope with all types of linguistic processes, and yet one which is not enmeshed in an unhelpful quasi-psychological theory. And further, Hjelmslev is at least attempting to describe some linguistically significant facts. Nevertheless, his debt to Guillaume is undeniable.

As far as we are concerned, however, it is not Hjelmslev but Paul Christophersen who is the most important linguist to owe some debt to the Guillaumiste tradition.³ In all his works Guillaume is concerned solely with French, paying little attention to other languages; but in Christophersen (1939) our attention is drawn to the description of English, and the theory undergoes several basic, and, as Christophersen (1939: 66-67) states, necessary changes. The most important of

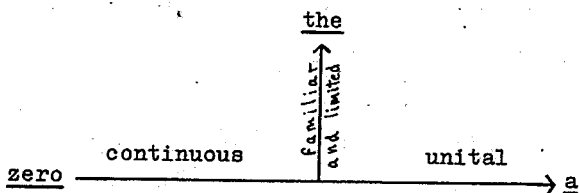
³ As will be seen, Christophersen's work goes far beyond Guillaume's both in merit and in its implications for a linguistic theory of the 'articles'. Thus it is somewhat unfair to describe him as a follower of Guillaume. At the same time, however, we have to recognise that Christophersen's theories rest to some extent on the prior existence of Guillaume's work, and so we may say that Christophersen is both within and beyond the Guillaumiste tradition.

of these concern 'continue' words and plurals, and the use of the 'definite article'. 'Continue' is Christophersen's term for an uncountable noun such as whiteness, and the problem here is that whereas French would use a 'partitive article' with continues and plurals, English uses no type of 'article' at all. Since Guillaume's theory, as it stands, is concerned only with the description of French, as we have said, he does not attempt to explain this fact. With regard to the problems caused by the differences between le and the, Christophersen (1939:69) suggests that the element of 'familiarity' is a rather more prominent feature of English than of French usage. 'Familiarity' is explained as follows (1939:72):

"Let us for the sake of convenience take a singular unit-word. The article the brings it about that to the potential meaning (the idea) of the word is attached a certain association with previously acquired knowledge by which it can be inferred that only one definite individual is meant. That is what is understood by familiarity."

Christophersen ascribes the non-occurrence of a with continues to the 'unital' characteristics of the 'indefinite article', that is, to its individualising function, in terms of Kruisinga (1932a). Taking into account the fact that the has a certain primacy over a -

where for some reason both might be expected to occur only the in fact does, compare here the remarks of Perlmutter (1970:240-46) and see too our own remarks in Chapter 11 - Christophersen is able to portray his system in terms of the following diagram (1939:76):



Now we are able to see that the criticisms which were made above concerning Guillaume's theories had to be, as they were, somewhat tempered by the fact that his theory did lead the way to a more systematic account of the 'articles'. For there can be no doubt that Christophersen has given a reasonable description of some of the major aspects of English 'article' usage in notionalist terms, without greatly deviating from what Guillaume proposed. Nevertheless, it is still necessary to make a number of critical remarks. The emphasis is

entirely on semantic theory,⁴ and the syntactic considerations which might both clarify the semantics and enable the 'articles' to be fitted into a more complete determiner system are almost wholly ignored. There can be no doubt that semantic facts are vital to linguistic studies, cf. again note 4, nor can there be any doubt that notionalist studies such as that by Christophersen often illuminate very fine distinctions in meaning; but the lack of formalism and any coherent syntactic theory are significant weaknesses. In their place is a reliance on rather vague psychologisms and philosophical posits which are unacceptable as a priori statements. A further example of this same tendency is seen in Christophersen's explanation of why proper names generally lack accompanying 'articles' (1939:65):

"A common name is only an idea with potential realisations; the idea itself is abstract, the realisations are concrete. A proper

⁴ Indeed, Toussaint (1967:95) says that Guillaume's aim was to "réduire la syntaxe à la sémantique". This, of course, is comparable to the aims expressed by Lakoff (1971c:267ff.), with much of which we are in sympathy. Our criticism here, therefore, is only that too little attention is paid to syntactic facts such as distributional regularities, and thus is not directed against the claim that it is semantics which is of primary importance.

name has no idea; it denotes only one definite individual and is therefore always concrete. Now, if we accept Guillaume's theory of the article as the connecting link between abstract and concrete, it is clear that proper names need no article."

We are entitled to ask what the linguistic significance of such a statement is, since it is far from obvious; yet in the theories of the Guillaumiste tradition no such clarification is, or can be, given.

In principle, Jespersen (1949)⁵ follows Christophersen's proposals, but he makes one interesting theoretical modification: this is, that he elevates the principle of 'familiarity' to a rather higher status, using it to explain, for example, why proper names, kinship terms, etc. do not normally collocate with the 'definite article'. Jespersen claims that the is used to mark a particular noun as 'familiar', and therefore he can say

⁵ Unfortunately this part of Jespersen's work had to be completed, by N. Haislund, after Jespersen's death, and so it would be dangerous to assign all the views expressed in it to Jespersen. Nevertheless, there is good evidence to show that, in principle, these views can be taken as the ones that Jespersen himself would have expressed, cf. the preface to Jespersen (1949) and Bodelsen (1949).

(1949:417-18) that it is because the 'familiarity' of, for example, kinship terms, is:

"so complete that no article (determinative) is needed."

If we refer back to the notion of deixis, with which the is certainly associated, then we can see that it is at least plausible that terms such as kinship terms will be so familiar to the hearer (or will be assumed to be so familiar) that they will need no kind of deictic, if that is what the is, to point out the referent. However, as will be seen later, Jespersen's explanation is on less secure foundations when dealing with proper nouns as opposed to kinship terms, although it is undoubtedly more acceptable than the explanation which we quoted from Christophersen (1939:65) above. Even so, it is difficult to see how this explanation might be formalised. With regard to proper nouns, the problem is that it is difficult to explain the lack of 'article' in, say, (1) as due to 'familiarity', even as defined by Christophersen:⁶

(2.1) Henry Kissinger is the power behind
the throne

Further, it would certainly seem to be the case that 'familiarity' is not the reason for the lack of the with

⁶ References to examples within the same chapter omit the chapter number; references to examples from another chapter include the relevant chapter number.

vocatives, as Jespersen (1949:418) maintains; the reason is rather different, cf. Thorne (1966) for its explanation. Despite these criticisms, we can agree that Jespersen has made significant modifications to the original Guillaumiste theory.

In the discussion of a in Jespersen (1949) the most important point to note is that here is yet another linguist who rejects the term 'indefinite' (1949:420):

"The term 'indefinite article' is not very felicitous, as this article actually refers to a definite item, even if it is not made known which member of the class is mentioned. The indefinite article is thus different from any, which does not refer to a definite item (known or unknown), but to some one among all items within its class, no matter which."

To exemplify, in:

(2.2) I bought a car yesterday
car is nothing if not definite, for I, the speaker, know, perhaps because this is an action in the past, cf. §2.3, exactly which car I bought. You, the hearer, will not know this, or are assumed (by me) not to know this, but that is hardly a reasonable criterion for lack of 'definiteness'. To say that the car is 'indefinite' is to make a mockery of that ill-used term. At the very least we must make some revision of our terminology, in

order to give a more felicitous description of the linguistic evidence. Further evidence of the inappropriateness of 'definite' is seen in opaque contexts, cf. Quine (1960:141-56). In, for example:

- (2.3) John wants to marry the girl with the
 most money

it is quite probable that neither the speaker nor the hearer will yet be in a position to point out the referent of girl. The only indicates that it will eventually be possible, in theory, for both of them to do so.

Apart from these matters, the main interest in Jespersen for us must lie not in his treatment of quantifiers, cf. (1949:620-22), but in his discussion of number. In Jespersen (1914) there is both a long and an illuminating discussion and an exemplification of the syntactic features of count and mass nouns, of how mass nouns may change both their meaning and their countability simultaneously, e.g., noncount paper in (4), count paper in (5):

- (2.4) All the essays were written on poor
 quality paper

- (2.5) Bill wrote six papers on Old Persian
 last year

In this discussion there is also mention of the 'dummy' words which, following Ianucci (1952), we shall term counters. The function of counters is solely to make a noncount noun countable, without in any way necessarily

adding to the semantic content of the sentence. Thus, while:

(2.6) The victim had had pneumonia
is grammatical, (7) is not:

(2.7) *The victim had had several pneumonia
The reason for this is that several cooccurs only with count nouns, and, of course, pneumonia is an uncountable mass noun. Therefore the 'dummy' counter, attack, plus of, is introduced into the object noun phrase in order to provide an acceptable alternative to (7), without altering the semantic content of the sentence:

(2.8) The victim had had several attacks of
pneumonia

We shall return to this topic, and attempt a more formal solution of it, in §6.3.

In his reviews of Christophersen (1939) and Jespersen (1949), Bodelsen (1939, 1949) suggests that the concept of 'familiarity and substance' (the latter is equivalent to Hjelmslev's 'concretisation') is insufficient to provide an adequate description of the use of the. Also needed, suggests Bodelsen, is the concept of 'quantitative existence'. Bodelsen gives the following explanation of this concept (1939:235-36):

"When we say gold is heavy, we are, of course, speaking about something material, but, as Christophersen himself points out, we are not thinking of gold as something quantitative,

but as something qualitative, and this is the reason why we have the zero-article here. If, on the other hand, we say the gold that is stored in banks, then we conceive the gold as something that exists quantitatively."

But of course, if it is accepted, as has been suggested above, that the has a strong deictic element, then naturally there will be a necessity for 'quantitative existence', a need, in other words, for something to point to. Further, it should be made clear that Bodelsen is not entirely fair to Christophersen, as can be seen from the following quotation (Christophersen, 1939: 71):

"The has as its special function the marking of familiarity, while a is purely the mark of unity. This theory can tell us why generic continue words and plurals have no article. Their very generality and the vagueness of their quantitative delimitation precludes familiarity, or to put it conversely: familiarity presupposes sharp and precise limits ..."

In conclusion, it is interesting to consider further some of the remarks quoted above from Bodelsen (1939). There it can be observed that he considers the contrast between (9) and (10):

(2.9) Gold is heavy

(2.10) The gold that is stored in banks is
heavy

is basically that in (9) gold is thought of qualitatively, in (10) quantitatively. The point that the is in some way performing the task of a quantifier is an interesting one, and one which we shall inspect more closely in Chapter 12, but it is difficult to accept Bodelsen's remarks concerning (9). By far the simplest explanation of why (9) has neither the nor a is that gold is an uncountable noun, and thus can never collocate with a; further, the is, in one sense, optional, but see our remarks in §9.3. This syntactic explanation of the grammaticality of (9) as opposed to (11) and alongside (12):

(2.11) *Horse has four legs

(2.12) A horse has four legs

seems quite sufficient, and there is surely no necessity for unverifiable psychologisms. They may be thought of as relics from the original Guillaumiste theory, which hide the fact that the whole tradition, although springing from what appeared to be barren ground, has developed a semantic theory of the 'articles' which is of more than transient interest. Nevertheless, it cannot be denied that the lack of interest in criteria which are purely syntactic detracts from the value of the theory, as does the lack of interest which is apparent with

regard to the 'non-article', more purely quantifier, members of the determiner systems.

2.3 Other notionalists

Of the notionalist works outside the Guillaumiste tradition, perhaps the most notable is the study by Collinson (1937), which, as we might expect from the quotation from Jespersen (1924), above, is specifically concerned with the underlying structures of language. The most important of his remarks are those concerning the behaviour of a and any in English. Just as Kruisinga (1932a) claimed that there are two functions of the 'article' a, so too does Collinson; these functions Collinson calls 'alternative' and 'instantive' (1937: 35). 'Alternative' is the equivalent of Kruisinga's 'classifying', 'instantive' is similar to 'individualising'. To adapt Collinson's own examples, (13) shows the 'alternative' use of a, whereas (14) shows the 'instantive' use:

(2.13) The chimney is filthy; we need a sweep

(2.14) As dusk fell, the travellers reached
a village

In (13) no particular sweep is thought of, whereas in (14) there is a particular referent, although one which is unknown to the reader or (perhaps) hearer.

In an attempt to give an explanation of this difference, it is constructive to compare (2). From this it is clear that the tense of the verb is important, or, perhaps, the need in (13) is related to modals referring to the future, cf. Anderson (1971d). The 'alternative' use of a, therefore, must be syntactically restricted, and, at the very least, is dependent upon the structure of the verb phrase. The ambiguity of a, which other writers have also noticed, cf. Robbins (1968:101-2), Zandvoort (1957:125) and below, suggests that while it may be correct to think of a as the proclitic form of one, as does Perlmutter (1970), there are still difficulties to be overcome. On the other hand, we must ask whether the ambiguity resides in a, or in the noun phrases, a sweep, a village, etc., as a whole. An attempt to determine the place of ambiguity will be part of the function of Chapter 11.

From the discussion of a, it is useful to move on to consider Collinson's remarks about any. Collinson claims that any is highly restricted in its cooccurrence with past tense forms (1937:91):

"We ... say 'There was not anyone who smiled' emphasizing the idea that one could pick on anyone one liked and not find a smiler. We do not, however, say either 'Anyone refrained from smiling' or 'Anyone did not smile' but this is due to the fact that we dis-

countenance the use of any as the subject of a definite occurrence in the past."

In fact, Collinson does not portray the state of affairs as exactly as he might, for there is an exception to the statement that any does not occur with past tense forms unless it is preceded by an interrogative or negative element.⁷ The exception is that any may occur if there is a restrictive relative clause dependent upon it, or there is a qualifying adjective which, in transformational terms, is derived from such a relative clause. This does not hold for nonrestrictive relative clauses:

- (2.15) a Any pupil who knew the answer was thrashed by McCoakumchild
- b Any promising pupil was instructed in the principles of Utilitarianism
- c *Any pupil, who knew the answer, was thrashed by McCoakumchild

Where any is preceded by a negative element, as in:

(2.16) I didn't read any books
then it may be possible to explain the quantifier as

⁷ We must also note the acceptability of any in sentences such as:

(i) Any indiscipline was instantly punished
But to state this as an exception quite different from that named below might be a loss of generalisation.
See, however, the discussion in §10.2.

being in some sense derivable from, or parallel to, not + some, cf. Klima (1964:279) and §3.3. However, the other occurrences of any seem to be rather more difficult to explain, especially those such as (15a) which involve a relative clause. There seems at present to be no way to derive them from some in these instances. However, see the further discussions in §3.3 and, especially, §10.2, for a resolution of the difficulties.

One essential point which is often ignored is emphasised by McIntosh (1968); for the to be used appropriately the referent of the noun in question must be known to both speaker and hearer, or the speaker must assume that this is so. Where the assumption is mistaken, then there can be a breakdown in communication, cf. §4.1. Although this point may seem elementary, it is one which is not always properly understood; thus Jespersen (1949: 79) writes:

"The definite article plus a substantive in the singular denotes one individual (supposed to be) more or less familiar to the speaker or writer: some image or notion of the thing or person denoted by the substantive is (supposed to be) already found in the consciousness of the speaker or writer before he makes the statement."

Jespersen ignores the problem of the hearer in this quotation, and McIntosh is correct in insisting (1968:7)

that use of the implies that "you (as well as I) know which one(s)", for this clearly demonstrates that the shows that both speaker and hearer are understood to have full knowledge of the referent of the cooccurrent noun.

Finally, the following is also crucial for an understanding of the uses of the and a. McIntosh (1968: 17) notes that to the question:

(2.17) Have you ever seen an axotol?

one may well reply:

(2.18) I have only once seen an axotol

On the other hand, it would be incorrect to reply:

(2.19) I have only once seen the axotol

although in other contexts that sentence is completely grammatical. The reason for this would appear to be that in (17) an is 'classifying' (or 'alternative'), and that this precludes an immediately consequent use of the with the same noun. On the other hand, the an in (18) is 'individualising' (or 'instantive') and therefore one may, in fact must, use the at the next occurrence of axotol with the same referent, as in, for example:

(2.20) I have only once seen an axotol;

the axotol was very beautiful

McIntosh's argument on this point strongly reinforces the earlier point that it will be very difficult to account for all instances of a as merely proclitic

variants of one, see above. The reason for this difficulty is, of course, that there are clear semantic and syntactic differences between an axotol in (17) and the apparently identical noun phrase in (18). Jackendoff (1969:233) has made an attempt to get round this problem, but see §6.5 for some critical discussion of his solution. We shall eventually see that it will be necessary to introduce a semi-logical concept of 'scope' if this problem is to be solved; how far, however, a transformational theory restricted to the sentence level can cope with this is a matter for some doubt.

When we discussed the Guillaumiste tradition in §2.2, we observed that one inadequacy of the proposals stemming from that tradition was that the grammarians concerned were almost exclusively interested in some contrast between the and a. In this latter part of the chapter, however, we have discussed two papers which deal with the ambiguity of a (if that is indeed where the ambiguity lies) and which lead towards a discussion of the quantifiers in general. They are interesting because they suggest that a, at least, is in some ways much more like a quantifier than an 'article'; in particular, some elements of the so-called 'indefinite article' are also associated with the behaviour of any. The consequences which this has for any analysis of the determiner and quantifier systems of English will be more extensively discussed in Part IV. But at present

we shall now turn our attention to some analyses of determiners and quantifiers which are totally different in approach from those which we have discussed so far.

Chapter 3

Structuralist models

3.1 Structuralism

By structuralist models of the determiner and
are quantifier systems, ~~it is~~ meant those studies which have
as their basis the attempt at a classificatory descrip-
tion of the elements present in the surface structure of
a given language, although the work may extend much
further. In this the influence of Leonard Bloomfield is
preeminent, for it was he who, with his assumption that
science was necessarily behaviouristic, determined that
such a taxonomic approach was essential. Therefore, in
marked contrast to the works discussed in the previous
chapters, we shall find here little resort to meaning
and certainly no attempts at a psychological evaluation
of the functions of the various members of these systems.
We are now at the opposite pole of the mentalist -
behaviourist axis to Guillaume.

But this chapter makes one theoretical claim which
is not commonly observed and will seem surprising; it
will therefore be necessary to state that claim now and
then attempt some immediate justification of it. This
claim is that no major theoretical break occurs between
the structuralist theories associated with Bloomfield
and the so-called 'Bloomfieldians' on the one hand and

the transformationalist theories outlined in Chomsky (1957) on the other. Rather, the break is claimed to occur between these two together as opposed to the various types of transformational grammar which stem from such works as Katz and Postal (1964) and Chomsky (1965). In other words, the chronological placement of the break is here placed at about one decade later than is most often assumed.

Our assumption can be criticised on several grounds, of which we shall discuss three. The first, but the weakest, is that this underestimates the theoretical contributions of Noam Chomsky. Now whilst it would be totally misleading not to recognise Chomsky's contributions as major, to divide linguistic work into compartments on the basis of who wrote such-and-such is an unacceptable personalisation of the issues. It is both more principled and more just to the scholars concerned to evaluate their work on quite impersonal grounds, for then we can see the true diversity of the work of each writer. Bloomfield is far from being the most rigid Bloomfieldian and Chomsky is not the most dogmatic of Chomskyians.

The second ground for criticism is that the division suggested above ignores the key concepts of deep and surface structure: structuralist grammarians, it is claimed, are interested only in surface structure; on

the other hand, transformationalists extend their interests to deep structure. Although this is to a large extent correct, it is not entirely so, for non- (indeed, anti-) transformationalists such as Hall (1964) have been quite happy to use the concept. While deep structure may be, or may originally have been, a necessary concept in transformational theory, it is neither sufficient nor exclusive. Furthermore, there is the empirical fact that in early transformational work the deep structures proposed for determiners and quantifiers are not significantly different from the surface structures. Probably this is in part due to the relative lack of attention paid to such items until the middle 1960's, but it can also be seen as due in part to the still-strong influence of surface-based structuralism.

The final point concerns the underlying contrast between mentalism and behaviourism. Transformational grammar has been, from the beginning, mentalistic, while structuralist grammar has always been associated with behaviourist theories. Should not such a distinct conflict be reflected in our theoretical divisions? The answer is that of course it should be, if that conflict is directly reflected in the works and analyses which we shall discuss. But that condition is hardly fulfilled, for in the questions with which we are concerned there can be no great theoretical divisions apparent when the kinds of solution offered are broadly similar, as is the case.

We have already remarked that in Bloomfieldian theory there is a strong distinction between syntax and semantics, and that this is a defining characteristic of the approach. Now what is important to note here is that for early transformational grammar exactly the same is true. Consider, for example, the following remarks from Chomsky (1957:101):

"It seems clear, then, that undeniable, though only imperfect correspondences hold between formal and semantic features in language. The fact that the correspondences are so inexact suggests that meaning will be relatively useless as a basis for grammatical description."

This statement has far more in common with the standard structuralist position than it has with the position adopted by Chomsky (1965:77):

"It goes without saying ... that purely semantic or purely syntactic considerations may not provide the answer in some particular case. In fact, it should not be taken for granted, necessarily, that syntactic and semantic considerations can be sharply distinguished."

The reason for such a change of heart may have been purely practical, as claimed by Katz and Postal (1964: 2-4), or it may be significant of a deeper change

affecting fundamental theoretical disposition, but for our purposes a decision between the two is hardly necessary. Although Chomsky (1957) did propose a great many striking theoretical changes, the immediate effect of that work on actual descriptions of determiners and quantifiers was, for the reasons we have described above, minimal. To put the matter in a crude chronological fashion, a transformational description of determiners written in 1960 looks (and is) a lot more like its structuralist counterpart of 1950 than its transformationalist counterpart of 1970. It is precisely that kind of 'brute fact' which must determine our divisions.

3.2 Bloomfield and others

Although Bloomfield (1935) does use the term determiner, his use of it is not precisely equivalent to the one which we have used here. Much closer to the present use of determiner is Bloomfield's term 'limiting adjective'. These adjectives he divides into two classes, 'determiners' and 'numeratives'. The former includes all that we here regard as determiners or quantifiers, with the exclusion of all, both, many, such, few, very

and the numerals, which are classed as 'numeratives'.¹ The reasoning behind Bloomfield's division would appear to be based upon the fact that singular count nouns obligatorily cooccur with a determiner, cf. Bloomfield (1935:203); however, on consideration such reasoning must be judged to be inappropriate.

If we look at Bloomfield's definitions more carefully, it will be noted that the only defining characteristic of the 'numeratives' is that they all require a collocating plural or mass noun, with the exception of one, which, of course, can only collocate with singular count nouns. Now this is an extremely crude reason for the separation into two classes. Consider, for example, all, every and each. Since the latter two would appear, at first sight, to collocate with singular count nouns only, they are classed as 'determiners', in contrast to the 'numerative' all. But in fact there is good reason to suppose that it is the distributive force of each and every which makes the collocating noun singular. If one were allowed, against all Bloomfieldian principles, a

¹ The status of some of these items, e.g., such, is obscure, for questions apart from the grammar of determiners may be involved. In other words, it is not necessarily the case that every one of Bloomfield's 'numeratives' is a determiner within the context of the present study.

level of underlying structure, it would appear most probable that in all cases of each/every + noun that the noun would be originally plural and only change to singular for the surface structure realisation (and such late transformations as that for concord). Evidence for this can be found in the fact that each can occur following a plural noun, and that that is semantically very similar to each preceding a singular noun:

(3.1) The students each had their own copy
of Aspects

(3.2) Each student had his own copy of Aspects
Even given the absence of the in (2) as opposed to (1), it is clear that an adequate description of each must take into account the fact that plural nouns do not exclude some collocations with each (and with every, although in that case the situation is more complex), and this Bloomfield's classification fails to do.

Another problem which we encounter with regard to Bloomfield's division is connected with the 'class-cleavage' of one, cf. Bloomfield (1935:206). According to the remarks there, one may either be a 'numerative', as in (3a), or a 'determiner', as in (3b):²

² We ignore here the use of the 'prop-word' one, which, Bloomfield (1935:204) correctly observes, belongs to a rather different grammatical category.

(3.3) a My one book was burned

b One book was burned

The reason for this 'class-cleavage' is that Bloomfield (1935:203) claims that with certain nouns, primarily countable nouns such as book, a 'determiner' is always required, cf. above. Therefore, in (3b) one must be a 'determiner'. But Bloomfield would also seem to believe that 'determiners' must occur exclusively of one another. Thus, since in (3a) my is the 'determiner', one must be something else, namely a 'numerative'. This implies that two instances of the apparently identical item, here one, but any numeral and several quantifiers would fit, both of which have the same meaning and the same phonological form, must be assigned to different syntactic classes. This is so clearly an incorrect conclusion that it is difficult to see how it could have arisen except as the product of a fundamentally mistaken taxonomic approach. As we shall see in Chapter 8, it is indeed the case that quantifiers (including one) which occur in postdeterminer position, as in (3a), require an analysis which is somewhat different from that for quantifiers occurring elsewhere, but the underlying semantic representation remain constant. This latter point is quite ignored by the 'class-cleavage' hypothesis.

The reason why the taxonomy is fundamentally mistaken is one to which we shall have to return several

times, since it is due to an assumption which is common to most structuralist grammars. Bloomfield does not permit recourse either to meaning or to some underlying structure in order to determine grammatical classification, thus completely contrasting with the notionalists of Chapter 2. Now if one makes such an a priori and illinguistic decision as that, then one has to accept that empirical facts may sharply contradict it. This is precisely what happens in the case of the 'class-cleavage' of one. If we wish to account for all the facets of the linguistic behaviour of one, we shall have to include all the linguistic information, including semantic information, as is pointed out above. To segregate 'limiting adjectives' on the basis of their cooccurrence with singular count nouns only gives a highly restricted syntactic tautology.

What would, perhaps, be a much more useful division would result from distinguishing between those items which are most closely connected with an underlying number system, i.e., all Bloomfield's 'numeratives' together with, in all their occurrences, each, every, some, etc., and those which are most closely connected with a deictic system, i.e., the, this, that, possessives, etc. This, of course, approximates to the distinction already made between quantifiers and deictics. It should be observed that this division appeals to the two criteria which Bloomfield excludes, namely semantic

and underlying structure. That the division appeals more strongly to our linguistic intuitions is surely some empirical evidence against Bloomfield's restrictions on grammatical theory; but whether or not that precise division is correct or essential we must wait until a later chapter to decide.

Bloomfield again subdivides his class of 'determiners', in the following manner (1935:203):

"A number of features subdivides the determiners into two classes, definite and indefinite. Of these features we shall mention only one: a definite determiner can be preceded by the numerative all (as in all the water) but an indefinite determiner (as some, in some water) cannot."

This, it seems to me, is a necessary corollary of the previous division Bloomfield makes, and it is no less mistaken. Upon examination it will be seen that his 'indefinite determiners' are precisely those 'determiners' which, together with the 'numeratives', should be classed as quantifiers, except for the interrogatives, which are a different matter again. That these 'indefinites' are quantifiers is, for the moment, an adequate explanation of why they do not cooccur with all in the way Bloomfield describes (although there are exceptions, cf. Chapter 4 and §10.4), and yet it permits all to occur with deictics other than the, as in:

(3.4) a All those boys

b All this milk

The introduction of the terms 'definite' and 'indefinite' for this subclassification function is, therefore, seen to be superfluous. One's suspicions must be that these terms were introduced in order to describe the two 'articles', that term being itself one which Bloomfield retains but for which he provides no justification.

Perhaps not unexpectedly, Bloomfield made the greatest of contributions to the study of determiners and quantifiers within a strictly Bloomfieldian framework, and later works such as those by Fries (1957) and Hockett (1958) do not add to our knowledge of the syntax of these items in any significant manner. Even an extended monograph such as Yotsukura (1970) shows no important theoretical advance. Indeed, it may be considered a regression, since it deals exclusively with the surface structure occurrences of the 'articles', including unstressed some (s'm). Concerned as Yotsukura mainly is with grammatical collocations of these items with countable and uncountable nouns, she is open to the same criticisms as we have applied to Bloomfield above. Also, of course, Yotsukura has restricted her study to a grammatical category - the 'articles' - which we have already shown in Chapters 1 and 2 to be of dubious value. Therefore if we wish to continue our search for a development of structuralist, although not necessarily

Bloomfieldian, theory of determiners, we must cross the Atlantic so that we can consider the comments of Strang (1962). In that work we find that determiners are once again treated as a major category, for the term as used by Strang is equivalent to Bloomfield's 'limiting adjectives'. However, there is a subdivision into three minor categories: (i) 'articles'; (ii) determiner-pronouns; and (iii) noun-phrase initiators. The second of these subdivisions is again subdivided, this time into two groups: variables and invariables (Strang, 1962:108-16). In fact, Strang makes a third subdivision of 'determiner-pronouns', which I have, for present purposes, treated as part of the 'invariable' group; this subdivision includes numerals, few, little, etc. It does not seem to me that this abbreviation does undue violence to her account, but cf. Strang (1962:114-15).

The 'articles' are given their special prominence because they do not occur alone in a noun phrase; this is a criterion which we have already discussed and already found wanting, cf. the discussion of Maetzner (1874a) in §1.5. The reasons given there for its rejection hold equally for Strang's classification and need not be dealt with again. Strang notes two uses of the 'definite article', 'particularising' and 'non-particularising', a distinction clearly designed to deal with generics, which are alone in belonging to the latter group. Of the various types of 'particularising' the

which Strang mentions, special note should be made of 'subsequent specification', for this notion is one which Hill (1966), see below, discusses in detail. However, it might be pointed out now that the sentence which Strang uses to explain this notion (1962:109):

(3.5) The passage I have quoted is not a happy choice, since it is easy enough to present a plausible case for this being an instance of the 'before mentioned' usage, i.e., where there must have been a previous mention of the passage or of a noun with a similar meaning and the same referent. Even more plausibly one might suggest that (5) is an example of the coming from the situational environment, see below and Strang (1962:109-10). In such a case one might claim that there is non-verbal previous mention, and that the therefore refers to some event which both speaker and hearer have just witnessed. The extent to which the linguist ought to be involved in such paralinguistic matters is highly debatable, and it is a point which I shall in large measure attempt to avoid as being of no direct interest at the moment. However see §12.3 for an attempt at a resolution of the problem as far as it concerns the.

In Strang's discussion of the 'indefinite article' there is some obscurity, for it is not at all clear whether or not she recognises the possible ambiguity of a, discussed at length above. One could claim that she

does see this ambiguity, but it is difficult to claim that she attaches any importance to the fact; this, of course, may be an eventual advantage. In common with many other grammarians, cf. Jespersen (1949:403-4), Sørensen (1958:83) and Yotsukura (1970:50), Strang recognises a 'zero article'. Whether or not this is correct will be most extensively discussed in Chapter 11; let us only remark for the moment that the prime motive behind this postulation would seem to be, in all cases, an a priori assumption that in English all nouns must be accompanied by an 'article' or some other determiner, and that this assumption has not been satisfactorily proven or shown to be desirable.

One innovation which Strang makes in her discussion of the 'articles' is the use of the term 'negative article' to describe no (1962:111-12). The reason for this is that no would appear to form, with the and a, the third part of a mutually exclusive syntactic system. However this is unsatisfactory. No has no semantic relation whatsoever to the, and to assume that it has can easily lead one into logical fallacies, cf. Geach (1968:11ff.). A not totally dissimilar situation arises if one considers no to be merely the negative form of a. Allowing for the present that a is derivable from one, what would prevent us from then claiming that no was derivable from not one, for this appears to be the import of Strang's classification? It is true that in

some cases this appears to be desirable, for we find:

(3.6) a Not one mountaineer was lost

b No mountaineer was lost

At first sight the difference between the two sentences seems to be one of emphasis, and this could be handled by rules similar to those presented by Perlmutter (1970) to account for one - a correspondences. But that analysis escapes the generalisation pointed out by Jespersen (1940:457) and elaborated upon by Steven Smith (1972), that in the type of structure exemplified by not one, the negation simply means "less than". That less than one equals none is a mathematical, not a linguistic, equation, nor is the linguist responsible for the fact that mountaineers are counted only in whole integers. Furthermore, there are instances where no is grammatical and not one is not:

(3.7) a *Not one milk was spilt

b No milk was spilt

Finally, it may be observed that no one does not mean the same as no in every case:

(3.8) a No one boy can kill Goliath

b No boy can kill Goliath

It therefore appears misleading to compare no directly with a, and this strongly suggests that the proposed

analysis of no is most probably inadequate.³ The problem could be more easily resolved if the 'article' were dispensed with as a separate class, for then no and a may, perhaps, be both analysable as quantifiers and the relationship between them should then be as easily classified as should the lack of relation between the and no.

To turn our attention to possessive pronouns, these Strang calls 'genitive articles'; again this is unsatisfactory, but this time more reasonable. However, within the limits of a surface structure analysis it is difficult to go further and it is only within a discussion of the underlying properties of possessives that a classification of their determiner-like functions can be made. Let us merely note that forms in Italian such as:

(3.9) ~~Il mio vestito~~ ("my suit")

may provide clues to their derivation and status, cf. Bloomfield (1935:203).

The 'variable determiner-pronouns' of Strang (1962) are the demonstratives, and the 'invariable determiner-pronouns' are the other determiners and quantifiers with

³ This is notwithstanding the fact that no is diachronically derivable from Old English nān, a compound form of ne + ān "not" + "one". Cf. the brief remarks in §6.5 on other parallel word formations.

the exceptions of 'noun-phrase initiators' and 'articles'. The criterion for this division is the relationship between number and determiner, and, as noted above, is similar to the distinction made here between deictics and quantifiers, although Strang, I would claim, relies to a greater extent on merely surface characteristics. Strang's final category is 'noun-phrase initiator'. The most important members of this category are all, both and half, cf. Strang (1962:116), and the criterion for membership of the class is occurrence immediately before the. That this is a doubtful, and even unilluminating, basis for a category distinction we shall attempt to demonstrate later, cf. Chapter 4. In any event, it would seem to be a matter of insufficient syntactic importance to justify a sharp distinction between them and the 'determiner-pronouns'.

Smith (1963) is primarily a statistical study of the 'articles', for which compare Yotsukura (1970), and as such contributes relatively little to the more theoretical aspects of their syntactic description. However, one point of interest is the use of the terms cataphora and ecphora to describe reference forward and situational reference respectively, in addition to the well-established anaphora, or reference backward, as for example is the case with the in:

(3.10) I met a man; the man ...

Cataphora is used to describe those instances of the which Smith considers are due to a restrictive clause or

adjunct which follows the cooccurrent noun. Two criticisms might be made here. Firstly, the notion of "following" as used by Smith is purely a surface one. In underlying structure there is no reason to suppose that such is the case. Secondly, some of the examples of cataphora which Smith gives are in themselves unconvincing, for example (1963:15):

(3.11) "... assume that the vehicles whizzing
by would ..."

for such examples of the usage of the do not appear to demand as a source the following restrictive adjunct or clause. The question is an extremely thorny one, cf. Huddleston (1971:212-15) for just one aspect of this, and as a result one must be dubious about the worth of Smith's statistical statements which purport to show that the cataphoric use of the is the most common in the text under analysis. See, however, the remarks on Robbins (1968) in §3.4, where the question is once more discussed. And even if Smith's examples of cataphora are not always convincing it may well be the case that it will be extremely useful, and perhaps even necessary, to describe (and analyse) certain instances of the as cataphoric; this is a question to which we shall return in Chapter 12. It would appear that many cases of the can only be accounted for in terms of their situational context. The most well-known of these are references to the sun, the moon, etc. where the use of the can be

explained in terms of the 'situational context of the universe'. Clearly this has some connection with the notion of 'familiarity' espoused by Christophersen (1939) and Jespersen (1949), and discussed in Chapter 2, but for such occurrences of the Smith uses the term 'ecphora' (1963:17). We thus have a tripartite description of the, and in our later discussions we shall consider how syntactically valid this largely semantic classification may be.

We must now return across the Atlantic to discuss a paper by Hill (1966). This paper is not easy to place theoretically, since it partly relies on taxonomic criteria and partly on transformational theory. It might be preferable, therefore, to consider it in the following section (on early transformational grammar), but, perhaps a little unjustifiably, I shall discuss it here. The main reason for doing so is that Hill acknowledges a great debt to Strang (1962), and such an umbilical cord is best not severed. Hill's paper deals only with the two 'articles', the and a, and thus has all the deficiencies which have been noted in other similar studies. There is, for example, no mention of the demonstratives or of the relation of a to one.

Hill's study is in two parts: the first is a rapid historical survey of major contributions to the theory of the 'articles', which often offers very interesting comments; much of what he says must be agreed with, but

he does seem to overrate Sweet (1898), which only glances at the 'articles' and offers, for example, no discussion of generics. One of the best features of this survey is Hill's dislike of the way the term 'definite' is currently applied. Thus he quotes Roberts (1964:12):

"The gives the noun a definite meaning, specifying a particular one or a particular group. A and some do not do this."

Hill comments (1966:222):

"As with earlier descriptions which insist on definiteness or particularity, I find that a dog bit me is quite as definite, particular and singular, as the sentence would be with the other article."

This is reasonable enough as far as it goes, but since it deals only with the position of the speaker it does not go very far. For the hearer there is a semantic difference which Hill ignores, and this detracts from the value of his statement.

Hill then goes on to analyse the meaning and status of the 'articles' for himself. In fact, though, he barely discusses a, and his remarks of interest are confined to the. He regards the as having two sources: (i) second mention; (ii) proximity (Hill, 1966:225, 228-29). Proximity is equivalent to Smith's ecphora, above, and while it is a reasonably useful and clear term, it must be stated that it only defines a problem; in no way

does it solve one. Hill considers 'second mention' to be the fundamental source of the, and its status is twofold. Firstly, there is simple anaphora, see example (10), above; secondly, where there is no such anaphora, the first mention is presumed to be sited in a defining (restrictive) relative clause or adjunct, cf. the remarks above on Smith's (1963) use of cataphora. However, Hill points out that this situation is not simple, since either 'article' can appear with such a construction, as in:

(3.12) A man who refuses alcohol is a teetotaller

(3.13) The man who refuses alcohol is a teetotaller

But by using the notion of 'second mention' as obligatorily demanding the, Hill accounts for (13) and (14) in the following manner (1966:226):

"I shall set up the source sentences for the first example given above as

A man is a teetotaller. The man
refuses alcohol

In the process of embedding the second sentence into the first, the second mention form (the man) is replaced by who, and the first mention form remains. For the second example I shall set up the hypothetical source sentence as

A man refuses alcohol. The man is a teetotaller

The process of embedding inserts the first sentence into the second sentence, replacing a man with who. Thus it is the second mention form which remains."

The 'second mention' derivation is certainly an ingenious device for accounting for large numbers of occurrences of the. However there are several questions to be asked, notably those concerned with the lack of evidence for such different derivations. The problem would seem to be that one type of the, the anaphoric one, has been elevated to a level where it is unable to bear the weight of explanation required, and there is a resultant ad hoc explanation which does not appear to have any syntactic motivation. Ingenious though it may be, Hill's account is quite unacceptable.

And there is yet one further point to be made. For me, at least, (12) and (13) do not appear to be typical occurrences of a and the, nor to differ in meaning significantly, since they can both be taken to be generic; in fact, that would be the most usual interpretation of these sentences. The generic quality is removed if we change to the past tense, but with the following results:

(3.14) ?A man who refused alcohol was a teetotaller

(3.15) The man who refused alcohol was a teetotaler

It does not seem to me that Hill is able to account for the less acceptable status of (14). It should be noted, and (14) and (15) provide tests for this, that whereas (12) is always generic, this is not so with (13), to which a non-generic interpretation can be assigned. In such cases the man would appear to be derived not from 'second mention' as a result of the process of embedding advocated by Hill and described above, but either from simple anaphora or what Hill calls 'proximity'. If this is so, then it must be doubtful whether the kind of 'second mention' that we have been discussing is adequate or even necessary. In any event, it can be seen that it fails to account for the ambiguity of (13). We may, then, conclude that there are at least three sources for the, namely, anaphora, 'proximity' and genericness. But in this connection see the discussion of Kruisinga (1932a) in §1.5 above, and of Vendler (1967) and Robbins (1968) in §3.5 below. Furthermore, there can be no doubt that relativisation has some bearing upon the use of the, and perhaps it is this fact which Hill is striving toward; that is a subject to which we shall return more than once.

In concluding this section, we must recognise as the major fault of Bloomfieldian and neo-Bloomfieldian linguistics the unwillingness to make any semantic

pronouncements on determiners and quantifiers. Also, and this almost certainly has its connections with semantics, there is the lack of interest in any underlying semantic phenomena which might help to account for the variations in surface structure. While what appears on the surface ought not to be ignored, neither should the underlying structure be ignored, even if it is not directly observable or testable. It is this latter omission which is at the root of the unfortunate distinctions which all the above grammarians have made, for example, the counter-intuitively sharp distinction between all and every, or Bloomfield's 'class-cleavage' of one.

3.3 Early transformational theory

The earliest transformationalist studies are, quite understandably, concerned primarily with providing a general theoretical exposition and a discussion of various syntactic phenomena which are easily accessible to the transformationalist framework. Since determiners and quantifiers are not, regrettably, to be included amongst such phenomena, it was natural that these studies pay very little attention to their syntax. For example, Chomsky (1957) mentions these items only in the context of a derivation of the from the node T, which itself is one of the obligatory constituents of an NP structure. Similarly, Bach (1964:67, 76), in so far as one can

judge, would appear to regard the constituent structure of NP's to be Article + Noun, where Article would include (at least) the, a and the possessives. That both accounts are totally inadequate cannot be denied, but it is to be expected when we consider that it was not the aim of either work to provide a systematic and complete explanation of the English determiner systems. However, the suspicion that the first studies in transformational grammar were often content to give merely a phrase structure formalisation of previous structuralist accounts is reinforced when we look at rather more comprehensive discussions in a similar theoretical framework, cf. Chomsky (1961:135), and the discussion of similar studies in Jackendoff (1968).

Thus, of the other introductory works which should be considered here, Roberts (1962) and Thomas (1965) barely do more than give PS rules which will generate the types of structures discussed in introductory structuralist handbooks, e.g., Fries (1957), and these two authors pay no more attention to transformational relations between these structures than do their structuralist rivals. However, a later work by Roberts is more interesting, since he states in its preface (1964:vii) that the determiner system and the rules he presents to generate it were outlined to him by Noam Chomsky. Despite such a pedigree the account has its shortcomings, the analysis of which must centre upon the following.

rules (Roberts, 1964:397):

Det	----->	(pre-article) + Art + (Demon) + (number)
pre-article	----->	several of, many of, both of ...
Art	----->	{ Def Nondef }
Def	----->	the
Nondef	----->	{ a some ∅ }
Demon	----->	{ D ₁ D ₂ }

Two of these rules are somewhat misleading. Firstly, but less importantly, Def/Nondef is a contrastive feature used only for pedagogical reasons, Roberts himself preferring the contrast of specific/nonspecific (1964:12). But whether even the latter is the correct contrast is doubtful, since it must be presumed that the is to be regarded as specific and a as nonspecific, and if the feature of specificity is to be employed syntactically surely its usefulness will be rather in separating the two different forms of a which we have mentioned previously and which we can exemplify by:

(3.16) I bought a car yesterday

(3.17) I must buy a car soon

where the latter a might be regarded as nonspecific, the

former one as specific. Indeed, we ought to note that this is very close to the [±specific] feature used in the rather later work of Fillmore (1967). The same feature, moreover, could equally be applied to instances of the, cf. example (2.3).

Secondly, and much more confusingly, the use of the term "Demon" conjures up the notion of demonstrative, an unfortunate connection which Roberts first encourages (1964:30):

"The symbol Demon stands for the word demonstrative, as you may have guessed, and you may know that such words as this and these are demonstratives."

and then attempts to obliterate:

"However Demon here doesn't stand for this or these."

Instead, as can be seen, Demon rewrites as "D₁" or "D₂", which have, according to Roberts, the meanings of nearness and remoteness respectively. Now it should be remembered that Art rewrites as Def or Nondef. Taking the combinations of Art + Demon we find the following permutations:

(3.18) Def + D₁ : Def + D₂
Nondef + D₁ : Nondef + D₂

From the first of these permutations Roberts derives, by 'phonological' rules, this; from the second he derives that. Setting aside the nature of the processes which

Roberts terms 'phonological', for perhaps 'lexical' is a better description of them, but that is unimportant, such an analysis we may consider adequate enough in (the pedagogical) context, considering the fact that nearness vs. remoteness is a commonly accepted description of the contrast between this and that (and not so very far from the 'truth').

From the third and fourth permutation in (18) Roberts derives determiners such as a certain and some, respectively, so that sentences (19a) and (19b) show the contrast $D_1 - D_2$:

(3.19) a A certain man came in

b Some man came in

Not surprisingly, Roberts is less than confident about such derivations. Indeed, he admits (1964:34):

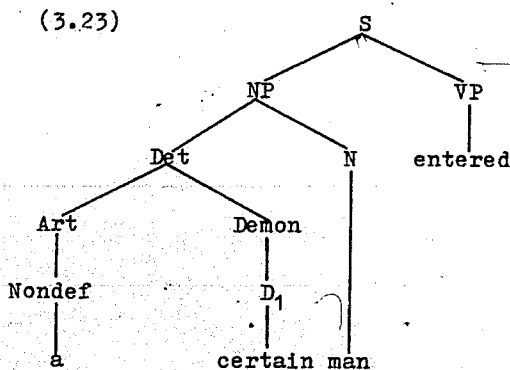
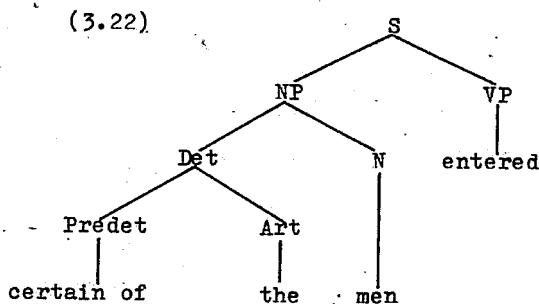
" D_1 and D_2 in combination with Def clearly contain the meanings nearness and remoteness; this and that. In combination with Nondef, this meaning contrast is not so clear, though one could perhaps argue that a certain is more 'near' than some."

This appears to me to be nothing like a justification of the derivations proposed. Not only do the semantic arguments verge on the ludicrous, but there are also severe syntactic drawbacks. For instance, there is little plausibility for the creation of such radically different structures for:

(3.20) Certain of the men entered

(3.21) A certain man entered

as those produced by Roberts' rules, which may be presented in the form of the PS trees below:



As can be seen, in (22) certain of is a Predeterminer, but in (23) certain, by itself, is a Demon. This clearly is a very inadequate classification, in some ways reminiscent of the 'class-cleavage' problem encountered in Bloomfield (1935), and it appears to arise for very similar reasons.

One other inadequacy in Roberts' rules and which is worth mentioning is that the transformation which he gives to change:

(3.24) A man was on the table
into:

(3.25) There was a man on the table
also predicts as grammatical:

(3.26) *There was John on the table
This, of course, can be solved, as Roberts says, by ad hoc-ly assigning the feature 'Def' to proper nouns. But, even so, there still remains a host of unsolved problems, for example the sentences:

- (3.27) a *There were all men on the table
b *There was each man on the table

The question of existential there⁴ is too complex to be

⁴ It is, of course, necessary to distinguish between two types of there; the one in these examples may be taken as 'existential', perhaps equivalent to the logical operator " \exists ". The other there is locative and can be found in sentences similar to (26) but with a different intonation pattern: the main stress is on there, not John, and there is a pause after John:

(1) There was John, on the table
This suggests a different syntactic structure and that locative there is not derived by the transformation under discussion. In this connection see Allan (1971, 1972), Sampson (1972) and the discussion in §7.4 below.

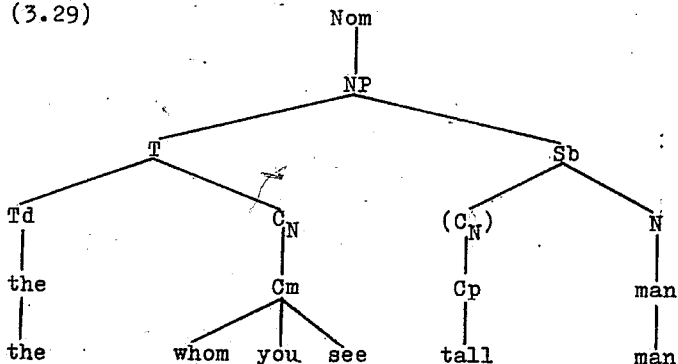
solvable by the kind of simple there-transformation which Roberts suggests. At the least, and this is a question to which we shall have to return, it necessarily involves an analysis of the internal syntax of quantifiers, at which Roberts makes no serious attempt.

If we now turn our attention away from such introductory studies to those which are more exclusively concerned with the analysis of determiners and quantifiers, but which still adhere to a theoretical framework closely allied to that found in the works discussed above, we find a number of papers which are concerned with the observably close relationship between the syntax of determiners and the syntax of relative clauses and adjuncts; of such works the most interesting are those by Lees (1961) and Smith (1964). The hypothesis behind both these articles is that the way relative clauses are embedded into higher sentences is determined by the kind of determiner which is contained in the NP upon which the clause is embedded. For Lees it is a question of the contrast between 'definite' and 'indefinite' 'articles' determining the structure. Thus, for (28) Lees presents the PS marker given in (29), (1961:

164):⁵

(3.28) The tall man whom you see

(3.29)

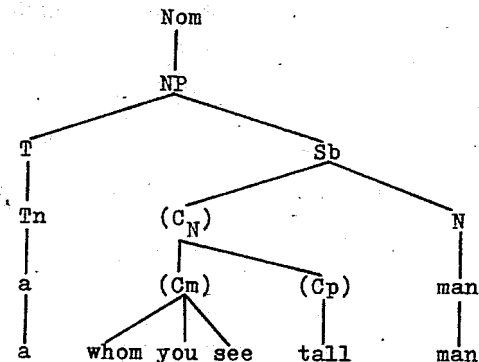


On the other hand, for (30) the underlying phrase marker (31) is suggested by Lees (1961:165): —

(3.30) A tall man whom you see

⁵ Abbreviations are as follows: Nom - nominal; Sb - substantive; Td - definite article; C_N - nominal complement; Cm - modifier complement (i.e., postnominal); Cp - property complement (prenoun adjective); Tn - non-definite article (i.e., indefinite and generic articles).

(3.31)



Even to the linguistically-naive observer it might seem strange that there should be such a marked difference in the structure of two such apparently similar sentences. And when we consider the matter carefully, it becomes even more worrying that relative clauses which are apparently of the same type, i.e., restrictive - but note the comments below about the status of the various relative adjuncts - should be derived from two different points in structure, namely as a rewrite of T in (29) and of Sb in (30). Although there is a faint syntactic justification for the analysis outlined above, in which respect see Smith (1961), it is surely insufficient to make the kind of structure postulated by Lees acceptable as part of any sophisticated transformational grammar. Our objections to Lees' proposals may be stated quite simply: the different underlying sources by which he proposes to explain the two different sentence types (28) and (30) are only justifiable in terms of those two.

different sentence types, and there is no independent motivation for them; given that, the analysis is viciously circular and incapable of giving an adequate explanation of the syntactic problem. In this respect Lees' proposal is not very different from that of Hill (1966), discussed in §3.2.

The main important of Smith's (1964) argument is that there are selectional restrictions; cf. Chomsky (1965:95ff.), operating between determiners and relative clauses. Involved at the very basis of her argument is a distinction between restrictive and nonrestrictive or appositive relative clauses (henceforth in discussing Smith (1964) these will be called R and A relative clauses). But this is in itself a not totally acceptable distinction. Certainly many of the traditional grammarians do make it, for example Poutsma (1904:420ff.), Curme (1931:223ff.) and Kruisinga (1932b:375ff.), but it is questionable whether such traditional formulations are correct. Thus, for instance, Sopher (1969:257) argues that the distinction is not at all clear and he dispenses with the classification:

"It is not practicable to classify relative clauses as restrictive (i.e., notionally defining or limiting) and non-restrictive (i.e., notionally continuative or non-defining), since many relative clauses appear to fit into either category without any

significant change, or, if there is a change of meaning, it is not relevant in the context."

A very similar point is made by Huddleston (1971:212-17), although he accepts that there may be occasions where the distinction is both plausible and necessary.

However, one may reasonably argue against Sopher and Huddleston, and also Morris (1969) and Zandvoort (1957:212-13), where again similar points are made, that their grounds for rejecting the classification described above are false, in that they fail to take account of certain relevant syntactic phenomena. Certainly, the distinction must be made between the R clause in (32) and the A clause in (33):

(3.32) The John Smith whom I know well
cannot be the thief

(3.33) John Smith, whom I know well, cannot
be the thief

Nevertheless, what must be said is that Smith does not offer explanations of R and A clauses except in terms of their relation to determiners and that in turn determiners are defined only by their relation to R and A clauses. The argument is thus circular, since she accepts the classification without external justification; it may well be that there is one, but it is not given.

Smith's argument then continues as follows: given that the R/A distinction is acceptable, which we shall, despite the above comments, assume for present purposes, although for even further critical discussion see Thompson (1971), it is possible to relate these two types of relative clauses to three types of determiners, which are, according to Smith (1964:248-49):

"... those accepting only A relatives, those accepting both A and R relatives, and those accepting only R relatives. These classes correspond to an intuitive classification of determiners as to definiteness; definiteness is associated with A relative clauses, indefiniteness with R relative clauses. The three classes are named Unique, Specified and Unspecified, to indicate that they are distinct from the traditional definite and indefinite determiners: with R relatives, Unspecified determiners occur: any, all, etc.; with R and A relatives, Specified: a, the, \emptyset ; with A relatives only, Unique: \emptyset (proper names)."

There then follows, Smith (1964:249), a set of PS rules which generate determiners and relative clauses which fulfil these conditions. These PS rules make use of the def/nondef contrast to separate the various Specified determiners.

There are several perhaps non-fundamental objections which must be made to Smith's account. Firstly, it would be quite erroneous to reach out for the aid of intuition if it claims, rather misleadingly, that indefiniteness is associated with R clauses and definiteness with A clauses; the reverse is surely much nearer the truth. Secondly, since Unique appears only to apply to \emptyset with proper names, how can Smith describe the common type of Unique which is the plus noun, as in:

(3.34) The sun; the moon

Is the considered as merely Specified in such occurrences? There is at least a case to be made that there it is much closer to the notion of Unique. Thirdly, although Smith notes the use of a 'zero article' (if we are willing to accept the existence of such a grammatical entity), she in no way explains it, nor, trivially, do her PS rules generate Specified \emptyset , despite her claims to its existence, quoted above. What ought to have been pointed out is that, in the context of Smith's study, Specified \emptyset occurs only with noncount nouns and the plural of count nouns, as in:⁶

(3.35) Milk which comes from goats is
nourishing

(3.36) Milk, which comes from goats, is
nourishing

⁶ The following discussion concerns only noncount nouns, but analogous arguments apply to plural nouns.

Speculation regarding the reason for this omission on Smith's part leads us to the fundamental objection to her account, for is it not the desire to consider the determiners as defined by their relation to relative clauses, and by that relation only, which leads to the omission? If, instead, there were an analysis of the semantic content of the determiners, would there not be more adequate ways of accounting for such matters as the occurrences of 'zero articles' in (35) and (36)? For example, ought we not to explain the presence of Specified \emptyset , or, preferably, the absence of the 'indefinite article', in collocations with mass nouns such as milk by one of the following two claims: either that a is a weakened form of the numeral one which can only occur with countable nouns (the claim of Perlmutter, 1970); or that a is in some way a realisation of the feature [+count], or [+singular], which, of course, is not to be found with mass nouns? For a discussion of these competing solutions see Chapter 11. We need only note at present that both at least move towards a more adequate solution of the problem than any statement in Smith's paper. Similarly, one might claim that the fact that Unspecified a is the type in (37) and that Specified a is the type in (38) can be discovered not by an analysis of relative clauses, but by a consideration of the relationship existing between a and any, and the syntactic status of the latter:

(3.37) A man could do the job in five minutes

(3.38) A man did the job in five minutes

Further, the syntactic status of any is more clearly seen in the light of its relation to negatives and other similar syntactic elements rather than to relative clauses. This is a point which we have already discussed, especially in §2.3, and we shall return to it in our discussion of Klima (1964), below, and more fully in §10.2.

One unfortunate aspect of Smith's paper is that her claim that no Unspecified determiner can take an A' relative, while very possibly true for her idiolect, is not true for mine, nor, apparently, for many other speakers of British English, where, at the very least, the restrictions are not so clear-cut. Thus for me the following sentences show differing degrees of acceptability, but none are completely unacceptable:

(3.39) Some dodos, who could not fly, were extant in the 15th century

(3.40) ?All men, who constitute the most vicious species on earth, are bipeds

(3.41) ??Many students, who had failed the exams, were sent down

Admittedly, many more examples are completely ungrammatical, for example:

(3.42) *Any book, which is about linguistics, is interesting

- (3.43) *No man, who came to the party, wore
a toga

But in (42) we may well be dealing with a rather different type of quantifier - relative clause relationship, and in (43) the problem is one of the inapplicability of coreference conditions, due to the negative. Touching upon this latter point, Smith (1964:258-59) claims that negation and question elements interrelate with A relative clauses and determiners to make the sentences (44) - (47) ungrammatical. But I find only (46) ungrammatical:

- (3.44) He didn't eat the mango, which I
bought for him yesterday
- (3.45) He didn't eat the mango, which was
overripe
- (3.46) *He didn't write a novel, which was
published by McGraw-Hill
- (3.47) He did not use the air mattress,
which belongs to the Halls

A similar conflict arises with questions, for Smith (1964:259) claims that all of the following are ungrammatical:

- (3.48) *Did he paint a mural, which hangs in
the Hotel Prado?
- (3.49) Did John, who is a journalist, write
a novel?

(3.50) Who ate the mango, which Eleanor
bought yesterday?

(3.51) *Who wrote a novel, which was pub-
lished by McGraw-Hill?

Any evaluation of Smith's analysis of these sentences, therefore, is bound to be complicated by the differing judgments of grammaticality. In the case of examples (44) - (51), nevertheless, it can be observed that the crucial distinction may be one of sentence negation (or questioning) versus negation (questioning) of a constituent of the sentence. For speakers such as myself, we may find that only constituent negation (questioning), which does not affect (include in its scope) the relevant NP, is present, and this may be the reason for the conflicting assignments of acceptability. It might also be noted that the sentences which are ungrammatical for me all involve an antecedent which, in the terminology of Fillmore (1968), is in the Resultative case.

The final section of Smith's paper is concerned with generic determiners, and in it is found the rather surprising belief that the is the only generic determiner. Thus she writes (1964:259):

"The following discussion is concerned with sentences that are said to be generic, or to have a generic determiner ... The determiner in question is the with singular affix."

What, then, of:

(3.52) A lion is a dangerous animal

Is there no generic determiner here? This would appear to be the position which Smith is obliged to hold. But surely most linguists would agree that at least the first a in (52) displays generic characteristics, although, of course, we might rather wish to claim that it is the NP a lion as a whole which is generic; however this distinction is not important at present. Perhaps (53) is an even more convincing example:

(3.53) During the winter a dormouse hibernates

There would seem to be only one reason for the kind of interpretation which Smith gives. It is that a in (52), (53) and similar sentences would be accounted for simply in terms of the contrast between Specified and Unspecified a. But as has been said above, the latter type of a is related to any, and that relation cannot be carried over to generic a without some modification, for that would suggest that:

(3.54) ?Any lion is a dangerous animal

(3.55) ??During the winter any dormouse hibernates

would be rather more acceptable than they appear to be. For a discussion of what the relationship of any to generic a might be, see Perlmutter (1970). It is a subject to which we shall have to return at some length in §11.4.

Let us now assume, in contradiction of the facts, that the plus singular affix is the only generic determiner, and consider the adequacy of Smith's analysis of it in vacuo. Smith asserts that the status of generic the is not to be regarded as a matter of grammar but as a matter of the interpretation of a grammar. This appears to be the first step towards a theory of interpretive semantics, which is extensively applied to the grammar of quantifiers in Jackendoff (1969, 1972b) and more generally advocated in Chomsky (1972b). Jackendoff's theory will be examined in Chapter 6, but some remarks specifically about Smith's position are in order here. The main justification for her position is that generic the occurs with relative clauses under the same syntactic conditions as does the nongeneric variant (Smith, 1964:260). But consider the following sentences:

(3.56) The elephant which lives in Africa
has big ears

(3.57) The elephant which lived in Africa
has big ears

As Smith's claims predict, (56) may be interpreted either generically or nongenerically. On the other hand, because of the past-present contrast between the verb of the relative clause and the verb of the matrix clause in (57), that sentence has only a nongeneric meaning. This is inexplicable in terms of the interpretive rule given by Smith (1964:263):

"(a) the determiner the may be interpreted as either anaphoric or generic if there is no grammatical previous mention, or if the sentence in question has no framing adverbial; (b) if there is a grammatical previous mention and the sentence in question has no framing adverbial, the determiner the must be interpreted as anaphoric."

Perhaps the most significant omission here is the fact that Smith does not (indeed, given the structure of her theory she may not be able to) take account of cataphora, which process would appear to be operating in (57) at least. Therefore, even leaving aside the general status of interpretive rules, we must conclude that Smith's proposal is inadequate as an account of generic the, and because of its restriction to that item only, as an account of generics as a whole.

In Smith (1964), as we have noted, there is a certain amount of attention paid to the interaction of negation and question elements with quantifiers, but by far the most extensive study of such matters within early transformational theory is to be found in Klima (1964). Since Klima is concerned primarily with aspects of negation in English, rather than with the precise structure of determiners and quantifiers in noun phrases, he offers no detailed analysis of the underlying structure of these items except where it is relevant to

negation and similar syntactic relations. We shall therefore consider only those aspects of determiners and quantifiers in which Klima is most interested, and disregard his proposals for the constituent structure of NP's where these are irrelevant to his main interests.

Klima (1964:esp. 276-84) notes that the 'indefinite quantifiers', of which the most important is any, have a peculiar syntactic distribution, in that they are ungrammatical if the sentence in which one of them occurs is declarative and positive and the verb is in the past tense or is aspectually perfect; thus we have:

(3.58) *I saw any Russians with snow on
 their boots

However, if such a sentence contains a negative element, then any is acceptable:

(3.59) I didn't see any Russians with snow
 on their boots

Of course, such observations have been made previously, most notably in the study by Collinson (1937), which was discussed in §2.3. What is especially interesting about Klima's work is his attempt to explain these observations within the framework of transformational grammar. The way in which he approaches such an explanation is to posit certain transformational rules, see Klima (1964: 279-80), which introduce into the structure of the sentence a negative element which, according to its position in structure, changes either the verbal or the

quantifier element. (59) is an example of the negative being incorporated into the verbal element and so permitting the quantifier to be 'indefinite', in this case any. The quantifier no, as in:

(3.60) I saw no Russians with snow on their
boots

is also generated by Klima's rules. In this instance the negative is incorporated in the quantifier rather than in the verbal element. When the negator is incorporated into both elements, which is a violation of Klima's rules, we then find the substandard form:

(3.61) *I didn't see no Russians with snow
on their boots

Klima's account appears to be correct with respect to the phenomena which he discusses, but in fact the correspondences between any and some (replace any by some in (58) and the sentence is acceptable) or, indeed, between 'indefinite' quantifiers in general and the other quantifiers, is open to even wider generalisation. It must be made clear that Klima is aware of this, and he notes (1964:311-15) that a number of other elements - questions, only and adversatives, e.g., stupid, reluctant, which he classes together with the negator as 'Affectives' - also permit grammatical occurrences of the 'indefinite' quantifiers.

However, any-usage is of an even wider range than is discussed by Klima, and this is clearly demonstrated

by Bolinger (1960:383-84), where examples similar to:

(3.62) He stole anything he needed

help to substantiate the claim that any may be used grammatically if there is a certain type of dependent restrictive relative clause present.⁷ Example (62) may be crudely paraphrased as:

(3.63) If he needed something he stole it

On the other hand:

(3.64) He stole something he needed

may be paraphrased as follows, in which there is no conditional:

(3.65) He stole an object; he needed that
object

The question of how far the conditional present in (63) contributes to the grammaticality of any in (62) is a difficult one, but that it is a vital factor seems to me to be provable. To show this we have to consider the difference between (62) and the very similar (66):

(3.66) He stole everything he needed

Now, note that it is not the case that (62) implies

⁷ That there is more than one type of restrictive relative clause, or, rather, that there may be more than one underlying source for the various structures which appear on the surface as restrictive adjuncts, further weakens the usefulness of the approach taken by Smith (1964). This is apart from the criticism offered by Sopher (1969) and others, mentioned above.

(66), which would be the case if the sentences were synonymous. (62) describes the criterion according to which things were stolen by him, whereas (66) describes the way in which he acquired all the things which he needed. We can thus observe that (62) sets up the condition for stealing, whereas (66) states what and how much was stolen. Thus the notion of a conditional is inherent in (62).⁸ And we may further observe that if is in fact similar to Klima's affectives, in that it permits the grammatical occurrence of any, as can be seen by comparing (67) and (68):⁹

(3.67) If he stole anything, that was wrong

(3.68) *He stole anything

These factors would seem to be good evidence for postulating an underlying conditional to explain the grammaticality of any in (62).

⁸ For further remarks on this point, cf. §3.4, where some relevant proposals by Vendler (1967) are discussed. In §10.2 an analysis of sentences like (62) will be suggested which largely accords with our statements here.

⁹ There is a grammatical interpretation of (68) which is ignored here. Such interpretations may be explicable in terms of a deleted conditional, but I shall not pursue the point here. We might also note that if (68) is given that interpretation then anything would probably be heavily stressed.

Even for some of Bolinger's most difficult examples, along the lines of, for instance:

(3.69) This acid consumes any rust

I believe that it is possible to suggest that they too can be explained by the presence of an underlying conditional, as in:

(3.70) If there is rust, this acid consumes it

The difference between (69) and the parallel sentence with every is akin to that between (62) and (66). In (69) the claim is not simply made about rusts which exist, but also about rusts which are not (yet) known (to the speaker). They too will be consumed by the acid, the speaker asserts. Indeed, as Vendler (1967) points out, cf. note 8, in cases such as (69) there is the possibility of nonreferential usage. But in the parallel sentence with every the claim is only being made with respect to known and existing rusts, and is not available for a bona fide extension to unknown and non-existent rusts. This distinction arises because it is only in (69) that the condition that something be a rust is stated. In the case of every rust there is no such condition, only a statement of asserted fact.

Given that sentences such as (62) and (69) appear to be good candidates for an underlying conditional source, which I believe that the above discussion has shown to be true, the question remains of what the exact source must be. This is a most complex and difficult matter to

which we shall return in Part III, especially §10.2, where its consideration will be more appropriate.

Some later transformationalist accounts of the some - any relationship have acknowledged that such elements as conditionals must be taken into consideration, see especially Seuren (1969), but most attention has been paid to a more obvious weak point in Klima's account. This point is that it is not clear in Klima (1964) what the exact grammatical status of any is. So, is any in all cases a suppletive form of some, transformationally derived, where it occurs, from some, or has it, as it were, a linguistic life of its own? One key piece of evidence is that there are sentences where both some and any, apparently, are grammatical, although there is a meaning distinction, which may often be rather subtle. Compare the pairs below:

(3.71) a If you have some bananas, I'll buy them

b If you have any bananas, I'll buy them

(3.72) a Do you want some whisky?

b Do you want any whisky?

In such cases Klima's rule is apparently meaning-changing, and after Katz and Postal's (1964) claim that transformations should not change meaning, the 'some-any rule', as Klima's 'Indef-incorporation' rule came to be known, was regarded with suspicion, since it was an

important counter-example to Katz and Postal's claim. Thus Jackendoff (1969) suggests that the some-any variation be handled by an interpretive rule (again meaning-changing, but within a theory which accepts such rules, contra Katz and Postal); on the other hand, R. Lakoff (1969a) suggests that the variation cannot be handled by one syntactic rule, but that reference to presuppositions is necessary. We shall discuss Jackendoff's position in §6.5 and Lakoff's in §10.2. For a fuller discussion of the meaning-changing controversy see Partee (1971).

The question of whether or not transformations ever change meaning is significant of a number of other issues which were entering discussions of transformational theory around the mid-1960's. It is not our business to provide here an historical summary of such changes, which are extensively discussed in Katz and Postal (1964), Chomsky (1965) and Lakoff (1970b) (the latter actually being written in 1965). Rather, we need only note that the results as far as our own studies are concerned were fairly radical. With the proper introduction of semantics into the realm of transformational grammar, and with a more highly developed formal apparatus becoming available, the analysis of determiners and quantifiers became less influenced by structuralist theory, and it quickly became established that it was far from adequate merely to write a number of PS rules,

with perhaps a simple transformation or two, if one wished to account for the behaviour of these items. The kinds of analysis proposed, therefore, are sufficiently distinct from those suggested by early transformationalist accounts to deserve completely separate consideration. Some of this we shall do in Part II, but we must first conclude the present Part I by looking at another group of transformationalist works which belong to a tradition slightly different from that which we have discussed above.

3.4 Nongenerative transformational theory

In this section we shall be considering work on our subject which has been carried out within the theoretical framework developed by Zeilig Harris and others at the University of Pennsylvania. Although in its earliest stages this theory was not to be differentiated from the theories developed by Noam Chomsky, who was himself a pupil of Harris, there was an early divergence in methods, and the two branches can now be said to have only a minimum amount of theory in common. Here it would be out of place to do any more than sketch in the very barest outline the differences between the two theories, for further discussion cf. Robbins (1968:58-83). Harris' theory is most like that presented in Chomsky (1957), in that he posits a set of kernel structures which essentially contain a sequence of word-

classes. By a set of transformational rules non-Kernel sentences may be 'decomposed' into kernel sentences and transformational constants. A most important distinction between Harris' grammar and Chomsky's is that the latter is generative, the former is nongenerative. Also, in Harris' grammar there are to be found no highly abstract deep structures as are found in the later versions of Chomskyian transformational theory, but not, of course, in the works which we examined in §3.3. Finally, it should be noted that in Harris' theory elements which may occur in kernel sentences are called primitive elements; those which occur only in non-kernel sentences, and thus are introduced by the transformational constants referred to above, are called derived elements. We shall see that it is proposed that, for example, a is a primitive element in the grammar of English, whereas the is a derived element.

The two most important works which attempt a description of the 'definite article' within the terms of Harris' theory are those of Robbins (1968) and Vendler (1967). There is a distinct difference in aims between the two, for the former is primarily a grammatical treatise whereas the latter properly belongs to the field of linguistic philosophy; furthermore, we should note that Robbins is mainly concerned with the grammar of the and exclusively concerned with the grammar of the 'articles'; Vendler's book, on the other hand, is a

collection of papers, of which only one concerns the, and elsewhere in the book he discusses the semantics and syntax of a number of quantifiers. Nevertheless, both Robbins and Vendler reach the conclusion that the is dependent on a restrictive relative clause, cf. Perlmutter (1970), in all its occurrences except those which are generic, as in:

(3.73) The lion is a dangerous animal

Thus Vendler states (1967:46):

"The definite article in front of a noun is clearly and infallibly the sign of a restrictive adjunct, present or recoverable, attached to the noun."

And Robbins (1968:54) makes the rather less ambitious claim that:

"Determinative the is always indicative of sentence combination: either a noun-sharing combination of one sentence with a transformed other sentence, or the inclusion in a Pred of a sentence nominalized into a definite noun phrase ... In this essay anaphoric the is treated as a special kind of occurrence of determinative the."

Naturally, both claims, if they are correct, support the theory that the is a derived element.

It would indeed seem to be the case that in certain nominalisation transformations the is introduced as a

result of that nominalisation. However, Perlmutter (1970:237-38) claims that the, or, rather, some kind of dæictic, is only obligatory when the resultant nominalised NP is uncountable. He claims that there is a set of nominalised NP's which are countable and which can take either the or a, giving the following example:

(3.74) I saw a changing of the guard

which he contrasts with examples such as:

(3.75) a *I saw a shooting of the hunters

b I saw the shooting of the hunters

No problem would seem to arise with respect to the syntax of nominalisations such as that in (74), which can be accommodated within a theory of the 'articles' which pays no attention to nominalisations. Or at least that appears to be the case, but it ought to be borne in mind that even if (74) is not completely ungrammatical, it is not wholly acceptable either. Further, it should be observed that there is a difference in meaning between the two sentences below:

(3.76) a A reading of this book will confirm
your hypothesis

b One reading of this book will confirm
your hypothesis

However, this may only be unhelpful to Perlmutter's claim that a ought to be derived from unstressed one, cf. Chapter 11 for further discussion. Nevertheless, there is yet another objection, which is that Perlmutter's claim leaves an unexplained gap in the

distribution of the so-called 'uncountable' nominalised NP's, which suggests that the problem is far from being resolved. This gap is that although we would expect a sentence parallel to (75a) but without a, no such sentence exists:

(3.77) *I saw shooting of the hunters

Why, we must ask, is the obligatory there? There is, unfortunately, no simple answer to this question, partly because the status of nominalisations is unclear, cf. Chomsky (1970) and the references therein, but some light may be shed upon the question by looking at the relation between the and restrictive relatives. This we shall do now, but no answer to the above question can be expected immediately.

That there is a relation between the and restrictive relatives, and that it is important, cannot be doubted, not only in the face of the syntactic and semantic evidence we shall consider below, but also by virtue of the etymological evidence that 'definite articles' and relative pronouns are often derivable from the same root, and in certain languages are even homonyms, e.g., German der, die, das.¹⁰ In this context the

¹⁰ Kent (1944) has an interesting description of the situation in Old Persian, where it is apparently very difficult to ascertain whether, in certain contexts, a 'definite article' or relative pronoun is being used.

status of the Greek category arthron, discussed in §1.2, will be recalled. It is, therefore, extremely tempting to introduce the, as Robbins does, by means of a relativisation transformation. Thus she would derive:

(3.78) I stole the flower which you liked
from the two (kernel) sentences:

(3.79) I stole a flower
You liked a flower

Each of the sentences contains the 'shared noun' flower, which is changed by the transformation process into which in the second sentence (with consequent change of word order). The instance of flower in the first sentence has its 'article' changed from a to the to indicate noun sharing. As Hill (1966:225-26) pointed out when he devised a similar transformation, cf. §3.2, the difficulty of such a solution is that there does exist a variation on (78) with a instead of the, namely:

(3.80) I stole a flower which you liked
and that also would seem to be derivable from (79).
Hill's solution is, as we have seen, totally unsatisfactory, but Robbins does not attempt any comparable solution, merely regarding the presence of a or the, i.e., (80) or (78), as due to different optional derivations from (79).

Vendler (1967:49-50) also discusses the problem and he suggests that when the shared noun is 'unique' it is the which is found. Thus for:

(3.81) I know the man who killed Kennedy
he says that the is obligatory, since kill demands a
'unique agent'. It is not, however, wholly correct to
state that kill does demand such an agent, for we find
sentences such as:

- (3.82) a John and Bill killed the landlord
- b A pair of criminals killed the guard
- c The Nazi's killed many millions of
 Jews

But notice that paralleling (82a) there is the sentence:

- (3.83) I know one of the men who killed
 the landlord

On the other hand, (84) is ungrammatical:

- (3.84) *I know a man who killed the landlord

In other words, as with Vendler's example (81) above,
the antecedent must here be 'definite'. It is possible
that a derivation of (82a) involving phrasal conjunction,
cf. Lakoff and Peters (1969), will help solve the dif-
ficulties which that sentence presents, although if this
is extended to (82b) and (82c) there are then problems
in attempting to provide the kind of justification neces-
sary for the desired underlying structures.

There is, however, a more serious counter-example
to Vendler's proposals, namely:

- (3.85) I know a man who killed his landlord

A comparison of (84) and (85) suggests that the use of
the with an antecedent NP is related to the syntax and

semantics of the restrictive adjunct as a whole, rather than simply to the agent-verb relation. Thus, only if the relative clause as a whole defines the relativised NP as unique is the obligatory; if there is no such definition the is not obligatory. Since, leaving aside the problems raised by the examples in (82), only one person can kill a previously defined (animate) referent, we can perhaps account for the ungrammaticality of (84) in a manner similar to the explanation which is needed for hyponymic referents, as in:

(3.86) Tom was watching a robin, until the bird flew away

(3.87) When I got on the bus, the conductor was demolishing the ticket machine

For such cases see Jackendoff (1971c:140) and Lyons (1968:453-56). In contrast, in (85) his landlord is not a previously defined referent, it is only defined in terms of the now-mentioned agent. Therefore a with antecedent NP is grammatical, as in (85). We must note, however, that (88) is also acceptable:

(3.88) I know the man who killed his landlord
Because Vendler is determined to derive all instances of the from a restrictive relative clause, his proposals are of little help here, as he is unable to provide a justification for distinguishing derivationally between (85) and (88). The difference between the two sentences could only be accounted for in his theory by using an optional transformation to change a to the in (88) but

not in (85). But as the two sentences have different meanings this is undesirable, for the existence of meaning-changing transformations will once again be asserted.

An alternative method of distinguishing between these two sentences would be to claim that the in (88) is anaphoric. Whether or not this is correct, and what other problems it leads to, we shall discuss in Chapter 12, but for the present we should observe that this solution, which is intuitively appealing, is not available for Vendler. To see why this is so we have to consider his claim that anaphoric the is also derived from a restrictive relative clause. In this case the restrictive clause is identical with the clause or sentence in which the first use of the noun with the same referent is found. So for anaphoric the in:

(3.89) I stole a flower. The flower was
pretty

Vendler suggests that the derivation of the second sentence in (89) would be as follows:

(3.90) A flower was pretty
I stole a flower

There is embedding of the second kernel sentence into the first, and consequent change of a to the. Vendler rightly notes that (1967:52-53):

"If our conclusions are correct, then a noun in the singular already equipped with the

definite article cannot take another restrictive clause, since such a noun phrase is a singular term as much as a proper noun or a singular pronoun."

Therefore an NP which has anaphoric reference cannot take a restrictive clause, which is why the alternative method for deriving the in (87), mentioned above and containing a restrictive clause, is not open to Vendler. But note that we can now predict, with accuracy, that the following sequence is nonanaphoric:

(3.91) I stole a flower. The flower which
was red was pretty

It is indisputable that two different flowers are being referred to in (91). On the other hand, if a nonrestrictive clause had been used, only one flower would be referred to:

(3.92) I stole a flower. The flower, which
was red, was pretty

Despite the predictive power of this analysis, which is also presented by Robbins (1968:128-61), it is not wholly satisfactory. The most serious objection is that the derivations required can be so complex that it is doubtful whether they can be acceptable. Thus Jackendoff (1971c:141) points out that the source sentence for the anaphoric NP's of:

(3.93) A man asked a girl for a book, but
the girl would not give the man the
book

must be "of the crushing proportions of":

- (3.94) ... but the girl who a man asked for a book would not give the man who asked the girl who the man who asked a girl for a book asked for a book for a book the book which the man who asked the girl who the man who asked a girl for a book asked for a book for a book asked the girl who the man who asked a girl for a book asked for a book

Jackendoff further points out that Robbins' proposals run into possibly insoluble problems concerning 'Bach's Paradox', for which see Karttunen (1971). Another objection is the one arising from Vendler's account and which we have already touched upon, namely that it would appear to be the case that NP's can only have one dependent restrictive relative clause, for otherwise the seeming ungrammaticality of restrictive relatives dependent upon anaphoric NP's, but cf. example (88), is not explicable. However we find examples such as:

- (3.95) The girl whom I know who wears a red hat is called Hannah

- (3.96) The girl in the miniskirt on the motorbike is going to Glasgow tomorrow

But it is possible to avoid this objection by conjoining the two clauses or adjuncts before relativisation takes place; then only one embedding transformation, with the

accompanying production of the, will be involved, and this would satisfy Vendler's restrictions. There is considerable controversy over whether a conjunction analysis, as suggested here, or a 'stacking' analysis is preferable for such multiple relatives. For some discussion of this see §12.2 and Stockwell et al (1972: 442-47).

Although we have observed that there are a number of objections which can be raised against the proposals offered by Robbins and Vendler, it is undeniable that there is a relationship between occurrences of the and restrictive relatives. One further piece of evidence in favour of their accounts is the fact noted by Perlmutter (1970:241-42) that certain instances of the + N, more precisely those where N is a proper noun, can only occur if a restrictive adjunct is present, for example:

(3.97) a The Paris that I love

b The Paris of the 19th century

For the moment, therefore, we may safely conclude that the presence of the is often associated with a restrictive adjunct; but we cannot yet provide an adequate formalisation of this association, since it is highly complex. In Chapter 12, however, we shall see that there is some independent motivation, arising from our analysis of quantifiers, which will help to explain the nature of the relationship. But whether or not the difficulties we have observed can be entirely resolved

is another matter.

As we have mentioned above, elsewhere in Vendler (1967) there is a discussion of some quantifiers, and it is to this discussion that we shall now turn our attention. Vendler (1967:70-96) looks at four quantifiers: each, every, any and all. These are to some extent ordinary language equivalents of the universal quantifier in logic, although, as Jackendoff (1972a) points out, this may not always be the case with any. Vendler's aim is to show that the logical analysis of these quantifiers is inadequate, cf. our remarks in §5.5, for it obscures syntactic regularities which may be peculiar to each one of them. He claims that we have to analyse these words more deeply in order to discover the true facts about them, and he comes to the conclusion (1967:74) that although they all in some way express totality:

"The reference appropriate to all is collective, and the reference appropriate to each or every is distributive."

He further comes to the conclusion (1967:76-78) that each is strongly distributive whereas every is weakly so. Later on Vendler notes that any and all often perform a similar task, i.e., they can both be nonreferential (1967:93). Vendler also concludes that sometimes all performs tasks similar to those of every, sometimes similar to those of any.

I believe that in his conclusion that there are two contrasting sets, one composed of each and every and the other of any, with all schizophrenically split between the two, Vendler provides a most useful appreciation of the behaviour of these quantifiers, and in support of this belief I would like now to present a number of arguments which are complementary to, and in support of, those given by Vendler. Consider firstly the following:

- (3.98) All professors who break the bank
are banned from the casino
- (3.99) *All professors who broke the bank
are banned from the casino
- (3.100) All the professors who broke the bank
are banned from the casino

The unacceptability of (99)¹¹ can be accounted for if we

11 Some speakers find (99) marginally acceptable. This may be due to the fact that for them it is possible to interpret all professors as if it were, in my speech, all the professors. Whatever the explanation may be, it is interesting to note that the Dutch sentence:

(i) Ik heb alle jongens gezien
must be translated into English as:

(ii) I have seen all the boys

The sentence:

(iii) Ik heb al de jongens gezien
is rather more emphatic, rather like:

(iv) I have seen all the boys, every single one

accept that all when not followed by (of) the is generic, for that sentence, because of the tense-switching between the verbs, cannot be generic. Similarly, (100) is acceptable, precisely because all the is not generic; and, of course, (98) is acceptable because there is no tense-switching and therefore generic reference, as in all professors, is possible. If we resort to an analysis of the meaning of these sentences we can see that (98) refers to the class of professorial bank-breakers being banned, and that all has primarily an emphatic purpose. On the other hand, (100) states that, of the professors who broke the bank in the past, all are now banned. (98) states a logical implication:

(3.101) If a professor breaks the bank, then
 he is banned from the casino

whereas (100) is purely descriptive of a certain state of affairs:

(3.102) Some professors broke the bank.
 All those professors are banned
 from the casino

An alternative description of the contrast between (98) and (100) is to say that in the latter case there is reference to a non-null set (which, in fact, must not be smaller than three, see Chapter 4). On the other hand, in (98) reference may be to a null set, for even if no professor has broken the bank the statement is still logically valid. The only way in which it can be

falsified is to show that some professor has broken the bank and has not been banned from the casino. Now, as Vendler (1967:93) points out, this nonreferential use of all is exactly like the nonreferential use of any, and therefore it is instructive to note how close a paraphrase of (98) is (103):

- (3.103) Any professor who breaks the bank
is banned from the casino

A further parallel between any and all is to be found in the fact that all has a restricted grammatical distribution. This is a point which has received remarkably little attention from linguists, perhaps because the distribution is not identical to that of any, but the following comparison is surely worthy of note:¹²

- (3.104) a *I saw any boys
b *I saw all boys

- (3.105) a Any latecomers are to report to
the office
b All latecomers are to report to
the office

Of course, the parallelism does not always hold, as has been remarked above:

¹² There is a marginally acceptable interpretation of (104b) in which all is equivalent to only, as in:

- (1) John went to an all-boys school

This usage does not provide immediate counter-examples, although it is far from easy to explain.

(3.106) a I didn't see any boys

b *I didn't see all boys

Nevertheless, the restricted occurrence of all bears enough resemblance to that of any to suggest that a detailed comparative study of their behaviour is essential.

The facts above may also help to explain Vendler's observation noted above that all sometimes relates to any, sometimes to each or every. Sub specie aeternitatis it might be considered unfortunate that English sentences such as (104b) or (106b) are ungrammatical, for it might be predicted that there ought to be a grammatical form in such contexts for a 'universal' quantifier collocating with an 'indefinite' NP. Therefore what we have to look for is a possible suppletion form, and this seems to be there in:

(3.107) a I saw every boy

b I didn't see every boy

The main objection to the claim that every is a suppletive form of all in such sentences must be that the collocating noun is singular; but the validity of this objection can easily be disproved. It is surely the case that in underlying structure the NP is plural, for the following reasons. Firstly, the reference of every boy is to more than one (indeed, more than two) objects. Therefore the NP is notionally plural. Secondly, the grammar must be able to account for the fact that every.

does not collocate with noncount nouns, for example:

(3.108) *Every milk was spilt

The clearest way to account for this is to claim that it collocates with plural nouns only. Thirdly, in partitive constructions, which admittedly are not strictly relevant here, the noun in the partitive construction must be plural:

(3.109) a Every one of the boys was late

b *Every one of the boy was late

Presumably the reason for the number switch with every, which must be handled transformationally, is what Vendler (1967:74) calls the distributive reference appropriate to every (and each). But at present the reason for the switch is not as important as the fact that it is needed; it is a point to which we shall return in §6.4, and for a more general study of the relations between each, every and all see Chapter 9. We have already seen, however, that there is considerable evidence in favour of Vendler's observations about the status of all and that it is possible to give some explanation of why the facts are as they are. It has long been accepted that any is a particularly difficult quantifier to analyse, but the truth is that the other quantifiers discussed above are no less difficult. We must be grateful that Vendler has pointed the way towards a solution.

3.5 Conclusion

In Part I of this thesis we have examined the development of grammatical theory with respect to determiners and quantifiers from the earliest sources within the Western grammatical tradition up to work written only a few years ago. In Chapter 1 we looked at what was called the 'classical tradition', to which we assigned not only Greek and Latin grammars, but also more recent work which still held to a 'parts of speech' theory. We concluded that that theory, which has its origins in the writings of Aristotle, the Stoics and Thrax, was inadequate as a linguistic theory. The main reason for this, as far as we were concerned, was that the formalism imposed by that theory was unable to give a natural classification of determiners. This was not necessarily the case with the earliest works in the tradition, which often gave insightful descriptions of determiner systems in Greek, and to a lesser extent in Latin, and to these writers we owe such important concepts as anaphora. We noted that these analyses often showed a surprising correspondence with recent non-classical proposals, but it is difficult to ascertain to what extent this is merely a matter of coincidence and to what extent they show a significant identity of analysis.

Later scholars within the classical tradition, especially medieval grammarians and the early vernacular grammarians, all too often showed a tendency to accept classical theory as dogma, and we observed that in several cases rather ludicrous results occurred because of an attempt to fit English into the structural pattern of Latin and Greek on purely surface structure criteria. Nevertheless, some of these grammarians displayed remarkable insights, especially when they refused to be overawed by dogma. One such whom we might mention here was James Harris, and it is perhaps worthy of note that the kind of theoretical framework within which he operated was closer to the earliest Greek grammarians than to the work of slightly later writers such as Thrax. The most recent scholars in the classical tradition are perhaps somewhat removed from the 'parts of speech' theory and they can often be associated with those whom we have called 'notionalists'. To the extent that this is true they represent an undoubted advance, but the retrograde influence of strict classical theory is still observable in their work. We may conclude that the theory of the classical tradition was often incorrect, but that within that theory the foundations for our study had nevertheless been laid.

Despite the difficulty encountered at the beginning of Chapter 2 in defining the term 'notionalist', once a definition had been established it was clear that

one could distinguish between a Guillaumiste 'school' and other notionalists who were not at all influenced by the work of Guillaume. Our major criticisms of Guillaume's work were that his theory had insufficient empirical support and that it was restricted, as far as determiners and quantifiers are concerned, to an analysis of the 'articles'. Although Guillaume's work, therefore, held little immediate interest, it was noticeable that those whose work was based to some extent on his pioneering efforts had rather more to contribute. Yet there was still a tendency to envelop conclusions in a mist of psychologisms which were singularly unhelpful. On the other hand, other notionalists, especially, perhaps, Collinson, made many acute observations which have not yet been fully explained. If we are able to formalise such observations it is probable that we shall have made an important step towards an adequate linguistic analysis of determiners and quantifiers.

In Chapter 3 our attention was primarily directed towards an examination of structuralist models of the determiner and quantifier systems, and we considered firstly several analyses which were broadly speaking Bloomfieldian in outlook. Such analyses were seen as a regression from those we had examined earlier, most especially in their separation of semantics from syntax, but also, in comparison with the notionalist descriptions, in the lack of interest in possible underlying

structures. Most of the analyses discussed were considered to be fundamentally mistaken, and it was difficult to see how they could have been improved, given the basic theoretical assumptions. It is questionable whether the earliest transformationalist works, which were discussed next, showed a measurable improvement in adequacy, although we must draw attention in this regard to the impressive work of Edward Klima, which was seen to be an extension and formalisation of some of the points which Collinson had discussed some twenty-five years earlier. Finally, we looked at the proposals made within the particular version of transformational theory originated by Zellig Harris, and we noted that both Robbins and Vendler had most interesting remarks to make about the objects of our investigation, especially the and all. Because of their emphasis on semantics, both these works may not have belonged to this chapter proper, since they often share the assumptions of the notionalists of the previous chapter. It is significant that these two scholars, who were seen to pay the most attention in this chapter to the integration of syntax and semantics, also provided the most useful insights of this chapter into the determiner systems.

Having completed our historical survey, we must now attempt to evaluate contemporary analyses of the determiner and quantifier systems in Contemporary English, and then provide, where possible and necessary,

alternative solutions. In attempting this task we shall make the distinction outlined in the Introduction between quantifiers and other determiners, but this is done solely in order to make the material more amenable to analysis and should not be considered as necessarily a decision of grammatical importance. Therefore in Part II we shall first see how adequate present theory is in practice, by attempting an analysis of a quantifier which has been little discussed in recent literature, namely both. Then we shall analyse the merits and demerits of present theory before presenting, in Part III, our own proposals. The discussion of the 'articles' and deictics (such as this and that) will be delayed until Part IV, by which time it ought to be observable that their status is not always independent of the status of the quantifiers, although in some respects, and with regard to some items, the divergences may be considerable.

Part II

Recent Quantifier Theory

Chapter 4

A grammar for 'both'*

4.1 The semantics of 'both'

Most students of English grammar have assumed that both, at least in positions where it is a surface structure quantifier, differs from all only in that the latter indicates that the reference of the NP in which it occurs is to more than two objects (unless the associated noun is uncountable, which case we may ignore at present), whereas the former indicates that such reference is to two and only two objects. Thus Strang (1962: 116) states:

"All collocates with either plural or uncountable head-words; ... Both can only have dual function, that is, its head must be two singulars ('Both Mary and John') or a plural with referents two in number ('Both the crumbling, gnarled old trees')."

And Jespersen (1914:197), in his discussion of "words referring to two", writes:

"First we have the word both as in both my sons, indicating that I have two, while all

* A slightly modified version of this chapter appears as Hogg (1973a).

my sons would imply that I have more than two." Such claims are supported by many pairs of sentences, for example:

- (4.1) a All the children like cream
b Both the children like cream
- (4.2) a All their trees are to be chopped down
b Both their trees are to be chopped down
- (4.3) a Peter knows all the women who were at
the party
b Peter knows both the women who were
at the party

Apparently the only difference between the (a) and (b) members of each pair is that in the (a) cases, where all is used, reference is to at least three children, trees, women, but in the (b) cases, where both is the quantifier, just two children, trees, women are referred to.

If, therefore, as certainly seems to be the case, the only difference between all and both is that the latter demands dual reference (i.e., reference to two and only two objects), the former nondual reference, then it would appear probable that we can derive both from the same source as all, as long as there is some means of marking duality. Perhaps this could best be handled by a feature [idual], which might be added to the feature complex of a deep structure quantifier ALL, from which the surface forms both and all would both be derived. It should be noted that the introduction of

such a feature would not be incompatible with the fact that all can collocate with both count and noncount nouns, while both can only appear with the former. Noncount nouns may not be [+plural] semantically (although they may be so syntactically) and [+dual] will be a rewrite of [+plural]. Therefore (4a) will be marked ungrammatical because beer is not [+dual], but (4b) is grammatical and will be marked so for exactly the same reasons as (4a) is not:

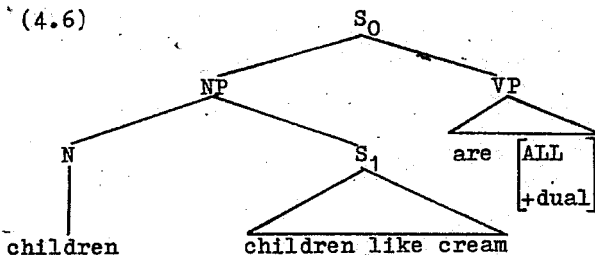
- (4.4) a *Both my beer is flat
 b All my beer is flat

Let us accept, pro tempore, the analysis for quantifiers commonly known as the 'Lakoff-Carden hypothesis', for despite the inadequacies pointed out by, for example, Lakoff (1970d), it is quite adequate for our argument at present.¹ We can now, if we incorporate the

¹ There may well be even more serious inadequacies, cf. the criticisms regarding semantic inadequacy in Jackendoff (1971b), and several of the syntactic arguments supporting Lakoff and Carden are also rather weak. On both points see Chapter 5 for further discussion. It should become clear, as the argument develops, that alternative analyses, such as those in Jackendoff (1968) and Vetter (1968), are even less adequate, but Jackendoff's proposals will be considered more fully in Chapter 6.

proposal made above regarding the underlying structure of both, provide an analysis of:

(4.5) Both children like cream
along the approximate lines of:



Two transformations operate on this phrase marker: the first is the rule of quantifier-lowering (or "Q-magic", cf. Carden (1968)), which wipes out S_0 and lowers the quantifier into S_1 ; the second will convert the lowered quantifier into both; the resultant surface structure is that for (5).

But notice now that a structure such as (6) does not necessarily point to a source for (1b). There are two reasons for this: the first of these is that it may be the case that quantifier-lowering applies only when an 'indefinite' NP fills the subject node, as in (6). That this is the case is suggested by the partial agreement of Lakoff (1970d:391) with the statement by Partee (1970:156) that:

"Quantifiers occur as predicates only with
indefinite noun phrases as subjects; quantifiers have some other source with definite

noun phrases."

But Lakoff's remarks are so vague that it is difficult to put any interpretation on them, and the situation is further confused by the fact that Carden (1968) ignores any difference between Quant of the N and Quant N sequences with respect to the operation of quantifier-lowering. Let us assume, therefore, that the first reason is non-existent in fact, and that the difference in underlying structure between (1b) and (5) is solely that where we find only children in (5) we find the children in (1b).

And this leads us to the second reason for suspecting any proposed relation between (1b) and (6): it is not convincing to claim that the difference in meaning between (1b) and (5) ought to be represented in underlying structure only by the contrast between presence and absence of the. Consider the further examples:

- (4.7) a John likes both books
 b John likes both the books
- (4.8) a John likes books
 b John likes the books

Whatever the difference in meaning between (7a) and (7b) may be, it can hardly be claimed that it is the same as, or even as great as, that between (8a) and (8b). Yet if we agree that the presence or absence of the is all that matters in underlying structure in order to distinguish between the two sentences of (7), and that is what we

have just said might be a plausible analysis, then we shall be distinguishing between (7a) and (7b) precisely and only in the same way as we distinguish between (8a) and (8b). And this is exactly what we must not do.

This argument is further reinforced by the fact that:

(4.9) All children like cream which we might think ought to be derived from a structure identical to (6) except that ALL would be [-dual], is quite clearly different from (1a) semantically. And the difference is that whereas (1a) is definitely non-generic, (9) is definitely generic, at least if we restrict the notion of generic which is being used here to cases where no existential reference is implied.²

The obscurities of generic sentences are great, but even so the difference between (1a) and (9) can be accounted for if we accept the claim made by Jespersen (1924:204) that plural nouns accompanied by the 'definite article' cannot have generic reference, but that plural nouns

² Dwight Bolinger (personal communication) has pointed out that the claim in Jespersen (1924) stated below is invalidated by examples such as:

- (1) The stars emit intrinsic light whereas
the planets emit reflected light

The qualification made here is intended to avoid such counter-examples.

unaccompanied by the 'definite article' may do so (of course, this must be restricted to nonexistential generics, as is pointed out in note 2, above). Thus in (10) the (a) sentence is nongeneric but the (b) sentence is generic:

(4.10) a The children like cream

b Children like cream

From the above it should be quite clear that all the children in (1a) cannot have generic reference, whereas all children in (9) may, and so (1a) is nongeneric, (9) is generic.

Returning now to (1b) and (5), it can be observed that, whatever slight differences in meaning and syntax there may be between the two sentences, it is not the case that there is an opposition between nongeneric (1b) and generic (5); both are indisputably nongeneric. But this involves us in two difficulties. Firstly, if the only difference in underlying structure between (5) and (9) is [dual], how can it be predicted that the former is nongeneric, the latter generic? Secondly, if (5) can only have a nongeneric interpretation, as is undoubtedly the case, then that would appear to contradict the well-established principle that plural nouns unaccompanied by the can have generic reference. These problems obviously have to be resolved. But apparently the only way to solve the first of them is to assume that there is a special constraint which blocks duals from appearing in

generic sentences, and such a solution is not particularly revealing, for even if it is the case that duals do not appear in such sentences, no explanation of why that is so will have been given, especially not one which relates to any of the known facts about generics in English.

However, there does seem to be a solution to the second difficulty. Comparing (1b) and (5), it is clear that the only surface structure difference is the presence or absence of the, and we shall see that it is this (deictic) element which is crucial. The purpose of a deictic element (and this applies to demonstratives and pronouns no less than to the 'definite article') is, in the first instance, to show that the reference of the relevant NP is to a given (already known to the hearer or presumed by the speaker to be so known) subset of the full set of potential referents of that NP, although this should not obscure the other coexisting functions of deixis, cf. note 3, below, and the references therein. Therefore, if no deictic element is present, the object or objects which are being referred to are only known, or presumed to be known, to the hearer in terms of the full set of potential referents. Thus, when someone hears:

(4.11) — Some children like cream

all that he knows, in theory, is that the referents of the subject NP are at least two but no more than n-2

members of the full set of potential referents of child-
ren, where that set has n members. Of course, the
existence of the related quantifiers many, a few and few
allow the hearer to guess that the answer is near the
middle range of possible answers, but the important
point to note is that that is only a reasonable guess on
the part of a reasonable hearer; for the absence of a
deictic element indicates that there is no given subset
to which the hearer should refer. Now, in (1a) the
referents are all the members of a given subset (of
children), but in (9), where no deictic element is
present, the referents are all the members of the full
potential set. In other words, the presence of a deictic
element indicates reference in terms of a given subset,
the absence of such an element indicates reference in
terms of the full set.

On the other hand, when we consider the correspond-
ing cases with both, i.e., (1b) and (5), it would appear
that in each case, the latter as well as the former,
reference is in terms of a given subset, containing two
and only two members, of the full potential set of
referents, and this despite the seeming absence in (5)
of any deictic element. To confirm this assertion, let
us consider what happens when a speaker refers in terms
of a subset - i.e., uses a deictic element with the
relevant NP - of which the hearer has no knowledge. Let
us suppose the speaker says:

(4.12) The children like cream

To this, if he does not yet have sufficient information about the relevant subset, the hearer is entitled to say, somewhat querulously even:

(4.13) What children are these? You
haven't told me about them

But, if the speaker says:

(4.14) Many children like cream

where reference is made in terms of the full set rather than a subset (and the fact that (14) has a generic interpretation confirms this), then the only type of legitimate question for the hearer with respect to set composition is one which asks if it is possible to define a subset, as in:

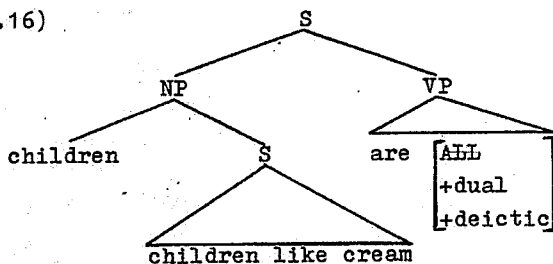
(4.15) Do you happen to know which
particular children?

He cannot complain, by way of (13), that a necessary subset has not been given. Now, with both (1b) and (5) the ignorant hearer is entitled to ask (13), and (15) is as inappropriate for them as it is for (12). In other words, both (1b) and (5) presuppose a given subset, acting as if a deictic element were present, although, apparently, none is present in the surface structure of the latter. Such a claim can always be checked by the relevance of questions (13) and (15); the former is relevant only if reference is in terms of a subset not known to the hearer, the latter only if reference is in terms of the full potential set. And so our claim is

confirmed. It might also be noted that the examples in (7) equally show that the presence or absence of the in surface structure is irrelevant in so far as this does not affect the terms in which reference is made.

If we now return to the analysis of quantifiers proposed by Lakoff and Carden, one solution which appears attractive is that which adds a further feature [+deictic] to ALL, in order to give as an underlying structure for (5):

(4.16)



but the disadvantages of this solution should be clear. Firstly, it seems highly unlikely that a feature such as [+deictic] can be added to the specification of quantifiers, especially if they are deep structure VP's or predicates. It seems a reasonable assumption that the kind of deictic element which introduces, for example, a 'definite article', does not appear in the analysis of

predicates, which do not refer.³ Secondly, this further feature specification seems in any case to be ad hoc; it offers no explanation but rather a quick exit from a still unresolved difficulty.

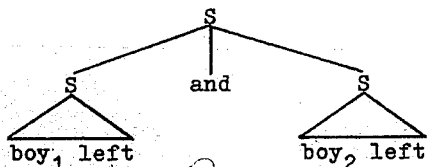
4.2 'Both' as a deep structure coordinator

Before attempting to reach a more adequate solution in terms of the Lakoff-Carden hypothesis, it may be useful to discuss an alternative solution to the problem of both which has been proposed in Carden (1970a). Carden suggests that both be derived from a deep structure sentence conjunction. Thus:

(4.17) Both boys left

is to be derived from:

(4.18)



A rule of "Both-Formation" (BF), which is a variant of

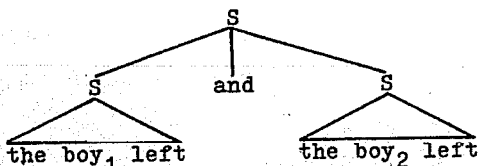
³ This does not exclude the presence of every type of deixis from VP's, for deixis may be associated with verbs such as come and go, cf. Fillmore (1966a). But in this context Anderson (1971b:122-23) argues plausibly that even then the deixis is contained within an N.

the well-known Conjunction Reduction rule, cf. Chomsky (1957:36), Carden (1970a:181), is then used to derive (17). Although it is almost certainly the case, as is argued by Lakoff and Peters (1969), that both is closely associated with sentence conjunction, it is not so certain that all instances of both can be derived in the fashion proposed by Carden, since the BF transformation does not account for the constant definiteness of the surface quantifier both, whether or not it is followed by the. Let us modify the BF rule so that it has the schematic form:⁴

(4.19) <the> N₁ and <the> N₂ ----->
 both <the> N_(1,2)

Thus (17) would be derived not from (18) but from:

(4.20)



and (18) would be the deep structure of:

(4.21) Both boys left

This appears to be at least a slight improvement on the BF rule proposed by Carden (1970a:185), which latter assumes that in the deep structure no 'definite article' is present, but that after the operation of BF the

⁴ The angled brackets follow the conventions in generative phonology, cf. Harms (1968:66).

'definite article' will always show up.⁵ So, although there is an explanation available of the syntactic difference between (17) and (21) there is still no explanation of the semantic difference, which, as we have pointed out, is not a 'definite' vs. 'indefinite' opposition. For an account of the status of rules which perhaps analyse the syntax but fail to analyse the semantics, see Lakoff (1971c:283) on "arbitrary syntax". But perhaps Carden can account for (21) by optionally deleting the 'definite article' in (17), rather than by a derivation from (18); yet this assumes that the two sentences are paraphrases of one another, a claim which we have not made and in fact would not make, and to which Carden makes no reference.

The modified BF rule is therefore unsatisfactory in so far as it does not clarify the basic semantic distinctions between (17) and (21), and the situation is made even more difficult by the fact that in sentence conjunction positions both does not have any deictic properties

⁵ As originally proposed, Carden's BF rule includes 'definite articles' in the relevant structural description, but in his examples there are none at this point, cf. Carden (1970a:181, 183-85). Therefore it is extremely difficult to ascertain what status he would assign to the 'definite article', and I apologise for any misinterpretations which might confuse or mislead the reader.

associated with it: there is deictic reference in (22) but not in (23):

(4.22) Both the men and the horses are
desperately needed

(4.23) Both men and horses are desperately
needed

Notice that as long as it is associated with sentence conjunction and is not a surface quantifier (which is the interpretation of (23) with which we are concerned), both can appear in sentences where the NP's have generic reference:

(4.24) Both teachers and children look
forward to holidays

and thus supplying further evidence that there is no element of 'definiteness' in such sentences. Interestingly, Carden's BF rule, combined with the possibility that that type of both which is involved in sentence conjunction may also appear in generic sentences, leads to the totally incorrect prediction that:

(4.25) Both boys have long hair
may be generic. There appears, therefore, to be good reason for assuming that Carden's hypothesis that in all surface structure positions both is derived from deep structure sentence conjunction is quite mistaken, especially as it fails to account for some of the most troublesome features of both.

4.3 A derivation for 'both N'

Having, I hope, shown that the derivation proposed by Carden fails to explain the deictic characteristics of both, let us now return to the earlier analysis suggested above, which proposed that both be derived from an underlying ALL with the feature specification [+dual, + deictic]. We have already demonstrated that such a solution is both implausible and ad hoc, and it would therefore appear to be the case that it is extremely difficult to derive both from an underlying structure which treats quantifiers as higher predicates. But the arguments proposed by, amongst others, Anderson (1968), Lakoff (1968) and McCawley (1968), in-favour of a theory of 'generative semantics', suggest that sweeping modifications of the base component which will dispense with such feature specification as used above may provide us with fruitful possibilities for our argument. This theory claims that quite 'simple' (in surface structure) lexical items must often be derived from comparatively complex (or, at the least, radically different) underlying configurations. In this respect compare the arguments of Anderson (1968) for deriving travel on foot into walk, or those of Lakoff (1968) for deriving kill from cause to die, and there are more extensive discussions of the whole theory in Anderson (1971b), Lakoff (1971c), Postal (1970) and, from a much more sceptical point of view, Bolinger (1971).

Consider the sentences:

(4.26) *All of the two children like cream

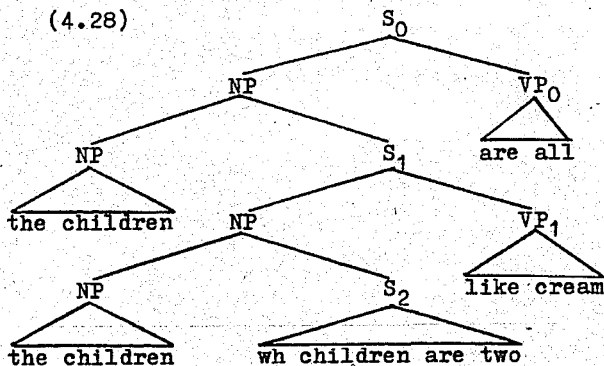
(4.27) All of the six children like cream

Notice that the former is ungrammatical, the latter is grammatical. Yet although (26) is ungrammatical, it is also a paraphrase of (5), and the two facts of ungrammaticality and the paraphrase relation to (5) allow us to construct a simple hypothesis, namely that both in (5) is derived by obligatory transformations upon an underlying structure which corresponds closely to something like (26). This seems reasonable, in as much as the surface difference between (26) and (9) is the phrase of the two; the is clearly a realisation of the feature we have called [+deictic], as in (16), since the 'definite article' is most probably the unmarked member of the class of deictics;⁶ two is obviously a realisation of the feature in (16) [+dual]; only of remains to be explained, and for the moment we may rest content with a description of it as the marker of partitive relation, which necessarily holds between a quantifier in pre-determiner position and its associated 'definite' NP, cf. Jackendoff (1968:428-29) and §10.3. In other words, the above hypothesis is able, given the assumption about the presence of of, to explain the following facts:

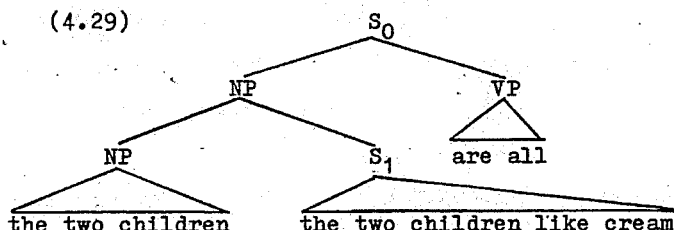
⁶ Other, more marked, members of this class include the demonstratives this and that and the possessive pronouns. For further discussion see Chapter 12.

(i) (26) is ungrammatical, which (27) is not; (ii) although (26) is ungrammatical it is a paraphrase of the grammatical (5); (iii) the difference (semantically) between (26) and (9) is exactly the same as the difference between (5) and (9).

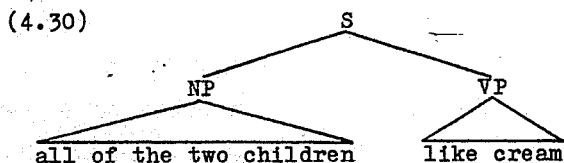
In order to capture the generalisations which flow from an analysis of (26), let us assume that we can derive quantifiers from a higher predicate even when there is a 'definite' NP collocating, perhaps in contrast to the unclear statement of Lakoff (1970d), but apparently in line with Carden (1968), see above. We must further assume that two has to be derived from a nonrestrictive relative clause when it occurs in the postdeterminer position exemplified in (26); for the argument behind this assumption see Carden (1970c) and compare Chapter 8, together with the reservations of note 1, above. Granted these assumptions, we can construct the following underlying phrase marker for (5):



Then, by the rules of Wh-be deletion and adjective preposing, cf. Smith (1964:251-4) and Lakoff (1970d:391), we obtain the following intermediate structure:



By the rule of quantifier-lowering, which in this case also inserts of before the 'definite article', (30) is derived:⁷



We propose that there then should be an obligatory Dual Copy transformation, which has the effect of mapping of the two onto all, giving a resultant both as the 'lexical formative', cf. Anderson (1968:308). This Dual

⁷ Of course, with all and both no of need be inserted before the. But to state that of is introduced between every quantifier and a 'definite article' and that it may then be deleted optionally after all or both seems to be the wider generalisation. Cf. §10.3 for further discussion.

Copy transformation may be more strictly defined as:

(4.31) all - X - Y - two -----> both

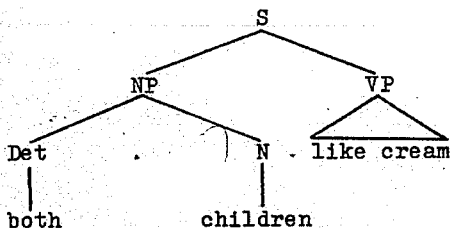
where X and Y may be zero; the reason for this will become clearer in the discussion below, and if we were to discuss the syntax of other quantifiers, for example either, a more precise formulation of it would be necessary, since Dual Copy has rather wider powers than are accounted for by (31). It might be noted, however, that this transformations performs the same functions as does subjunction within a dependency framework, which, for example, allows one to relate (32a) and (32b), cf. Anderson (1971c):

(4.32) a John gave me his help _____

b John helped me

The Dual Copy transformation will thus convert (30) to:

(4.33)



4.4 A derivation for 'both the N'

It will have been observed, however, that we have not yet provided an analysis for (1b), where the 'definite article' appears after both (of). One possible solution would be to claim that (1b) and (5) are

paraphrases of one another. We could then have an optional variant of the Dual Copy transformation which would permit retention of the (=Y in (31)). But there are strong arguments against this. Firstly there is the simple point that we shall have to add a condition to rule (31), and that this is a (admittedly slight) complication of the grammar. Quite obviously, if we can derive (1b) without any modification of the Dual Copy rule, that would be preferable. Secondly, as we noted in the discussion of Carden (1970a) in §4.2, it is not at all clear that the paraphrase relation we have described above actually exists. One's doubts seem to centre on the fact that whereas both in (5) appears to fulfil two purposes - being both deictic and emphasising the 'twoness' or duality of the NP - in (1b) both seems to fulfil only the latter purpose; in other words it carries no deictic reference (which is in fact, and quite naturally, carried by the 'definite article'), but only emphasises that two and not just one of the two children like cream.

As opposed to the negative evidence of the preceding paragraph, there does appear to be a certain amount of positive evidence in English grammar to suggest that a mechanism whereby the is optionally retained is inadequate as an explanation. Consider firstly:

(4.34) All six of the children like cream.

I would suggest that the purpose of all in this sentence

is to emphasise that six and not just five (or four or three, etc.) of the children like cream, and that, further, all has no deictic function here. This claim about the purpose of all in (34) is perhaps not acceptable when stated as baldly as this; but a more adequate characterisation of the quantifiers than is possible within the confines of the Lakoff-Carden theory will surely reveal that all does have a primary function of emphasis here, cf. Chapter 9. Now, it is possible that one might wish to derive (34) from (27), or vice versa, and thus, as we shall see, further claim that (5) and (1b) have the same underlying structure, which is essentially the negation of the argument presented here. But we might note the following two objections: firstly, the non-existence of a paraphrase relation which would permit a meaning-preserving transformation; secondly, the acceptance of an additional transformation entails an undesirable complication of the grammar. And so such a proposal cannot be admitted.

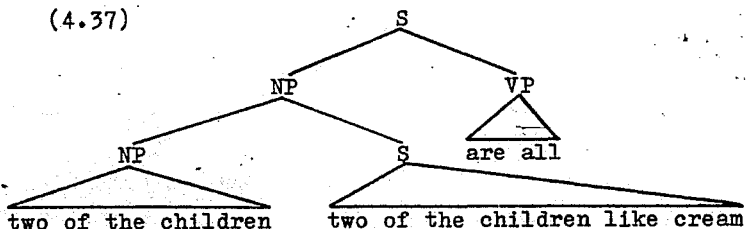
But now note that (35) might be expected to show exactly those features represented in (34):

(4.35) *All two of the children like cream
and that is the case, but it also happens to be the case that (35) is ungrammatical. However, a comparison of (34) and (35) with (26) and (27) reveals that the ungrammaticality of both (35) and (26) can be accounted for by the same fact, namely that all ... two is not a

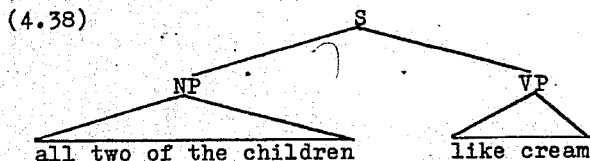
permissible surface structure sequence. Further, our previous arguments point to the fact that (35) is identical in meaning to:

(4.36) Both of the children like cream

Therefore we should be able to state a transformational relationship between the two sentences; and indeed this can be accomplished with the transformational apparatus at hand. An intermediate structure for (35), which regrettably fudges a decision about the precise source of two, is:⁸



By the rule of quantifier-lowering we obtain:



If we now apply the Dual Copy transformation as stated

⁸ The fudge is perhaps necessary, since it is very difficult, if not impossible, to determine a plausible source for two here within the Lakoff-Garden theory. This is one (minor) reason for the critical remarks of note 1.

in (31), all two (with X and Y both zero) will be copied as both, which results in the surface structure of (36). Thus the combination of quantifier-lowering and Dual Copy, both preexisting rules, will derive (36) without any additional rules being needed, and, further, will account for the ungrammaticality of (35). We can therefore state that the difference between (5) and (36) is expressed exactly by the difference between (26) and (35). There still remains one problem about (1b), namely of, but for the moment we shall follow note 7 and assume that of may be optionally deleted in those cases where it remains after the Dual Copy rule immediately following all or both, i.e., underlying all.

There does appear to be some further slight evidence for the derivations proposed above, which rests on the admittedly tenuous fact that whereas (39) is perfectly acceptable, (40) is only marginally so, if at all:

(4.39) Both the children who came to the
party like cream

(4.40) ??Both children who came to the party
like cream

To explain this contrast we have to enquire further into the circumstances in which both is used as a quantifier without following (of) the. Let us use for our enquiry sentence (5). As is obvious from the 'definiteness' of both children, and from our proposed derivational history (28) - (30), (33), both is employed in such

situations when, as has been said, the exact composition of the particular subset of children being referred to is presumed by the speaker to be known to the hearer. Now since such a presupposition is unwarranted by (5) itself, it must be warranted by previous reference either to the particular subset of children being referred to or to some object(s) which define(s) that subset uniquely. Thus the type of reference which both has in (5) is anaphoric, where we define anaphoric reference to be reference either to the referent of an expression which has occurred previously or to a referent which has been uniquely defined by a previous expression, cf. §12.3. In this respect it might be noted that it would seem unfortunate to restrict anaphora to the case of reference within one sentence, as does Dougherty (1969: 488) when he claims that a pronoun has anaphoric reference only when:

"it can be understood as being coreferential with some other noun phrase in the sentence."

Again, compare our remarks in §12.3.

What I want to suggest now is that in that variant of the Dual Copy transformation which involves deletion of the, only anaphorically-derived the may be present; this entails that the underlying the in both children is anaphoric, but has no implications for underlying the in both the children. Now, as Vendler (1967:52-53) has most persuasively argued, cf. §3.4, any NP containing a

'definite article' which is anaphorically-derived (whatever mechanism is necessary for such a derivation, cf. Chapter 12) cannot have dependent upon it a restrictive relative clause. Therefore, if the restriction which we have placed upon the Dual Copy transformation is correct, (40) should be ungrammatical, and that of course is the case. But it may be that we do not even need such a restriction, for it is possible that the different derivational histories of anaphoric and non-anaphoric the are sufficient to account for the facts which we discuss here; this point will be taken up again and elaborated upon in §12.3.⁹ Furthermore, note that it will not be the case that (39) will be predicted to be ungrammatical, for the underlying the in (39) is not deleted transformationally, and so that instance of the 'definite article' may be either anaphoric or non-anaphoric. In (39) it is in fact the latter, as opposed to that in (40), for the subset signified by the is defined by the restrictive relative clause who came to the party (and is hence cataphoric). Only if we accept that two alternative structural analyses undergo Dual Copy, and that one of these involves the-deletion while the other

⁹ The marginal status of (40) may perhaps be due to its close resemblance in surface (and, indeed, phonological) structure to (39). I feel quite certain that such resemblance is the prime reason for any possible margin of acceptability which (40) has.

does not, can we account for these facts. And so we may conclude that there is a certain amount of syntactic evidence within English grammar to justify the different derivations for both N and both the N.

4.5 Some additional arguments

We have seen above that both is not a simple quantifier, in the general sense that we might use to describe all or some. Rather, it is a complex of various elements: a quantifier of totality, a quantifier of duality and a deictic element. Nevertheless, it has been possible to provide derivations for both which involve only one addition to the transformational apparatus at hand, namely the Dual Copy rule. Further, we have been able to demonstrate that it is necessary to derive both N and both the N from different underlying structures, yet at the same time no further addition to the transformational apparatus is needed. There is therefore some justification for confidence in at least the fundamental characteristics of the analyses proposed.

Since the above hypothesis, however, may be of some wider interest, in that it favours a grammar in which lexical items may be derived from considerably more complex underlying structures, it would be useful to discover further facts which might confirm or disconfirm the hypothesis. Below are listed some four points which

are worthy of consideration, although they are not necessarily of equal weight. But they all have in common the fact that they support one or other part of the above proposals.

Firstly, it is quite obviously the case that the derivation of both from a structure including underlying all will help us to account quite simply for the close parallels of distribution between the two quantifiers; for we shall be able to state such regularities as the optional (at least in British English) deletion of of to give both the, all the, in terms of a transformation upon one underlying quantifier rather than on two distinct (possibly unrelated) quantifiers. Transformations which operate on two quantifiers in their attempt to account for such distributions, as, for example, that in Jackendoff (1968:429), are essentially ad hoc, since they fail to express the correct generalisation.

If we look more closely at the distribution of the two quantifiers, however, we find an interesting asymmetry of pattern, as exemplified in:

- (4.41) a The boys all have long hair
b The boys both have long hair
- (4.42) a Boys all have long hair
b *Boys both have long hair

The problem is: why is (42b) ungrammatical, in contrast to the grammaticality of both (41b) and (42a)? If both

and all were different but somehow related quantifiers, as in Jackendoff's system, there could be no non-ad hoc solution. But within the terms of the hypothesis presented here, there does appear to be an explanation. This is that the is not permitted to move to the right of its noun, and it must be left behind when both is shifted as in (41b), despite Dual Copy. Now this appears to be equally ad hoc, but consider the following Dutch sentences (admittedly somewhat archaic):

- (4.43) a. Beide jongens hebben ...
 ("Both boys have ...")
 b. De beide jongens hebben ...
 ("The two boys have ...")
 c. *Jongens hebben beide ...
 (*"Boys both have ...")
 d. De jongens hebben beide ...
 ("The boys both have ...")

What is crucial here is that the semantics of Dutch point quite clearly to (43d) being related to (43a) rather than to (43b). It therefore appears that when beide is postposed, then the 'definite article' must be left prenominal. Otherwise, the paradigm of (43) can hardly be accounted for. But this account of Dutch beide (with its implicit consequences for the analysis of the English sentences (41b) and (42b)) is only possible if it is accepted that our hypothesis, that both and its Dutch equivalent involve an underlying the, is correct.

The comparison with Dutch leads to our second subsidiary argument. It cannot be expected that every language must have a lexical item identical in meaning to both; but if a language does not have such an item then we might suggest that in many cases this might simply be because there is no version of the Dual Copy rule in that language. It is therefore instructive to consider the French translation of both, namely tous les deux, i.e., the structure proposed here for English before the operation of Dual Copy. If it can be shown that there are a number of languages like French rather than English in this respect, this would be strong evidence in favour of this chapter's hypothesis in general and Dual Copy in particular.

The third piece of evidence stems from the fact that there are in English two other dual quantifiers like both, that is to say, quantifiers whose presence in an NP shows that the reference of that NP must be to two and only two objects; these items are either and neither. Now it is quite simple to demonstrate that if these quantifiers are derived in exactly the same way as both except that all is replaced by any for either and by neg + any for neither, then we can account for their distribution. Thus we find:

- (4.44) a *Any boy passed the exam
b *Either boy passed the exam
c Did you pass any boy?

- d Did you pass either boy?
- e No boy passed the exam.
- f Neither boy passed the exam

Further support for this thesis is found in the observation of Kirwin (1968) that either is used for any in the Newfoundland dialect of Canadian English. The probability that Dual Copy and the accompanying underlying structures can be generalised over a class of lexical items rather than being confined to one item only further strengthens the hypothesis presented here.

The fourth and final point concerns the contrast between anaphoric and non-anaphoric the, and the claim made above that the former is present in the underlying structures of both N, but need not underlie both the N, which is derivable from non-anaphoric the. Consider the following sentences:¹⁰

(4.45) Both (of) the Irish delegates are
here already

(4.46) Both Irish delegates are here already

It seems to be the case that the conditions under which (45) is appropriate are different from those for (46).

¹⁰ Sentences (45) and (46), and the remarks below concerning their contextual appropriateness, are due to Dwight Bolinger (letter of 13/6/72), but the conclusions drawn are the present writer's responsibility and all blame should be attached to him.

Thus, if two people come into an assembly room and they are previously unacquainted, after one of them has looked around the room he may say to the other, in an attempt to break the ice, sentence (45). But (46) could hardly be used in the same context, unless there were an intonation rise on already. But in that case it would have to be assumed that both speaker and hearer knew certain facts about the Irish delegates, or Irish delegates in general, and the speaker knew that that was the case, or was attempting a particular type of joke. Both these instances are surely irrelevant here. What we should note is that only at a later stage in the conversation, perhaps after the Irish delegates have been mentioned, would a both N construction, as in (46), be appropriate.

What conclusion may we draw from the facts that both of the N is more appropriate for the first speech-occasion and that both N only really comes into its own on further speech-occasions? If (46) involves an underlying anaphoric the, as we have argued above, it is quite simple to see why it is inappropriate on the first speech-occasion: there is nothing in the context of situation, let alone discourse, to provide an anaphoric reference. On the other hand, the in (45) is not necessarily anaphoric, and therefore (45) is acceptable on the first speech-occasion. Later in the conversation, when anaphoric reference has been established, both N is acceptable. The possibility of the above explanation

only comes with the contrast in underlying structures which our hypothesis proposes, and therefore it is an additional justification of that hypothesis.

These four additional arguments all confirm the initial hypothesis, and, presumably, whatever wider conclusions may be drawn for the theory of grammar from the hypothesis. Therefore we may claim to have demonstrated that our statement that we have given an adequate description of both, stated at the conclusion of §4.4, is indeed by and large correct. But one major problem remains: not only have we nowhere justified the rule of quantifier-lowering and hence the general principle that quantifiers are underlying higher predicates, but we have even displayed a considerable degree of scepticism regarding its correctness. This hypothesis, which we have named the Lakoff-Carden analysis, must therefore be critically examined in the following chapter.

Quantifiers as predicates

5.1 The Lakoff-Carden analysis

The Lakoff-Carden analysis of quantifiers¹ claims that, depending upon their surface structure status, quantifiers are in underlying structure predicates in one of the following structures: (i) higher sentences; (ii) restrictive relative clauses; (iii) nonrestrictive relative clauses. But not all these structures have equal status in the theory. Thus the claim that quantifiers derive from predicates in restrictive relative clauses, which first appeared in Lakoff (1970b), was subsequently rejected in Lakoff (1970d) following

¹ What is referred to here as the "Lakoff-Carden analysis" has been advanced in a large number of papers whose chronology is obscured by publication dates. From internal and external evidence, however, it is possible to suggest the following order of chronological priority for the more important of these papers: Lakoff (1970b), Carden (1968), Lakoff (1970d), Carden (1970b), Carden (1970c), Lakoff (1971c). Lakoff (1970b) was written in 1965, Carden (1968) is a revised version of Carden (1967), to which latter I have not had access, and Lakoff (1971c) is in part a revision of Lakoff (1969).

criticisms made by Partee (1970). The rejection is as follows (Lakoff, 1970d:400):

"In my dissertation I claimed that

(23) Did many inmates escape?

was ambiguous. In one sense it is presumed that some inmates escaped, and it is asked whether the number was large. In the second sense, no escape is presumed. It is only presumed that the speaker is discussing many inmates and asking whether they escaped ...

A sober post-dissertation look at the sentence shows that I simply had the facts wrong. So far as I can tell, the second sense simply does not exist. I also agree with Partee's critique of the mechanism I had set up to account for that sense of (23), namely, deriving the quantifier from a restrictive relative clause on an indefinite NP ..."

Since that particular claim is incorrect, and has been acknowledged as incorrect by Lakoff himself, there is no need to discuss it further. The claim that quantifiers appear as predicates in higher sentences, i.e., as VP_0 in (4.28) was made in the earliest papers, namely Lakoff (1970b:175) and Carden (1968:2), and has remained present in all the variants of the Lakoff-Carden theory (although Lakoff (1971c:239) presents a variant which,

it is claimed, is "closer to reality", whatever that may mean, cf. below and note 12). That quantifiers may have their source as predicates in nonrestrictive relative clauses is first made in Lakoff (1970d:400-2) and Carden (1970c:416). This claim is best considered as a replacement of the one that quantifiers have one source in restrictive clauses. Carden (1970c) demonstrates that there is a good deal of evidence that only postdeterminer quantifiers, i.e., those in the position of X in the X boys, are so derived. An example of such an underlying source is the position of are two in (4.28), see above.

This brief summary should make it clear that the analysis of quantifiers as predicates has undergone some modification since its original appearance. But even so, I think that it is also the case that the argumentation for such an analysis has not been sufficiently closely and exhaustively examined, and it is such a thorough examination which it is hoped to present here. Apparently there are three questions to be asked: (i) do quantifiers appear as predicates in underlying structures? (ii) have quantifiers a source in a higher S? (iii) have quantifiers a source in a nonrestrictive relative clause? Further, we might ask whether it is it is the case that (ii) and (iii) can only be valid questions if the answer to (i) is affirmative. That there is to some extent such an internal ordering

relationship cannot be doubted. To clarify this abrupt statement, consider the remarks of Carden (1968:5), who, after arguing that quantifiers are derived from higher sentences, states:

"This doesn't show that it [the quantifier: RMH] is the verb of that S; but, within the present theory, what else could it be? The S must have an NP and a VP, and the NP is needed to identify the NP the quantifier modifies in the embedded S."

In other words, if we are to accept that quantifiers are derived from higher S's, then they must be predicates, for there is no other point in underlying structure at which they might be placed.

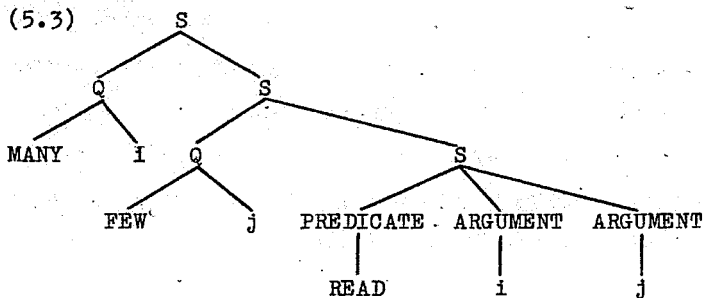
In this respect it might be mentioned that the structures footnoted in Lakoff (1971c:239) do not appear to correspond to this theory, for here, although quantifiers are in a higher S, they are directly dominated by "Q" and appear to be neither 'predicates' nor 'arguments'. Thus, for the interpretation of:

(5.1) Many men read few books

which is paraphrasable by:

(5.2) Many are the men who read few books

Lakoff suggests that the following representation most closely approximates to the underlying structure:



Without any explanation of this structure on Lakoff's part it is extremely difficult to discuss it, and it is perhaps wisest to assume that in principle it assigns a status to the quantifiers not radically different from that in earlier works by Lakoff. Only one comment seems possible at the moment: if many, etc. are to be dominated in underlying representations by "Q", then presumably we are to deny any possibility of explaining the behaviour of quantifiers in terms of any generalisations which may be possible with reference to the predicates or arguments, which seems unfortunate. But even so, (3) may not be the 'deepest' structure, and in that case this criticism is misplaced, and Lakoff has simply been misleading. Even if it is assumed that (3) is some kind of variant of the notation of symbolic logic, it is not possible to define the status of "Q" unambiguously. This is because Lakoff fails to relate clearly (3) to any extended rule mechanism. However, see below, §5.5.

To return to the context of the original Lakoff-Garden proposals, we can see there that if quantifiers

have their source in a higher S, then they must be predicates. The position with regard to the nonrestrictive relative clause source is rather different, partially because of the restricted set of quantifiers which may appear in such contexts, but one's conclusions must be broadly similar: the relevant quantifiers must be part (at least) of a predicate at some stage in the derivation of the sentence, but see below, §5.3, for a fuller discussion of quantifiers in postdeterminer position within this theory. However, we must conclude at the moment that the primary hypothesis is that quantifiers are underlying predicates, and that the hypothesis that quantifiers are derived from a higher S source is secondary.

It is necessary to emphasise this distinction for it is easily confused. For example, the arguments of Carden (1968), reproduced in part above, do appear to claim that quantifiers are predicates because they are derived from higher S's. On the other hand, Lakoff (1970b:175) makes use of comparisons with the behaviour of 'true' adjectives, which are unarguably derived from predicates, cf. Lakoff (1970b:115-33), to suggest that quantifiers are predicates too, and only then does he suggest that quantifiers are derived from higher S's. Undoubtedly the position taken by Lakoff is preferable to that taken by Carden, and the reason for this is quite simple. Carden's argument may be condensed as:

since quantifiers must be derived from higher S's, then they must be predicates; Lakoff's argument can be paraphrased as: since quantifiers are underlying predicates, they may be derived from a higher S source. In Carden's case there is a formal claim made about quantifiers, but in Lakoff's case there is an empirical claim which has possible formal consequences.² The hypothesis which subordinates formal claims to empirically-verifiable facts, in this case Lakoff's hypothesis, is indisputably superior to that which does not do so, i.e., Carden's.

Having established that question (i) - are quantifiers predicates? - is the primary question, we are now faced with the unfortunate fact that this question is in itself two questions rolled into one; for, in order to make the claim that quantifiers are predicates, it is firstly necessary to determine that they are not at all stages of derivation constituents of the NP which, to use a traditional term, they modify in surface structure. Each argument in favour of the predicate status of quantifiers will therefore have to be examined with this in mind. The reasons why there are two questions and not one should be quite clear: it is only if quantifiers

² For discussion of the multiply ambiguous use of 'formal' in linguistic writings see Lyons (1968:135-37). It is used here in the sense which Lyons contrasts with 'substantive'.

cannot be considered as constituents of the modified NP at all stages that they must be considered as something else at one stage or another. However, what that "something else" must be is still at that point a matter for debate. There are at least four possibilities: (a) sentences; (b) nouns; (c) verbs (predicates); (d) quantifiers, where that would be a special category, perhaps like the "Q" of (3) above, or like that suggested, perhaps not seriously, by Force (1968), who includes the and the partitive of amongst the elements dominated by a Q node. To my knowledge no one has suggested alternative (a). Jackendoff (1968) suggests that some quantifiers are nouns but that others are 'articles'. This is slightly reminiscent of Force (1968), since 'articles' appear to be roughly equivalent to items dominated by Q, and it is also the case that both Force and Jackendoff work within an interpretivist framework. But it would be foolish to push the comparison too far. Alternative (c), of course, is the Lakoff-Carden analysis.

There appear to be four major arguments which have been explicitly formulated in favour of the Lakoff-Carden proposals, and these may be termed "Equi-NP Deletion", "'Archaic' constructions", "Negatives and quantifiers", and "Logic and linguistics". At least the first three of these have been discussed fully in the literature, cf. especially Lakoff (1970b and d), Carden (1968), Jackendoff (1971b) and Partee (1970). There are a

number of other arguments, but strictly speaking they are dependent upon one or other of the above, and therefore need only be considered if the above arguments are correct. We shall now consider each of the above arguments separately, commencing with Equi-NP Deletion.

5.2 Equi-NP Deletion

The clearest presentation of the argument from Equi-NP Deletion is to be found in Carden (1968:5-7), where it is demonstrated that since:

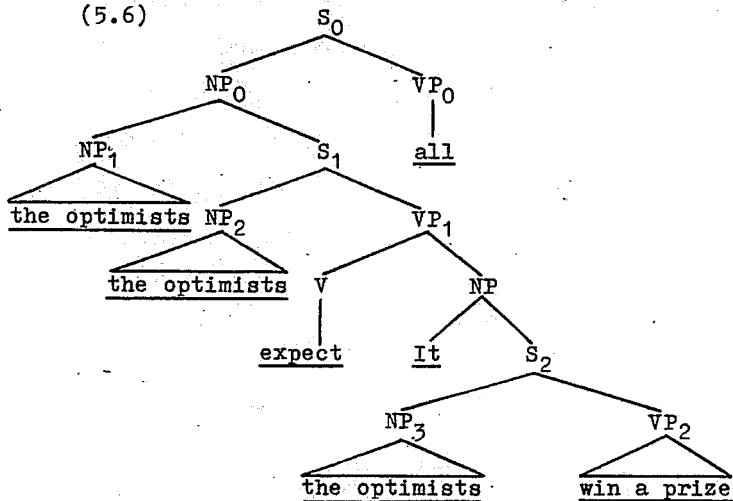
(5.4) All optimists expect to be President

(5.5) All optimists expect all optimists
to be President

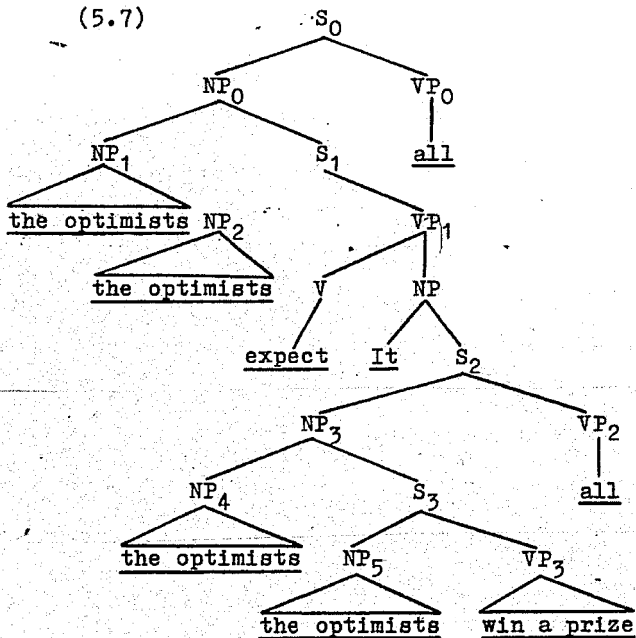
are not paraphrases of one another, the generative semantics theory of grammar demands that they have different underlying structures, the one for (4) permitting Equi-NP, the one for (5) not doing so. (6) and (7) are such underlying structures, for if we accept quantifier-lowering as ordered after Equi-NP then the appropriate NP in (7) will not be equi-deleted because of its failure to meet the required identity condition, cf. Carden (1968:7), Jackendoff (1971b:285):³

³ The earliest published description of Equi-NP Deletion is to be found in Rosenbaum (1967:6), where it is called Identity Erasure.

(5.6)



(5.7)



But, as Jackendoff (1971b:286-87) points out, there is the difficulty that noun phrases which are in surface structure quantifier-less also display the same semantic differences as exist between (4) and (5):

- (5.8) a Senators from New England expect to be treated with respect
- b Senators from New England expect senators from New England to be treated with respect
- (5.9) a Obnoxious people generally want to be rejected from society
- b Obnoxious people generally want obnoxious people to be rejected from society

This problem was noted by Carden (1968:44-45), where he proposes the solution that sentences such as (8) and (9) have "disappearing quantifiers in their deep structures". This is slightly modified in Carden (1970b:287), where the "disappearing quantifier" is claimed to be a generic, and we may, at least for the moment, follow Jackendoff (1971b:287) when he suggests that this solution is quite inadequate in the light of the rather different syntactic behaviour of generics and quantifiers.

However, it is very probably the case that neither Carden's arguments for nor Jackendoff's arguments against the use of higher predicates to solve the Equi-NP problem are logically impeccable. Consider the following

argument: (i) there are independently-motivated reasons for assuming that quantifiers are higher predicates in underlying structure; (ii) if there are these other reasons for deriving quantifiers from higher predicates, there is then a mechanism to avoid the semantic neutralisation between (4) and (5) which the rule of Equi-NP Deletion would otherwise engender, assuming that transformations do not change meaning (for the history of this assumption see Katz and Postal (1964:32) and Partee (1971:4-8); (iii) given the mechanism of the second stage of this argument, then it is preferable to assume that there is a "disappearing quantifier" in (8) and (9), for then the lack of neutralisation in those sentences can be explained by an already existing device.

Jackendoff (1971b) attacks the first and third stages of this argument, but since his attack on the third stage is apparently intended to be valid independently of whether his attack on the first stage (which is almost a little less than muted) is right, it is only the attack on the third stage which I wish to discuss. If stages (i) and (ii) are correct, then it is quite indisputable that this is strong evidence for the correctness of stage (iii), since it is clear that they provide evidence about the possible existence of "disappearing quantifiers". It may be true that the evidence which they provide is to some degree in conflict with other semantic and syntactic evidence which

involves the behaviour of the verbal auxiliary system, but even if that conflict is inevitable if only one generic quantifier - which conveniently "disappears" at the right moment - is used, it may yet be possible to find another quantifier-type source for the cases involving quantifier-less NP's. It could be, for example, that there is an appropriate adverbial source, and if we follow Lakoff (1971a and b) then this source would also be a higher predicate in underlying structure. Indeed, we could go further: given that adverbs are connected with the verbal auxiliary system and that adverbs are at least sometimes derived from higher sentences, just like quantifiers, the suggestion by Carden that quantifiers and generics are related categories is not as absurd as Jackendoff claims. If adverbs and quantifiers are higher verbs, then it is by no means clear that it is correct for Jackendoff (1971b:287) to state that:

"... the theory of GENERIC as a higher verb entails an apparent LOSS of generality."

The claims of Lakoff and Carden, if they are correct, go a long way to showing that only an analysis of 'generic' as a higher verb avoids a loss of generality, no matter how difficult the syntactic problems which will be encountered may be.

But even if Jackendoff's arguments do not necessarily show that the three-stage argument constructed above is incorrect, they undoubtedly have considerably

more force against Carden's arguments, because these latter do not correspond to the argument constructed above. More precisely, it is not at all clear that there are any independently-motivated reasons for assuming that quantifiers are derived from higher predicates. Carden (1968:4) states:

"There are three pieces of evidence showing that quantifiers come from higher sentences: Kuno's hypothetical verb EXIST which must be postulated in order to explain certain sentences with two "some's"; the "Everyone expects to die" sentences; and the Not-Transportation rule."

Carden only discusses the latter two, and I have been unable to uncover any more illuminating reference to Kuno's unpublished paper. However, we might remark that if the hypothesis that quantifiers are derived from higher predicates is to be justified by reference to a hypothetical verb, then first the latter has to be justified; thus the derivation of quantifiers as proposed by Lakoff and Carden will be only a remotely testable hypothesis. On the other hand, there also appears to be a certain amount of evidence which Carden fails to mention above, but this can await a discussion below in §5.3, when we shall see that it is far from conclusive.

There are, therefore, only two decidable arguments in Carden's paper which favour the theory that quantifiers are underlying higher predicates: Equi-NP Deletion and Neg Transportation. This, of course, is a reversal of the position we originally assumed, and implies that we have indeed, as Jackendoff claims, to inquire whether the mechanism of Equi-NP is correct on independent grounds. But, perhaps more importantly, there is the question of whether Equi-NP demands that quantifiers are higher predicates. Now what is interesting about this question is the fact that the demand has not been proved, but only assumed; Carden (1968:5) says:

"We conclude that the quantifier must not be inside its NP at this time. Presumably [*my italics:RMH*] then, it is in a higher S."

In other words, this quotation demonstrates only that the argument from Equi-NP shows that the quantifier must not be within the relevant NP at the time of its deletion, and it does not show that quantifiers are higher predicates. In this respect, it is interesting to compare the remarks of Lakoff (1971c:238):

"The main point at issue is whether quantifiers in underlying semantic representations are in a higher clause than the NP's they quantify (as in predicate calculus) or whether they are part of the NP's they

qualify (as they are in surface structure)."

Even if the first of these alternatives is correct, it is an assumption to then claim that quantifiers are higher predicates. We may therefore conclude that Equi-NP is not an argument in favour of the Lakoff-Carden analysis, except in so far as, if quantifiers are higher predicates, then Equi-NP does not contradict this, ceteris paribus. Therefore the three-stage construction of our argument is correct and since Carden's arguments in relation to Equi-NP do not correspond to that, they are incorrect.

Notwithstanding the above, it is perhaps still necessary to examine Carden's solution to Equi-NP to see whether or not it is correct, leaving aside the above remarks. There appear to be three arguments against his solution: (a) it is semantically inadequate; (b) it demands "disappearing quantifiers", cf. above; (c) Equi-NP is itself dubious. The first of these arguments is succinctly stated by Jackendoff (1971b:286):⁴

"One trouble with this [Carden's:RMH] solution is that the difference between the underlying structures of Figures (6) and (7) does not adequately characterise the semantic

⁴ In the following quotation I have altered Jackendoff's original numbering to conform with the numbering for this chapter.

differences between (4) and (5). In particular, (4) means that each optimist individually expects himself to win a prize, but he does not necessarily have any expectations about the fate of the other optimists.

Figure (6) gives no more indication than Figure (7) that each optimist is concerned only with himself. Thus, while the proposed underlying structure for all does produce the correct strings 4-5, it still fails to account for the semantic differences between them."

Jackendoff's argument seems to be both correct and in no need of further elucidation.

With regard to the second argument, we are here dealing with the correspondence between Carden's analysis and stage (iii) of our argument above. It would seem quite natural to allow "disappearing quantifiers" if the nature of quantifiers were independently such that it provides a solution to Equi-NP, but of course that position is the reverse of the case. What we actually have is a situation where Equi-NP both provides an argument in favour of quantifiers as higher predicates and an argument against that. The need to postulate "disappearing quantifiers" is the argument against, for this involves the introduction into the grammar of categories and rules which might otherwise be unnecessary.

In other words, although it is possible that the analysis of quantifiers presented by Carden may simplify in one respect the (semantically inadequate) grammar, in another respect it complicates it. This suggests that Equi-NP is at least (for Carden) an inconclusive argument.

The question of whether Equi-NP is dubiously formulated, or perhaps whether there should be that kind of transformation, follows from the first two arguments. The semantic distinction between (4) and (5) is undoubtedly that which Jackendoff has stated, and the introduction of higher quantifiers which 'disappear' under unspecified conditions is the solution proposed by Carden. But as Jackendoff (1971b:286) points out, the problem is at least one of coreferentiality. This can be seen if we consider:

(5.10) The masochists whipped themselves
which is clearly ambiguous: either each masochist whipped only himself, or each masochist whipped himself and all the other masochists. This implies that the mechanism for Equi-NP is extended to pronominalisation. That may not be all that unfortunate, since pronominalisation is not crucially dissimilar from Equi-NP Deletion; but since it is a rather later transformation than Equi-NP, cf. Carden (1968:45), Lakoff and Ross (1968), it must be ensured that quantifier-lowering does not intervene. What is a good deal worse, however, is that the same

ambiguity applies in many sentences which are not subject to Equi-NP or pronominalisation, but merely have the form [NP_{pl} - V - NP_{pl}], as in:

- (5.11) a The boys kissed the girls
b The sadists whipped the masochists
c The mice frightened the elephants

Each of the sentences in (11) is ambiguous; thus (11a) means either that each of the boys kissed one of the girls or that each of the boys kissed all of the girls.⁵ Furthermore, there appears to be a sliding scale of preference: in (11a) the preferred reading seems to be the first; in (11b) each reading seems to be about equally preferable; and in (11c) the latter reading is preferable.

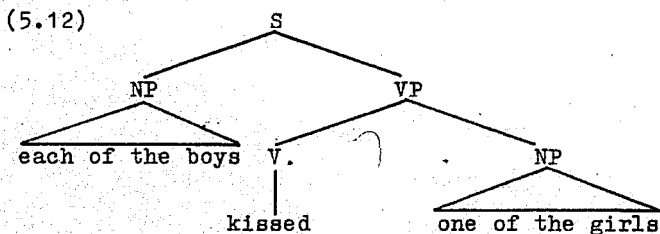
This complication, together with the facts mentioned in footnote 5, suggest that it is basically wrong to

⁵ This statement is over-crude, for in fact there appear to be several, perhaps even many, other interpretations, which interact in a manner not dissimilar to, but rather more complicated than, the interpretations of:

- (i) All but one of the boys danced with all
but one of the girls

cf. Karttunen (1971:172-73). For the conclusions which must be drawn from this, see the further discussion below.

attempt to sort out the ambiguity by differences in underlying structure, and that, moreover, Equi-NP and pronominalisation are only special instances of this state of affairs, in their case coreference being involved as well. The only possible method of handling the ambiguities of (11) appears to be to assume that only a two-way ambiguity exists and that thence there are only two possible underlying structures. But the assumption is unwarranted, as has been noted, and at least one of the underlying structures is unwieldy - furthermore, both are unjustifiable, given the methods open to transformationalists. That the assumption is unwarranted is sufficient criticism, but it is worth noting that the underlying structure for the first of the interpretations above would have to look either like:⁶



which is unsatisfactory in that (amongst other reasons) it fails to express the fact that all the girls were kissed, or take the form of an indefinite conjunction.

⁶ The required structure is grossly simplified here, but even that does not help a protagonist of such a solution.

Although some grammarians have hinted at the possibility of relating quantifiers to coordinations, cf. McCawley (1970:297) and, of course, Carden (1970a), see §4.2, the stumbling block of indefinitely large phrase markers is always reached.

In any case, different underlying structures appear to be beside the point. What really seems to be happening is that in certain cases, e.g., (11a), reference is being made to a number of individual, but possibly simultaneously performed, acts. Now although this does not affect our concept of the subject-verb relation, it does affect our concept of the verb-object relation. We appear to be faced with a 'tolerable ambiguity' in English (and, we may suppose, most other languages). In other words, all sentences of the type $[NP_{pl} - V - NP_{pl}]$ are potentially ambiguous in the manner of (11), and this ambiguity is primarily dependent on the semantic relationship between the subject/verb complex and the object.⁷ Thus, where the most probable interpretation

⁷ "Subject" and "object" are, of course, rather vague terms, and it might be preferable to relate the facts stated here to underlying functional or case relations, cf. Anderson (1971b). But it is uncertain how this could be accomplished, for it is very difficult to see at what level of structure the ambiguity is determined. This strengthens the case against attempting different derivations.

is that a number of individuals are separately affected by the action of the subject which the verb represents, then the first interpretation is preferred, as in (11a), but where the semantics prefers a collective interpretation of the subject - verb - object relationship, there the second interpretation is preferred, as with (11c). A very obvious case is:

(5.13) The girls picked the flowers

Although there is a possible ambiguity here it is tolerated. I would suggest that there are two reasons for this: firstly, the ambiguity is not crucial for comprehension, i.e., it is acceptable 'noise'; secondly, the ambiguity resides not in any differences of underlying structure, but in the inherent nature of the relationship of plural object NP's to verbs with plural subject NP's.

We are faced with what might be termed a systematic referential ambiguity in language, and the result is that we are forced into an impasse, for, as we shall see, this ambiguity needs to be brought into the syntactic description, and there is no context into which it can fit. The truth of this latter statement can be observed by the fact that the ambiguity is multivalent and depends upon an infinitely variable expression of the functional relationships under discussion. But, I would suggest, it is needed precisely to express the occurrence of (4) - (5) and (8) - (10) under Equi-NP and

pronominalisation. What we need is a mechanism by which, when the second interpretation of (11a) is taken, Equi-NP and pronominalisation are blocked. This, of course, is to return to the proposals of Carden (1968), for he attaches a higher quantifier to the NP which would otherwise undergo the relevant transformation.

But it is precisely those sentences such as in (11), which do not undergo Equi-NP Deletion or pronominalisation, that show that that solution is false. The difference between (4) and (5), in relation to the structure of the constituent NP's, is exactly the same as that between (11a) and (14):

(5.14) All the boys kissed all the girls
but whereas there is no reading in common between (4) and (5), the only reading of (14) is identical, except for the here irrelevant matter of emphasis, to one of the many possible readings of (11a), or, for those readers who find (14) ambiguous, it is so in exactly the same way as (11a). Given the first of these cases for (14),⁸ it therefore has to be assumed that the underlying structure of (14) is identical to one of the underlying structures for (11a), but that the quantifiers are freely (?) deletable in the latter. But that has two consequences: (i) we have to permit massive

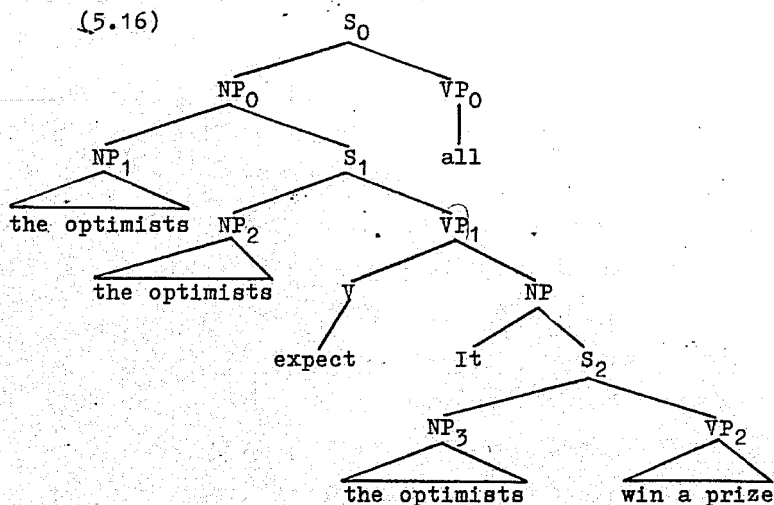
⁸ Since the consequences follow in either case, I have merely taken the simpler of the alternatives.

deletion of quantifiers under very loose conditions; (ii) we have to permit (11a) and all sentences of a similar form to have a possibly indefinite number of underlying structures. Since neither of these consequences is acceptable, we are obliged to state that (7) is not an adequate underlying structure for (5), and hence that Equi-NP must be formulated differently.

Our final argument against Equi-NP Deletion is not radically different from that above. Consider the sentence:

- (5.15) All the optimists expect the optimists to win a prize

The underlying structure of this sentence is presumably:



But since NP₂ and NP₃ are identical when required to be so, Equi-NP will operate freely; and the resulting

surface structure from (16) will not be (15) but:

(5.17) All the optimists expect to win a prize. Thus (16) cannot, if Equi-NP operates as has been suggested, be the underlying structure of (15). The solution would seem to be that there will be three distinct underlying structures, each of which will correspond to one of the surface structures for (15), (17) and the equivalent of (5), where all the optimists has not been deleted. For (17) the structure will be that of (16), but the structures of both (15) and the equivalent of (5) must have a higher quantifier occupying the VP_2 position in (7). In the latter case it will be all, but what will it be in the case of (15) - the "disappearing" generic of Carden (1968)? Given the nongeneric status of the sentence that seems implausible. And further, in the case of a modification of (15) where there is no initial quantifier in surface structure, how do we determine whether or not there is still a quantifier in VP_0 position? That question, unfortunately for Carden, seems to require the answer that there is no possible way to so determine. However, let us not pursue that point and instead accept the notion of a generic quantifier as a filler for VP_2 . But once again this is a fudge. The reason why there is no Equi-NP Deletion in (15) is that the reference of NP_3 is different from that of NP_2 , and thus is unrelated to quantifier syntax.

In order to clarify the latter statement it is useful to consider the following sentences:

- (5.18) a All the Liberals expect all the Liberals to merge with Labour
- b All the Liberals expect the Liberals to merge with Labour
- c All the Liberals expect to merge with Labour

Even without discussing the grammaticality of these three sentences, it should be clear that (18a) is alone in not having a reading which states that what is expected is that the Liberal party will merge with the Labour party. (18b) seems to have only that reading and (18c) is potentially ambiguous: like (18b) it may have that reading, but it could also have a reading analogous to that of (4). In other words, the reference of the subject NP of S_2 in each sentence of (18) has a different potential force. Now what this fact about these sentences shows is that it is extremely difficult for the Lakoff-Carden proposals to give an adequate account of the consequential facts. The verb merge demands that its subject and object be semantically alike in certain respects, and that which concerns us here is that since Labour is [+abstract, -animate], the subject must have the same features. Therefore, only the first reading which we have discussed above is fully grammatical, and so (18a) is ungrammatical. It is impossible to relate this to the syntax of quantifiers in the way that the

proposed solution for Equi-NP, by a manipulation of quantifiers, would suggest was correct to get the necessary descriptions for the sentences of (18). In fact, the whole problem seems to have very little to do with the syntax of quantifiers. And so another solution for Equi-NP, which does not place so much reliance on higher quantifiers, will have to be found if we are to explain the facts of Equi-NP and (18) in the same way, as we ought to do. Finally, we might note that Anderson (1974) suggests a structure for sentences such as (15) which do not appear to run into the identity problems of Equi-NP which confront (16). However since this relies on an analysis of all about which there must be grave suspicion, cf. Chapter 9, especially §9.2, it cannot be regarded as crucial evidence. Nor is it clear that it provides an adequate solution to the problems surrounding (18). In any case, Anderson's solution does not support the theories of Lakoff and Carden, since Anderson does not consider quantifiers to be higher predicates.

In the light of the above arguments we are clearly obliged to conclude that the evidence of Equi-NP Deletion in no way contributes support to the hypothesis that quantifiers should be derived from higher predicates. Moreover, we can state with confidence that even if such a hypothesis were independently justified, there is very little evidence that this would help towards a

solution of the Equi-NP problem. For if it were so, then we would be committed to the claim that all plural NP's have higher quantifiers, a claim that cannot be justified in as much as it does not shed light on the problems of reference which are at the heart of the question. If we are to find evidence that quantifiers are higher predicates, then we shall have to look elsewhere.

5.3 'Archaic' constructions

An argument first presented in Lakoff (1970b:175), but later expanded in Lakoff (1970d:395-99), purports to give further evidence that quantifiers are predicates in underlying structure. The evidence is that in 'archaic' English there are constructions of the type:

- (5.19) a ?The men are few
- b ?The men are many
- c ??The men are five

Unfortunately, Lakoff gives no evidence to suggest that such constructions are archaic, and indeed it may be doubted that this is the correct description. Presumably, although he does not say so, he is relying on the OED entry for many, but not that for few. The point is that although such constructions did occur at earlier stages in the history of English, there is no reason to assume that they have ever had more than the highly restricted currency which they have in the present-day

language. Some of Lakoff's remarks about the history of English, therefore, should be treated with a pinch of salt.

The quantifiers which occur in this position are called 'absolute' quantifiers, for reasons irrelevant here; those quantifiers which can never occur in predicate position, such as all, some, every, are called 'relative' quantifiers. Lakoff's argument then runs as follows (1970d:398):⁹

"Now in a grammar of that [archaic:RMH] dialect (and at an earlier stage of English), the quantifiers in (19) would have to be set up as predicates. In order to relate quantifiers in predicate position with the corresponding pre-nominal quantifiers, as one would have to do in such a dialect, one would have to set up a rule of quantifier-lowering. Thus, such a rule would be independently motivated for quantifiers of absolute size, and would apply optionally for such quantifiers ... Now if all

⁹ The original numbering of the quotation is retained. (19) refers to examples similar to (19) in this paper; (1) and (2) are irrelevant to the present discussion but can be found in Lakoff (1970d:390) and Part (1970:153).

quantifiers are generated in predicate position and if the rule of quantifier-lowering is generalized to operate on quantifiers of relative size (it would be obligatory for relative quantifiers, optional for absolutes), then one can account for the fact that relative quantifiers like some work like few in sentences like (1) and (2)."

Before we discuss this argument in detail, we must consider the fact that only those quantifiers which can function 'archaically' as predicates can also occur in postdeterminer position, as noted by Partee (1970:157). This point is discussed by Carden (1970c): his solution is that if we mark the 'relative' quantifiers as obligatorily undergoing quantifier-lowering, an extension of Lakoff's suggestion above, then exactly that set of quantifiers will be blocked from appearing in postdeterminer position, since no rule of quantifier-lowering is involved in such derivations. The blocking results from the fact that 'relative' quantifiers will, with respect to quantifier-lowering, belong to the "positive absolute exception" type discussed by Lakoff (1970b:49-56), however that has to be reformulated, cf. Lakoff (1970b:ix-x).

The facts stated by Lakoff and Carden are in principle correct: 'absolute' quantifiers can appear 'archaically' in predicate position, and only those quantifiers

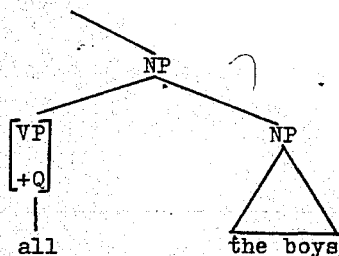
that can appear in predicate position can appear as postdeterminer quantifiers. Therefore, the question which is at issue is whether or not the mechanism of quantifier-lowering adequately explains the surface structure patterning. Lakoff's argument in favour of quantifier-lowering, and thus considering quantifiers as underlying higher predicates can, like the argument discussed in §5.2, relating to Equi-NP, be analysed into three stages: (i) 'archaic' dialects of English show us that some quantifiers have to be considered as underlying predicates; (ii) if all quantifiers are considered as predicates we have made a valid generalisation from stage (i); (iii) there is a further generalisation available in that the correct set of postdeterminer quantifiers can be generated most economically by postulating positive exceptions to the quantifier-lowering rule.

But each stage of the argument is false. While it is true that 'absolute' quantifiers may occasionally turn up in surface structure as predicates, we can only deduce from that that the relevant part of the underlying structure involves a predication, and there is no justification for claiming that it is nothing but a predication. Once again, we can observe that Lakoff and Garden have made assumptions beyond the point that the evidence will take them. If there is an alternative solution which makes the least assumption empirically

justifiable, and as we shall see in Chapter 8 that there is, then that solution will be preferable, especially if it can be generalised to explain why 'relative' quantifiers do not turn up as predicates.

One additional reason for this conclusion is that the generalisation at the heart of stage (ii) of the argument is only apparent. To confirm this, we need only consider the function of quantifier-lowering. Certainly, one purpose of the transformation is to get the quantifier into the right sentence, but that is not what concerns us here. What is far more important is, that the quantifier which is originally dominated by VP, and hence a predicate, is lowered into an NP-dominated position, where it is a determiner of sorts. That this is so seems to be denied by Carden (1968:10-11), where the relevant fragment of the derived phrase-marker is:

(5.20)



But it is extremely difficult to defend such a position as far as surface structure is concerned, for a quantifier in the position of all in (20) hardly acts like a predicate. We might note that Lakoff (1970b:176) appears

to suggest that after quantifiers are lowered they are dominated by a determiner node, but there is no explanation of this probably more correct position. Carden (1970b:287) also goes a long way to accepting this. Therefore, we may state, despite the tree of (20), that quantifier-lowering alters the status of quantifiers from underlying predicates to surface non-predicates. Now consider the case of a 'relative' quantifier such as some. If we accept the Lakoff-Carden proposals it is necessary to consider it a predicate in underlying structure. But then it is obligatorily lowered. But this has the effect of deleting all trace of predicate status. What has been done, in fact, is to assign to some a predicate status for which there is no empirical evidence, and as a consequence we have then to obligatorily remove all trace of that status before surface structure is reached. In other words, we can only make the generalisation that all quantifiers are predicates if we construct a mechanism for wiping out that generalisation when, as in many cases, there is no evidence to support it.

The fact that 'relative' quantifiers cannot appear in postdeterminer position involves Lakoff and Carden in an even greater deviation from simplicity. As we have seen, they can only explain this failure to appear in postdeterminer position by stating that 'relatives' obligatorily and 'absolutes' optionally undergo quantifier-

lowering (more precisely, meet the structural description for quantifier-lowering). This explains the failure, because at no point in the derivation of a post-determiner quantifier is the structural description for quantifier-lowering met. But that a solution such as this should be necessary is extremely strange. After all, if all quantifiers are underlying predicates, why shouldn't they all appear in postdeterminer position? There are two alternative answers to this question: the first is that suggested by Lakoff and Carden and discussed above; the second is that the correct analysis of quantifiers shows that not all quantifiers are underlying predicates, or, more precisely, that not all quantifiers are involved in an underlying predication.

As we have already said, the first of these answers leads to the postulation of a set of items which must meet a certain structural description, and thus a costly formalism will have to be provided in the grammar to state this case, cf. Lakoff (1970b:49-56). The cost is not simply to be measured in terms of the insertion of a metarule to handle such 'absolute exceptions', which is in fact fairly inexpensive; rather, there is the fact, inherent in such cases, that there is no generalisation possible to predict which items will have to be marked as exceptions. On the other hand, the second answer above has quite simple consequences, for if a quantifier involves no predication, then there is no necessity

to have rules beyond those that already exist to explain why it does not appear in postdeterminer position.

Moreover, the fact that some quantifiers are involved in underlying predication will provide us with an independently justifiable explanation of why that and only that set of quantifiers appears in postdeterminer position. Finally, we shall have dispensed, in this context, with the notion of absolute exceptions, which Lakoff himself (1970b:ix-x) clearly suspects.

What conclusions may we draw from this discussion? Firstly, it must be accepted that the argument from 'archaism' and postdeterminer quantifiers does indeed support a hypothesis that a number of quantifiers, those called 'absolutes', are involved in an underlying predication. However, it would be departing too far from the evidence at hand to claim that these quantifiers are underlying predicates. Secondly, there is no evidence whatsoever from the above constructions to substantiate the claim that the 'relative' quantifiers are underlying predicates. If we accept the assumption of Lakoff and Carden that quantifiers should have one primary underlying source, e.g., (but not i.e.) as predicates, the necessary conclusion must be that quantifiers are not higher predicates as Lakoff and Carden claim, but something else, which may, however, in the case of 'absolute' quantifier, involve a predication. In Chapters 6 and 7, especially, we shall see that there is substantial

evidence to suggest what other status quantifiers may have, but for the moment we need only conclude that the argument from 'archaic' constructions in no way supports the specific claims made by Lakoff and Carden.

5.4 Negatives and quantifiers

In this section let us firstly consider the rule of Neg(ative) Transportation (also called Not Transportation). This is a minor rule, cf. Lakoff (1970b:30-48), which moves a neg particle from the highest embedded sentence into the matrix sentence, thus relating pairs such as:

- (5.21) a John thinks that he hasn't won the
prize
b John doesn't think that he has won
the prize

This rule is extensively discussed in the literature, cf. R. Lakoff (1969b), Lindholm (1969) and Horn (1971) for references. As a minor rule it applies only if one of a restricted set of verbs is dominated by the V node in the matrix sentence; for example, Horn (1971:120) lists twelve verbs, including think, want and seem. The relevance of Neg Transportation to the hypothesis that quantifiers are higher predicates is embodied in the following claim by Carden (1968:8-9):

"When we apply Not-Transportation to embedded S's containing quantifiers, the meanings we

get confirm the analysis proposed in 2.1.1 [that quantifiers are higher verbs:RMH] and incidentally prove that Not-Transportation precedes Q-Magic [quantifier-lowering:RMH]

- 9) a John doesn't expect all the boys to run
- b John expects that not all the boys will run
- c John expects that none of the boys will run

(9a) can only be synonymous with (9b), never with (9c). It follows that the embedded S of (9a) was '(not all) the boys run', and could not have been 'all the boys (don't run)'. That is, the 'not' must have been on the 'all' at the time Not-Transportation applied. But the Not-Transportation rule takes the 'not' from the topmost embedded S, which must therefore contain 'not all' but not 'not run'."

If this claim is correct, then there is good evidence to suggest that quantifiers are higher predicates. However, Jackendoff (1971b:287-96) has shown quite conclusively both that Carden's argument is correct only if none has the source all ... not and that Neg Transportation is in any case a doubtful rule. One most interesting argument concerning Neg Transportation is

that the rule was originally proposed, by Fillmore (1963) and Klima (1964), on semantic grounds, cf. R. Lakoff (1969b:140). But as reported by R. Lakoff (1969b: 140-41), Dwight Bolinger has correctly pointed out that Neg Transportation is a meaning-changing rule. Therefore, the original justification for Neg Transportation has been shown to be incorrect. Indeed, in the theory of generative semantics, to which R. Lakoff subscribes, transformations may not change meaning, cf. Partee (1971) and §5.2, above. Therefore it is self-contradictory for R. Lakoff both to accept the theory of generative semantics and to claim that Neg Transportation is a valid rule of the grammar. Furthermore, since the hypothesis that quantifiers are higher predicates is a basic tenet of generative semantics only, the rule of Neg Transportation cannot be used to support that hypothesis. A further discussion of the meaning-changing status of Neg Transportation is to be found in Lakoff (1970c:158-62), where it is suggested that the rule might be obligatory but sensitive to semantic information. If it is possible to formulate such a rule then our objections here would be nullified, but Lakoff's own remarks underline the difficulties involved. The kind of solution which Lakoff is clearly aiming towards would involve a global rule, and the status of global rules in general will be discussed, with rather sceptical conclusions, in §8.4, see too Lakoff (1970a). Therefore there seems no reason at present to accept Neg

Transportation into the transformational component of the grammar.

Of course, as he himself points out, Jackendoff is not committed to the contradictory position sketched out above, since he makes no claim that all transformations preserve meaning. His rejection of Neg Transportation is on purely syntactic grounds. Here we take the position that the syntactic inadequacies of the rule and its probable meaning-changing property combine, in present circumstances, to justify its rejection. And despite Jackendoff's acceptance of meaning-changing rules, it is possible to accept in large measure his alternative analysis (1971b:288-89) of the sentences quoted from Carden above, because at no crucial point is a meaning-changing rule involved. It might appear that we ought to make one distinction between Jackendoff's formulation and our own,¹⁰ for he accepts a rule which derives any from some - the Indef Incorporation rule of Klima (1964: 319). But this rule may be meaning-changing; therefore should we not reject it, cf. §3.3? If that were true, we should indeed do so, but in §10.2 we shall attempt to show that the some-any rule, which allows a wide generalisation to be made and is therefore prima facie a strong case for retention, is indeed meaning-preserving.

¹⁰ It should be noted that in Jackendoff (1971b:288) figures (8a) and (8b) appear to have been transposed.

Therefore no such distinction needs to be made at present, although that is rather beside the point just now. What is important is that we accept Jackendoff's critique of the argument which uses Neg Transportation to support the hypothesis that quantifiers are higher predicates, and therefore reject Carden's claim.

Before we leave the area of negation and quantifiers, it is necessary to discuss one further matter. Carden (1970b:282) claims that the following sentence:

(5.22) All the boys didn't leave

is, subject to great dialect variation, ambiguous.

There is one interpretation of (22) which has the reading of (23), where the negative is originally on the quantifier, i.e., the neg-Q reading; another interpretation which has the reading of (24), where the negative is on the verb, i.e., the neg-V reading:

(5.23) neg all the boys left

(5.24) All the boys neg left

This is claimed to be confirmation of the Neg Transportation rule and also the theory that quantifier are underlying higher predicates for the following reason. If (23) and (24) are true readings of (22), then negation on either the quantifier or the verb is possible. But only in the case of negation of the quantifier is the neg on the highest embedded sentence when we have:

(5.25) I think all the boys didn't leave

Neg Transportation only operates if the neg is in such a

position. Therefore, if Neg Transportation operates on (25), we shall find that the resultant surface structure has a reading synonymous with the neg-Q reading of (25), but not with the neg-V reading. This is the case with:

(5.26) I don't think that all the boys left

We therefore have further evidence of the plausibility of Neg Transportation and the claim that quantifiers are higher predicates, according to Carden. However, we have already noted that Neg Transportation appears to be a meaning-changing rule, which makes nonsense of Carden's claims about synonymy here. What is worse is that, as has been pointed out already, Carden explicitly rejects meaning-changing rules on a priori grounds, cf. Carden (1970b:281). Therefore this further argument from Neg Transportation is also invalid.

Nevertheless, (22) is an important and interesting sentence, and there are two points that are worth discussing with regard to it, both of which suggest that the situation is by no means as simple as it appears. The first of these points applies only to those speakers who interpret (22) as I do, but we must follow Carden (1970b:281) in his emphasis on idiolect variation, and since the point for discussion does cast doubt on certain key notions it would be remiss to ignore it. In my own speech, spoken forms of (22) are unambiguous, for the neg-Q reading (23) is possible only if all is heavily stressed. Otherwise, only the neg-V interpretation

represented by (24) is possible. Now with the neg-Q reading neg must command all in underlying structure.¹¹ In the surface structure of (22) neg and all command each other. Therefore, if Lakoff (1971c:244-45) is correct in his account of command relations, to get the required neg-Q reading either neg must precede all, which is a correct prediction of (23), or the neg element in (22) must have heavy stress. But, in order for (22) to have a grammatical neg-Q interpretation in my speech, it is essential that all, rather than the neg element, be heavily stressed. We have, therefore, a situation where in order to obtain a neg-Q interpretation it is necessary to stress exactly that element which Lakoff predicts should not be stressed. On the other hand, the neg-V interpretation of (22) is correctly predicted by Lakoff in his account of those command relations involving quantifiers and negatives. Therefore we are faced with an uncomfortable choice: either Lakoff's account of command relations is incorrect, or Carden's hypothesis about the neg-Q interpretation of (22) is incorrect. The evidence we have would suggest that the latter is the case, but since that in itself would cast doubt upon Lakoff's account of many command relations, we shall postpone any discussion until §8.4, when it will become more relevant.

¹¹ For a discussion and definition of the command relation see Langacker (1969).

The second point for discussion arises out of a comparison of (22) with:

(5.27) Many of the boys didn't leave

The problem is that not only is (27) unambiguous, having only a neg-V reading, but it can be quite simply demonstrated that this is the case and that:

(5.28) Not many of the boys left
has a different meaning. The sentence:

(5.29) Many of the boys didn't leave, but
many of them did

is both grammatical and non-contradictory. This latter follows from the fact that the subset indicated by many of the boys may be smaller than the subset indicated by half of the boys, given, of course, the same set of boys. In other words, (30) is valid:

(5.30) [many of the boys] < [half of the boys]

On the other hand, (31) is contradictory:

(5.31) *Not many of the boys left, but many
of them did

This follows from the possibility of (30) being valid. But if (28) is a possible reading of (27), as (23) is a possible reading of (22), then there should be a reading of (29) which is contradictory in exactly the same way as (31) is. But there is not, and so (28) is not a possible reading of (27); therefore a neg-Q reading of (27) is impossible. But if we follow Garden there must be such a reading, since he derives many from an underlying structure identical in relevant aspects to that

for all. This may be difficult to believe, but nowhere does Carden suggest that it might be otherwise (the same appears to be true of Lakoff). Indeed, there is ample evidence that the position outlined here is a correct description of both Carden's and Lakoff's approach. For confirmation of this see Lakoff (1970d:175-83; 1971c: 239-42) and Carden (1970c:425). This latter reference shows that some distinctions are drawn between all and many, but no conclusions are reached which would be relevant to the point discussed here.

The best that can be stated for the quantifier-lowering hypothesis, therefore, is that it is in need of considerable reformulation in order to account for the discrepancies mentioned here. Nevertheless, any alternative hypothesis will have to account for such facts in a more consistent and well-motivated manner than even a modified quantifier-lowering hypothesis will be able to do. As we shall see, this is far from simple, and negation is perhaps the trickiest problem to be faced. In §8.4 we shall attempt to provide at least an outline of a solution to the difficulties, see too Hogg (1974). But as the criticisms in Johansson (1974) show, the question is far from being resolved.

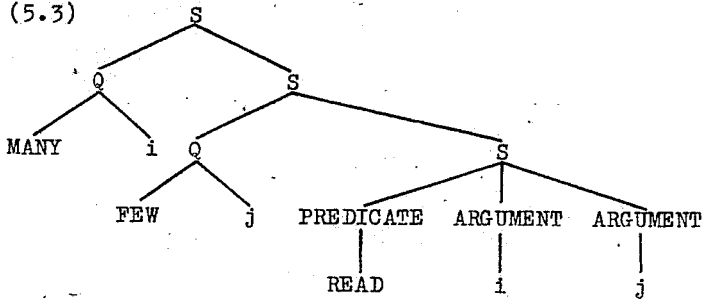
5.5 Logic and linguistics

In §5.1 we noted that Lakoff (1971c:239) suggested (3) as an underlying structure for (2) - repeated here

for convenience:

(5.2) Many men read few books

(5.3)

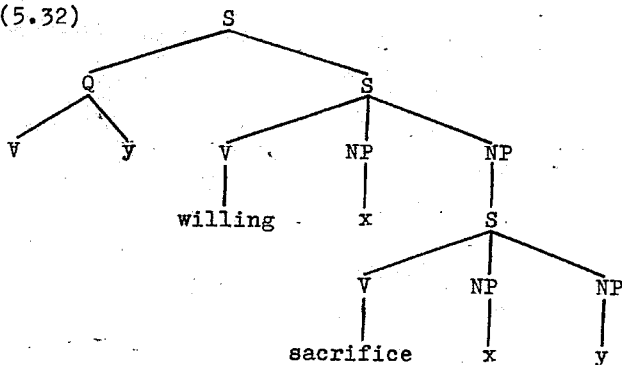


Although Lakoff (1971c) makes no attempt to justify (3) nor to explain the status of the constituents,¹² there are much clearer hints to be found elsewhere, notably in Lakoff (1971a and b), that trees such as (3) are intended to be notational variants, or nearly so, of representations in the canonical notation of symbolic logic, cf. Quine (1960). For example, Lakoff (1971b:10) gives representations both in tree form, as in (3), and in logical notation, and there is a claim that the representations are equivalent. This claim, although only implicit, is obviously true. To take an example pair from Lakoff (1971b:10), there is no explicit difference between (32) and (33) in terms of their explanatory

¹² The only claim made by Lakoff (1971c) is that (3) is "somewhat closer to reality", cf. §5.1, above. As it stands, this claim is meaningless, and it has linguistic interest only for students of the rhetoric of polemic.

power:

(5.32)



(5.33) [(x) (willing (y, sacrifice (x,y)))] (a)

In other words, what Lakoff has done is shown that the notation of symbolic logic can be converted into the notation of transformational grammar without any loss of adequacy; or at least that would appear to be the case. But in fact it is not so, for what Lakoff has done is not a conversion of the notation of symbolic logic into a transformational underlying structure of English, but merely a conversion of that logical notation into tree-branching phrase structure markers, which is a simple mathematical operation, cf. Chomsky (1957:26-33). The real issue then is whether or not the underlying structures of language are as described by symbolic logic; Lakoff's particular notational variant is interesting only in so far as it presents a clearer (to linguists) view of the descriptions of symbolic logic than does canonical notation. To attempt to find the answer to

this issue perhaps two questions are relevant.

The first of these is: why should the notation of symbolic logic be thought appropriate for the underlying representation in a grammar? The answer to this is fairly simple. Logic, it is claimed, is the analysis of the 'laws of thought' as they are rationally reconstructed in natural language (Reichenbach, 1947:2). Thus, logic is an attempt to obtain semantic representations for natural languages. Now grammarians such as Lakoff, i.e., generative semanticists, believe that the underlying representations of a grammar ought to be semantic representations. There would therefore appear to be an affinity of purpose between logicians and grammarians, and it cannot be denied that if the claims of the generative semanticists are correct this is to some extent true.

But there are ~~differences~~ differences: for example, linguistics is an empirical science, it depends upon the construction of a rule device which generates actual sentences of an actual natural language. On the other hand, philosophy, and hence logic, is a theoretical science. As Strawson (1970:14) says, the student of philosophy

"... will be prepared from the start to use a vocabulary which is overtly semantic, or, in a broad sense, logical, for the classification of elements abstractly conceived ... [He] may finally relate these theoretical

models of language to what is actually found in empirically given languages."

Furthermore, as Strawson (1970) also points out, proponents of symbolic logic do not claim to be attempting a complete analysis of any natural language. As Quine (1960:160) states:

"On the whole the canonical systems of logical notation are best seen not as complete notations for discourse on special subjects, but as partial notations for discourse on all subjects."

It therefore seems reasonable to conclude that symbolic logic may provide linguists with much aid in the solution of many semantic problems, but that nevertheless the empirical constraints on grammars and the restricted aims of logic must mean that there will be many differences between the representations of the linguist and those of the philosopher.

The above, of course, is a theoretical conclusion, and it is only when we answer the second question - how adequate is the notation of symbolic logic as an underlying semantic representation of natural language? - that we can reach a proper empirical conclusion. As Lakoff (1971b) shows, the use of canonical notation in the construction of underlying representations can clarify certain problems such as opaque reference, although even here we ought to note the criticisms of

Lakoff's analysis which have been made by Heny (1973: esp. 238-44). But that such a clarification may be possible is hardly surprising, since opacity has been one of the basic concerns of logicians. A fair summary of Lakoff (1971b) would seem to be that it has demonstrated that the fruits of logic can be transplanted onto the trees of transformational grammar, but, because of the close mathematical relation between the two notations, this is hardly a major advance. The problem with which we must be concerned is whether or not it is possible to account for certain linguistic facts which have not otherwise been accounted for within such notation.

Within the realm of quantifier syntax there is the following problem: the three sentences:

(5.34) All men have two legs

(5.35) Every man has two legs

(5.36) Men have two legs

are all convertible into the following logical form:

(5.37) $(x) (m(x) \supset h(x))$

where m = man and h = has two legs. This is logically adequate, since only the structure necessary to determine the truth values of (34) - (36) is required, cf. Quine's "maxim of shallow analysis" (1960:160). But grammarians have further tasks, such as explaining the linguistic difference (of emphasis?) between (34) and (36) and the singular concord of (35). Also, grammarians

must explain the ungrammaticality of:

(5.38) The man has two legs

(5.39) Man has two legs

in an interpretation equivalent to (37). In other words, the problem is that the universal quantifier of symbolic logic has many equivalents in natural language but that these equivalents are not semantically and syntactically, as opposed to logically, equivalent.¹³ With respect to the existential quantifier, as we shall see in later chapters, in many cases there is no equivalent in natural language.

The conclusion that we must come to, therefore, is regrettably indecisive. There is evidence both in favour of the use of canonical notation in underlying structures and evidence against its use. Therefore, in those areas where symbolic logic has proven itself to be of use, as, perhaps, in matters of referential opacity, there is no reason why we should not make use of it. But this should not commit us irrevocably to the

¹³ For further discussion see Jackendoff (1972a).

There is no doubt that logicians have available techniques which permit them to distinguish between different expressions of the universal quantifier in English, cf. Quine (1961), Reichenbach (1947:99-101). But it is surely a matter for dispute as to whether grammarians should adopt such descriptions.

hypothesis that symbolic logic presents us with the correct underlying representation in every case. More precisely, with reference to the grammar of quantifiers the theoretical fact that logical notation suggests that quantifiers may be underlying higher predicates is of no greater importance than the empirical fact that English does not provide us with much decisive evidence in favour of such a hypothesis.

5.6 Conclusion

In Chapter 5 we have considered the history and adequacy of the hypothesis that quantifiers should be represented as higher predicates in underlying structures. We have observed that the original proponents of this thesis, George Lakoff and Guy Carden, have, to some extent and with the passage of time, changed their opinions on the justification for this hypothesis, but that the essential justifications have remained consistent; therefore we have examined the most important four of them. The first of these is Equi-NP Deletion. It was claimed that this transformation could only operate satisfactorily if quantifiers were derived from higher predicates. But we saw that in fact the problems surrounding Equi-NP are present even when there is no overt quantifier in surface structure and, further, that these problems were also to be found in other parts of the grammar, indeed wherever questions of referential

properties were involved. Although the hypothesis of a 'higher-Q' analysis might have been able to solve these problems mechanically, the disadvantage was that it disguised the fact that the essential problem was one of reference. In other words, arbitrary syntax swept the semantics under the carpet.

The second justification was that derived from so-called 'archaic' constructions. We saw that this argument appeared to have some force in connection with a limited subset of quantifiers, but that when it was extended to other quantifiers, that was done at the cost of distorting the syntax and semantics of those items, and so it could in no way be considered to be a correct justification. The argument started from the correct analysis that some quantifiers have some predicate-type features and thence proceeded to the quite invalid conclusion that all quantifiers are basically predicates.

The third justification revolved round Neg Transportation and the relation of quantifiers to negatives. The first argument in this context failed, we observed, simply because under the assumptions of generative semantics it is not possible for Neg Transportation to be a rule of the grammar. But that is not to say that we must always reject meaning-changing rules. The point is, if such rules are accepted then the arguments which have been brought forward in favour of a higher predicate source for quantifiers will have to be rejected,

since the positions are mutually contradictory. On the question of other points of relation between negatives and quantifiers, it was accepted that the Lakoff-Carden thesis was no further distant from a solution than any other thesis, but in itself, this was hardly a strong argument for accepting it.

The fourth and final justification was the appeal to the notation of symbolic logic. It was agreed that this was an interesting point, but there were crucial differences to be noted, mostly in the matter of aims, between logic and grammar, which suggest that although symbolic logic is an essential helpmate in the construction of a grammar of a natural language, it would be unwise to accept that logical systems should determine the underlying structures which grammarians propose. This justification, therefore, is no more valid than the others we have discussed, and it is necessary to conclude that the Lakoff-Carden hypothesis has not been shown to have a good chance of being correct. There are now two courses open to us: we could either try and find other justification for their theory, or see what justification competing theories might have. In view of the flimsiness of this present hypothesis, it seems only correct that we should consider other theories, and they are therefore the subject of study in Chapter 6. After that we can continue, in Part III, with our own proposals, which do not entirely reject the claims of Lakoff and Carden.

Chapter 6

Some alternative analyses

6.1 Introduction

The particular theory of grammar which one espouses, and within that theory which analysis one prefers, is obviously the prime issue for a linguist, and it is to be hoped that in this chapter the study of quantifiers will be seen to be of some relevance to this issue. The choice of a theory is not an a priori matter, even within transformational grammar and despite the remarks of Hall (1968), cf. Hogg (1970) and Pullum and Humberstone (1971). It is an empirical issue, and so we must examine the evidence with which natural languages present us. In this respect we have already noted in §5.5 that the approach of certain generative semanticists is occasionally misconceived. Quantifiers have a very important empirical role to play, and this is not only because of the kind of evidence we presented in Chapter 4, which suggested that the theory that the underlying representation contains all the necessary semantic information for the understanding of a sentence was best equipped to explain the semantic and syntactic characteristics of both. That, of course, is important, but there is another matter which, perhaps accidentally, is even more important.

The reference here is to the status of the Lakoff-Carden proposals discussed in Chapter 5. As we have observed, the hypothesis that quantifiers are derived from higher predicates is argued for solely in terms of the theory of generative semantics. Further, for most, but not for all, cf. §6.6, generative semanticists, that hypothesis is the accepted explanation of the behaviour of quantifiers. Now, the arguments of Chapter 5 show that the Lakoff-Carden hypothesis is insufficiently supported by the semantic and syntactic evidence to be acceptable. Yet it is apparently crucial to an acceptance of the theory of generative semantics. There are two reasons for this: firstly, and here the principle is universally valid, if the theory of generative semantics cannot give an adequate account of such a major area of English grammar as the quantifier systems, then it must be rejected totally; secondly, some generative semanticists have elevated the Lakoff-Carden hypothesis to the status of a necessary foundation for their theory. But that point is rather trivial and in any case not true for all generative semanticists, and so it can easily be claimed that the Lakoff-Carden hypothesis is wrong but the theory of generative semantics right, thus ignoring the second point above.

On the other hand, it seems only reasonable to take the claims of the relevant generative semanticists at their face value, and accept that the inadequacy of the

Lakoff-Carden theory implies that we should turn to some other theory - although keeping within the context of transformational grammar, for the reasons briefly outlined in Part I. Therefore, we now have to examine any other analysis of quantifiers which is consistent with some theory of transformational grammar. If such an analysis can be found and then shown to be adequate, then it cannot be doubted that we must accept the theoretical consequences that that analysis has.

Unsurprisingly, such analyses have been proposed, and amongst those the one which is perhaps the most comprehensive is that which has been put forward in several papers by Ray Jackendoff (1968, 1969, 1971a and 1972b), and therefore it is to that theory that we must first turn our attention. The first three papers differ considerably in their aims: Jackendoff (1968) is an attempt to determine the underlying syntactic structure of quantifiers and pays comparatively little attention to semantic features; Jackendoff (1969, 1971a) are attempts to provide rules of semantic interpretation (see below, §6.5) for quantifiers and, but this will not concern us greatly, for other items too. Jackendoff (1972b) contains a more general account of these and various other topics, mainly semantic.

The split between syntax and semantics seen in the earlier papers is possible only because Jackendoff is an adherent of the theory of interpretive semantics. This

theory holds that not all semantic information is contained in underlying representations. Transformations may change meaning and rules of semantic interpretation may add meaning, cf. Partee (1971) for further background information. Thus Jackendoff's semantic interpretation rules do not apply only to underlying structures, but may also apply to intermediate and surface structures, and even cyclically; see Jackendoff (1972b:378) for a concise definition of the applicability of semantic interpretation rules at different grammatical levels. This leads to a certain amount of difficulty within our present discussion, for which should be thought of as prior: the underlying syntactic representations, or the rules of semantic interpretation? If one set of rules is shown to be incorrect, does that mean that the other set is wrong too? Logically, that seems to be most probable, for if, for example, the underlying representations are incorrect and must be altered, then the structures upon which at least some interpretive rules operate will also be altered and so these latter rules will have to be changed too. Perhaps the reverse is less likely, but it is an open question.

Further, it may even be the case that we conclude that only a semantically-based underlying representation, containing all and only all the semantic information necessary, is adequate. In that case it must be concluded that both parts of Jackendoff's theory are

inadequate, whatever the adequacy of one part or the other in isolation. But it would be preferable to delay such a problem until we are directly confronted by it. For the moment we shall proceed by first examining the adequacy of Jackendoff's underlying syntactic representations.

6.2 Analyses in conflict

Jackendoff (1968) considers three groups of words which occur in similar noun phrase constructions. The first group consists of noun phrases, e.g., a group, a wagonload, a pound, a number, a pair. Group II involves at least the following quantifiers: some, each, few, which, all and both. In Group III there is another set of quantifiers, including a few, many, one, three. The distinction between Group II quantifiers and Group III quantifiers is a familiar one, for the latter may appear in postdeterminer position, the former may not, cf. the discussion in §5.3. It would therefore seem appropriate to say that Group II quantifiers are 'relative' quantifiers and those in Group III 'absolute' quantifiers. And indeed it is the case that there is a large measure of agreement between the categorisation provided by Jackendoff (1968) on the one hand and Partee (1970) and Lakoff (1970d) on the other. Only one serious discrepancy arises: to use Partee's terms, Jackendoff (1968: 423) claims that few is a 'relative' quantifier, whereas

it can be deduced from the remarks made by Lakoff (1970d: 396) that he would consider the instances of few in (1) and (2) to be identical:

(6.1) The few arguments in favour of the proposal were easily dismissed

(6.2) There were few arguments in favour of the proposal

and therefore that few is an 'absolute' quantifier. Since for Jackendoff a few is an 'absolute' quantifier, it must be the case that he would regard the occurrence of few in (1) as derived from a few. This is quite plausible, since, as Perlmutter (1970:244-45) claims, it is reasonable to believe that a is always deleted when immediately following the, although in Chapter 11 we shall offer an alternative analysis of a which rules out such an explanation in the case of a few, where a is not precisely equivalent to the normal 'indefinite article' and may be better considered as idiomatic.

But there are some very strong arguments against Jackendoff's position. Thus, although in §5.3 we were reluctant to accept the conclusions drawn by Lakoff and Carden from an examination of the so-called 'archaic' constructions with quantifiers in predicate position, it seems reasonable to accept that there is a high correlation between the grammaticality of quantifiers in post-determiner position and of quantifiers in predicate position; thus compare the examples below:

- (6.3) a The many arguments
b The arguments are many
- (6.4) a *The some arguments
b *The arguments are some

Now although few is grammatical in predicate position, a few is not:

- (6.5) a The arguments are few
b *The arguments are a few

Thus it would seem most probable that few in (1) must be derived from few rather than a few, otherwise the correlation would be destroyed.

Another argument against Jackendoff follows from (3). Lakoff (1970d:395) argues that few ought to be derived from not many. We shall see in Chapter 8 that this is probably an over-simplification, but nevertheless the basic principle, that few is to be derived from a source very similar to that of many, appears to be correct. Now there is no disagreement that many is an 'absolute' quantifier, and given that that is the case and that the source of few is so nearly identical, it surely follows that few must also be an 'absolute', not a 'relative', quantifier. If few were a 'relative' quantifier the only possible explanation for the consequent contrast between its syntax and that of many would be that the underlying negative element had caused the switch, and that is plainly implausible.

It is instructive to consider why Jackendoff does not take note of such arguments. In the first case Jackendoff explicitly rejects any analysis of quantifiers as predicates (1972b:205) and thus commits himself to ignoring the evidence of (3) and (4). His only reason for doing so appears to be that sentences such as (4b) are ungrammatical, and while, as we agreed in §5.3, this weakens the Lakoff-Carden position considerably, it is an insufficient condition for complete rejection of their proposals. A similar, but equally unsatisfactory, position is held by Chomsky (1972a:184). It is not at all clear how Jackendoff would generate quantifiers in predicate position, but the denial of the crucial correlation is clear. In the second case Jackendoff (1969: 235; 1972b:341-42) argues against a rule deriving few from not many because of the "unsystematic and sometimes drastic changes in 'spelling'" which can occur. To some extent this also affects Jackendoff's attitude to the some-any rule discussed previously, but both these points will be taken up more fully in §6.5.

In both instances there seems to be the same fault, namely that Jackendoff takes only a rather restricted set of surface structure paradigms in order to establish putative underlying structures and further imposes the restriction that so-called 'spelling' changes, as of not many to few "are exactly the sort of changes we are trying to eliminate" (Jackendoff, 1972b:342). But to

restrict an analysis of quantifiers to prenominal positions only is empirically falsifiable, and we have already seen, in Chapter 4, that the lexicalisation transformations to which Jackendoff objects are theoretically desirable, for otherwise not only might it be difficult to explain correctly the behaviour of few, but it would also be almost impossible to characterise the grammar of an item such as both. Therefore Jackendoff's theory seems to be poorer both empirically and theoretically than, say, that of Lakoff and Carden, at least with respect to the grammar of few. This is a very grave disadvantage, which must be borne in mind when we proceed, immediately below, to more detailed analysis of each of Jackendoff's three groups of quantifiers. We should not, however, prejudge the adequacy of Jackendoff's theories on his failure to describe accurately a single quantifier.

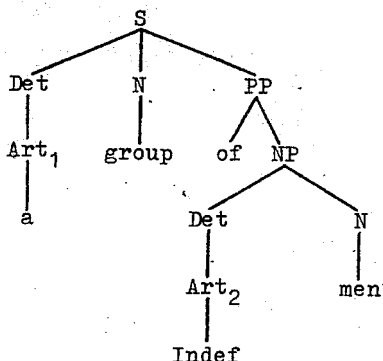
6.3 Quantifiers as nouns

Jackendoff (1968) takes as his starting point for his discussion of quantifiers the structure of NP's containing Group I words. For NP's such as:

- (6.6) a A group of men
 b A gallon of the whisky

Jackendoff (1968:426) suggests the following underlying structure:

(6.7)



(For (6b) the Art₂ node would dominate the.) Jackendoff notes two restrictions which apply between the head noun phrase and the complement prepositional phrase. The first of these is that it is not possible for both the 'article' of that noun phrase and the 'article' of the complement phrase to be 'definite' at the same time, unless there is a relative clause present. The second restriction is that those words belonging to Group I can never take a singular noun phrase complement, although some can take mass nouns. This, it is interesting to note, is some support for a theory that mass nouns should not be thought of as singular nouns, except perhaps in matters of noun - verb concord. It seems fairly clear that the syntactic behaviour of mass nouns is much closer to that of plural nouns than to that of singular nouns. However that is not to say that they are [+plural], for as was observed in §4.1, that is patently not so. But see below for a partial resolution of the

problem of marking the number of mass nouns, especially when they are syntactically plural.

Jackendoff is undoubtedly correct in noting the above two restrictions, but at least as far as the second restriction is concerned there seems a great deal more to be said. Consider firstly the status of nouns which are plural syntactically yet refer to one object only, for example, scissors, trousers. In these cases we find a Group I construction, apparently, which refers to a semantically singular but grammatically plural object:

(6.8) A pair of trousers/scissors

Perhaps this would not be worrying if it were not for the existence of analogous surface structures where the referents are more than one:

(6.9) A pair of doves/hawks

The problem with Jackendoff's analysis is that he refers simply to syntactic number, which implies that reference in (8) is made to the same number of objects as are referred to in (9). But it is clear that semantically this is not so. Note for example the contrast between (10a) and (10b):

(6.10) a *A couple of trousers

b A couple of doves

What we find is a situation where a pair accepts all grammatical plurals in the complement, but a couple, or almost all the other comparable words belonging to Group

I, only accept semantic plurals (including, in some cases, mass nouns). That a pair only accepts semantic singulars if they are grammatical plurals is shown by the ungrammaticality of:

(6.11) *A pair of hawk/dove

A solution would seem possible if we reconsider the notion of 'counters' which is presented in Ianucci (1952) and briefly mentioned in §2.2. It will be recalled that 'counters' are words used to change a noncount noun into a count noun. A good example of this occurs with the word tea in its sense "a beverage". Both forms in (12) seem acceptable, although perhaps (12a) is the older and more standard form:¹

(6.12) a Two cups of tea, please

b Two teas, please

Since tea in the sense described is not usually countable, a construction was found in order to deal with a situation where a specific number of portions were being referred to. This was effected by introducing cups as a carrier of the [+count] marker in sentences such as (12a). It would appear that tea has itself become acceptable as [+count] in this sense, and therefore the existence of (12b). But the important point is that the

¹ At least for British English (12b) is ambiguous, since teas may refer to a meal, but that point is ignored here.

two phrases are nearly semantically equivalent - indeed in many contexts they are equivalent. It can therefore be observed that cups merely carries the [+count] values. This is not to deny that there is some extra semantic value in (12a), but that seems to be related to the causes of ungrammaticality in sentences such as:

(6.13) a *A herd of marshmallows ...

b *A cord of lettuce ...

which are noted by Jackendoff (1968:424).

If we accept the proposition that 'counters' are used to change noncount nouns into countable ones, then there seems to be no good reason why we should not use 'counters' to explain constructions such as (8). In such cases the 'counter' a pair is used to show that the syntactically plural scissors has only singular reference. It is not clear what mechanism is necessary to account for this, but perhaps something along the following lines will be adequate. Let us assume that scissors is [-count, +plural]. Of course, this is a highly dangerous assumption since the two features are apparently contradictory; but how else can the following sentences be explained, even in a modified form of Jackendoff's theory?

(6.14) Scissors are made in Sheffield

(6.15) *I want to buy two scissors

The only alternative would be to assume that scissors is, in underlying structure, [-count], and then have a

very late transformation which changed it to [+count] for grammatical concord only. But this would appear to be open to the objection from Jackendoff, as an interpretivist, that the underlying structure would then be determined on semantic rather than, and in opposition to, syntactic grounds. Another possibility might be to have two categories of number, one semantic, one syntactic, rather like the two categories of sex and gender, which are both needed to explain the following French sentence:

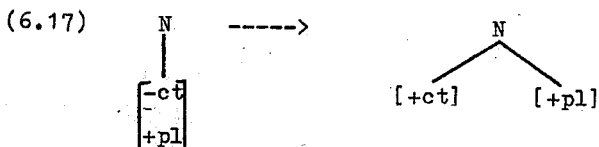
(6.16) Le professeur est enceinte

which is discussed briefly by Langendoen (1969:39-40). However, although there is good reason to suppose that a sex - gender split is necessary, see too Jones (1967), we can hardly claim to have sufficient evidence for an exactly analogous split in number. Yet Perlmutter (1972) contains a number of important points which may indicate that some kind of split in number is necessary.²

We must therefore accept, at least temporarily, that scissors is indeed to be categorised as [-count, +plural], for which we shall need to use the theories of exceptions and markedness introduced by Lakoff (1970b). Whatever the disadvantages of this, at least it provides

² It should be noted that the remarks made here are perhaps in contradiction of the claims about number and countability made in Lakoff (1970b:11).

an explanation of (14) and (15). Also, there is no doubt that scissors is a highly marked form, and the semantic categorisation shows this precisely. The question now is: how do we deal with the type of syntactic structures exemplified in (8)? I would suggest that we have a rule which is of the form:



In other words, this (optional) change in categorisation, which is highly marked, takes the form of a segmentalisation, along the lines suggested by Postal (1966). Although Postal's segmentalisation rules have been attacked on both theoretical and empirical grounds, notably by Delorme and Dougherty (1972) and Sommerstein (1972), there does appear to be strong support for such rules, as we shall see when we come to discuss the grammar of a in Chapter 11. There seems to be no a priori reason why (17) should in fact be rejected.

The segmented feature [+count], the result of (17), is then realised as a pair. That this is necessary is clear not only from (8), but also from constructions such as:

(6.18) A couple of pairs of trousers

Other strings where two Group I words cooccur are at best dubious:

(6.19) ?A group of companies of soldiers

(6.20) ?A wagonload of tons of potatoes

We have therefore provided a derivation of a pair in certain environments which is rather different both from other derivations for a pair and from the structures which Jackendoff proposes for Group I words generally. This would suggest that the above instances of a pair do not belong to Group I. Now this is important for one reason only: namely, Jackendoff offers no criterion for judging when double nominal constructions are classed as Group I constructions and when they are not. A further example of this can be seen in the following pair:

(6.21) A wagonload of potatoes is standing
at the corner

(6.22) A group of men are standing at the
corner

The contrast of singular vs. plural noun - verb concord clearly has to be explained, but the structures which Jackendoff (1968) proposes plainly do not enable us to account for these differences, since the structures for these two sentences would be identical in relevant respects.

The evidence which we have presented above suggests strongly that the underlying structures proposed by Jackendoff are insufficiently discriminating. Not all of the surface structure constructions which he discusses can be derived from the same underlying source, and

this casts doubt upon the validity of his analysis as a whole. Also, since such similar surface structure constructions as:

(6.23) a The love of God

b The shooting of the hunters

are certainly derived from different underlying structures, cf. Lyons (1968:249-53), Jackendoff's proposals must be sceptically received.

Although we have not provided an alternative underlying structure for all Group I constructions - but that is not the purpose of this chapter - it is certain that the structures proposed by Jackendoff (1968) are not 'deep' enough. Nevertheless, there remain two possibilities: firstly, that he has provided the correct surface structure; secondly, that he has provided a correct intermediate structure. The first possibility is to some extent accepted by Garden (1970b:287), and we shall not discuss it, but rather address ourselves to the second possibility.

Perhaps prepositions are one of the most controversial of linguistic entities, since they appear to be surface realisations of several different underlying structures, cf. Fillmore (1966b). Thus in (23) we find two strings which are reducable to NP of NP, yet in each case the relationship between the two NP's is, or may be, quite different - (23a) and (23b) are ambiguous.

If we then consider (6) once more, we again find strings reducible to NP of NP, and the relationship is again different. But it is noticeable that (23a) and (23b) have a common feature which does not occur in (6a), although it perhaps does in (6b): in the former two cases the of may be replaced by another preposition. That this has the result of disambiguating the sentences only adds strength to what I wish to say: in each case of is, as it were, a dummy preposition, which can stand for a number of others.

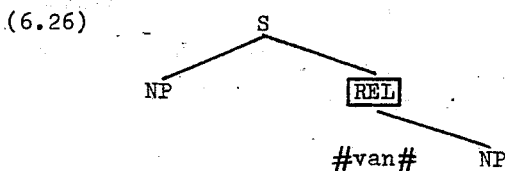
It will be necessary now to omit from our discussion the partitive-type constructions illustrated by (6b), which, contrary to my suggestion in Hogg (1972), are rather different from (6a). It must be said in passing, however, that this fact is also a disadvantage for both the Lakoff-Carden theory and the Jackendoff theory. But as I state in the above paper, Lee (1971) is quite incorrect in relating (6b) to simple possessive constructions. The whole question of the status of partitive constructions involving quantifiers will be more fully and precisely discussed in §10.3. Now, let us claim that in (6a) of is some kind of dummy preposition, but of a different order from that in (23) and, perhaps, (6b). For this possibility to be plausible of must be fulfilling one of two functions. Either it is the marker of a more complex structure, or it is inserted in order to create a grammatical surface structure.

The former of these has indeed been suggested elsewhere. Klooster (1971:205) makes the claim that Dutch van (= "of") in sentences such as:

(6.24) Het boek van Jan
("The book of John's")

(6.25) Een lengte van twee meter
("A length of two metres")

can be considered as a lexical entry of the form:



However, Dutch shows quite clearly that van has a restricted occurrence; for example, it does not occur in:

(6.27) a Honderden kilometers
("Hundreds of kilometres")

b Een paar blikjes frambozen
("A couple of tins of raspberries")

Although Klooster (1971:247) does specify that the "genitive marker" in English is zero after quantifiers, we have in (27) strings which quite clearly show that the same appears to happen in Dutch even when the preceding word is not a quantifier. This would suggest either that in (27) no relative clause is found in underlying structure, rather than that the Dutch genitive marker is zero here, or at least that (24) and (25) are derived quite differently from (27). Indeed, (24),

as Klooster points out, is a standard possessive construction, for which see our remarks above.

But the fact that the structures in (27) are closest to the Group I constructions of Jackendoff (1968), and further, the fact that:

(6.28) *Honderden van kilometer(s)

is ungrammatical in Dutch, leaves the way open for our second possibility, that of in English is inserted to preserve surface grammaticality, presumably because of a constraint that exists in English but not in Dutch, rather than its being a true reflection of some underlying configuration. A simple comparison of (27) with the equivalent English sentences suggests that this constraint might be that adjacent NP's form an ungrammatical string in English, i.e., *NP NP. Unfortunately, matters are not so simple as that, for consider:

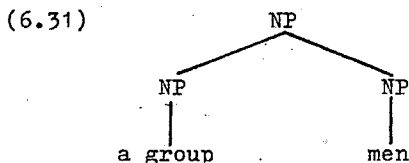
(6.29) John gave the girl a kiss

This perfectly grammatical sentence clearly violates our putative constraint. One way out of this might be to accept the case theories presented in Fillmore (1968), and then state that NP NP sequences are ungrammatical only if both NP's are dominated by identical case nodes (but not necessarily one and the same node). To go even further, if we accept Anderson's (1971a) claim that it is preferable to formulate a case grammar in a dependency framework, we could reduce the constraint to adjacent identical case nodes. Thus we could preserve

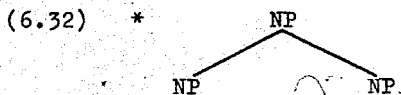
(29), yet (30) might still be ungrammatical:³

- (6.30) a *A group men
b *A gallon whisky

But there is no need to accept case theory in order to accommodate these facts. If we modify (7) so that of is present, as in (30a), we obtain:



This shows that it is possible to constrain occurrences of (30) by a modification of the adjacent NP constraint which states that adjacent NP's which are immediately dominated by the same node do not form grammatical strings, that is:



The adjacent NP constraint will not block derivations of (29), for these derivations will not generate structures

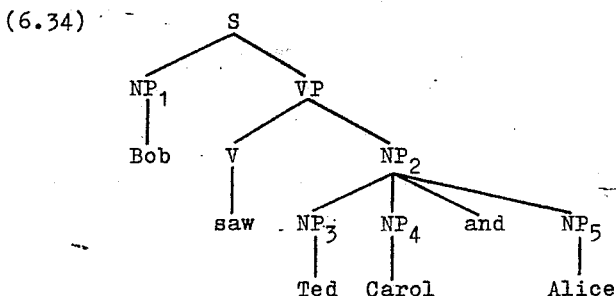
³ Of course, it remains to be shown that the two nouns in the examples of (30) are dominated by identical case nodes. Also, it is useful to compare here the Dutch forms een groep mannen and een liter whisky.

of the form (32).⁴

However there appears to be at least two types of surface structure constructions which involve violations of this constraint. Consider firstly the case of lists or coordinations:

(6.33) Bob saw Ted, Carol and Alice

This must be assigned something like the following surface structure:



or at least that would appear to be the case. But it will be observed that NP₂ dominates an asymmetrical set of nodes. Although the argument cannot be discussed in depth here, there can be little doubt that in shallow structure, cf. Postal (1972:42), there is also an and between NP₃ and NP₄. One might conclude from this that (32) applies at that level rather than the surface. However, it is interesting to consider the intonation

⁴ A similar constraint is to be found in Chomsky (1970:41-42) and Jackendoff (1972b:135), see below for further comments.

and stress patterns of sentences such as (33). It seems to be the case that between NP₃ and NP₄ there is some kind of phonological marker, perhaps the "silent stress" mentioned by Abercrombie (1967:35-36). Even if the present theoretical state of transformational grammar is not properly equipped to deal with such a phenomenon, that is hardly a reason for disputing that there must be some element between NP₃ and NP₄ at surface structure upon which, as it were, to peg the phonological event. Once that is done, we can see that even at the level of surface structure (33) will not violate (32), as does (34), and that therefore it is not a counter-example to the claim that (32) operates at the surface level.

The second possible counter-example is found in phrases of the type:

(6.35) A sausage salesman

The validity of this counter-example rests upon a claim that two NP's are present in (35), one of which dominates sausage, the other salesman. But such a claim has the unfortunate consequence that it then seems impossible to explain a correctly. The 'indefinite article' clearly collocates with salesman, as can be observed if we pluralise (35) in different ways:

(6.36) a A sausages salesman

b Sausage salesmen

That being the case, however, a should appear after sausage, not before it. The only way to avoid this

would be to segmentalise (see above) the a out of its NP to a position before the left-adjacent NP. This seems highly implausible and in any case does not explain why sausage cannot have an 'article'. But two other hypotheses will explain the paradigm: either (35) contains a compound noun or sausage is there an adjective. There is no need for us to decide between the two, although the latter may be the more probable, since both show that there is no string NP NP and thus that (35) is not a counter-example to our constraint.

It is not relevant at the present time to consider why particular items are inserted between adjacent NP's. It seems quite certain, however, that and and or are markers of coordination. This leaves the way open for of to be the marker of non-coordinating relationships, including, but not exclusively so, subordination. If in fact of does have such a wide range, this will help to explain the multiply ambiguous nature of the genitive construction. One important point is that there will be no need to demand that possessives and the constructions we have discussed above have very closely related underlying structures. All that they need have in common is that there are in surface structure two adjacent NP's related by a means other than coordination.

We may therefore conclude that the structure given in (7) is perhaps the correct surface structure for

Group I constructions, but it is certainly not the correct underlying structure.⁵ We appear to have wandered some way from the syntax of quantifiers by now, but this is not so, for Jackendoff (1968:427) claims that Group III words, e.g., many, three, have the same structure as Group I words. Thus we are already in a position to state that Group III words, or 'absolute' quantifiers, cf. §6.2, do not have quite the underlying structure which Jackendoff claims for them.

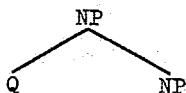
Jackendoff bases his claim on the apparent fact that Group I words and the 'absolute' quantifiers have virtually identical surface structure patterns. The only difference, he claims, is that of is deleted when the following NP is 'nondefinite', since (37) is ungrammatical:

(6.37) *Many of men

This is an interesting point, since it bears clearly upon our putative constraint (32). Let us suppose that many is not an NP in surface structure, whatever it may be in underlying structure, but simply a quantifier. We can then propose:

⁵ Definitely not in the case of a pair. Note that we have now provided an explanation of the occurrence of of after a pair, which we had not previously done, and which might have been thought to be a sin of omission.

(6.38)



as a surface structure for (37). It will be noted that then no of-deletion will be required, although it will still be possible to derive (6a). This appears to be a significant improvement on Jackendoff's formulation, since we do not require his ad hoc rule to delete of, but can appeal to a rather more general grammatical constraint. Indeed, Jackendoff (1972b:135), like Chomsky (1970:41-42), seems to accept that some kind of of-insertion rule is needed in the grammar, and this greatly strengthens our case against his.

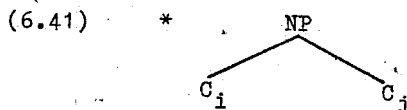
Jackendoff (1968:428) claims, however, to have found several counter-examples to this solution, which are to be found in the paradigm:

- (6.39) a Guess what we don't have any of:
insect repellent
- b We don't have any insect repellent
- c *Guess what we don't have any:
insect repellent
- d *We don't have any of insect repellent

Jackendoff suggests that preposing of what in (39a) prevents the of-dropping which occurs in (39b). Therefore an of-dropping rule seems necessary, but this ignores the fact that we find:

- (6.40) We don't have any of what?

Jackendoff's rules would predict of-deletion here. It might therefore seem reasonable to suggest that the constraint (32) might be modified to:



where C_i and C_j are identical grammatical categories. There does seem to be a good case for this, as can be observed from the existence of strings such as:

(6.42) Three of seven of the men

but it is dubious as an explanation here, for we find in addition to (40):

(6.43) We don't have any what?

There is also the fact that no justification has been given for assigning any and what to the same grammatical category. This latter objection is avoided by Klooster (1971:247), who gives, in effect, a rather more restricted variant of (41), but his proposals cannot account for the grammaticality of both (42) and (43).

But consider now possible answers to these two questions. To (40) one may answer:

(6.44) We don't have any of the tomatoes
and to (43):

(6.45) We don't have any tomatoes
but the answers cannot be reversed. This would suggest, contrary to Katz and Postal (1964:91-93), that what may be either 'definite' or 'indefinite', according to

circumstance. Now in the case in which it must be 'definite', (40), we find of, in the other case there is no of. If we agree with Jackendoff, uncontroversially, at least at present, that of is not deleted before 'definites',⁶ but still claim that of is not present in surface structure before 'indefinites', which implies quite different processes of derivation in the two cases, we can explain these cases simply, without needing the of-deletion which Jackendoff proposes.

Even if it is correct that of-deletion is not needed to generate the correct surface structures for constructions involving Group I and Group III words, this does not have the consequence that Jackendoff's major assertion - that 'absolute' quantifiers are at least very closely related to nouns in their syntactic behaviour - is incorrect. Indeed, we may say that that assertion has in no way been denied. We can therefore conclude that we have not yet found much evidence, although we ought to bear in mind the case of few, to substantiate a claim that 'absolute' quantifiers are not noun-like. There is in fact a fair amount of evidence which will cast doubt upon Jackendoff's position, but we shall discuss that in the section which follows. So far,

⁶ It should be remembered that our remarks above suggest that of is never deleted, but rather that it is occasionally inserted, due to (32).

however, our argument has primarily been that Jackendoff's proposed underlying structures are insufficiently 'deep'.

6.4 Quantifiers as 'articles'

Group II quantifiers are distinguished from Group III quantifiers at two points in their surface structure. Firstly, they may not be preceded by a 'definite article'; secondly, the Group II quantifiers may be subdivided into 'singular' and 'plural' quantifiers, and the former of these subsets may take one when followed by a 'definite' complement (Jackendoff, 1968:437). These two differences are exemplified by:

(6.46) a *The some men

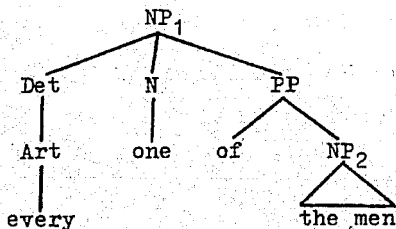
b Each (one) of the men

After a consideration of various alternative analyses, Jackendoff (1968:439) proposes (49) and (50) as the most adequate underlying structures for (47) and (48) respectively:

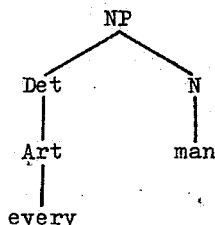
(6.47) Every one of the men

(6.48) Every man

(6.49)



(6.50)



A transformation named "ones-absorption" then deletes one in (49). This transformation is probably optional in the case of the 'singular' quantifiers, except that it is blocked with every, and obligatory with 'plural' quantifiers.⁷ Group II quantifiers will never appear in postdeterminer position because they are determiners themselves and of will not be deleted (from (49)) because N_1 is not "[+Q]".

Jackendoff thus appears to have three different reasons for postulating radically different underlying structures for Group II quantifiers against Group III quantifiers: (i) the surface appearance of one; (ii) the lack of a grammatical postdeterminer position for Group

⁷ Jackendoff (1968:440) offers an alternative notation to handle ones-absorption, but, as he says, "there are no different claims made by these two variants". We shall therefore confine our remarks to the one variant which we have outlined and assume, with a fair amount of certainty, that these remarks apply equally to the other variant.

II; (iii) the distribution of of. In fact this third reason plainly does not exist: the distribution of of is identical for Group II and Group III, for of appears before the but not before an 'indefinite' noun. The mere fact that Jackendoff gives quite different underlying structures serves to disguise this, and therefore the parallelism of the distribution is a strong argument against making the distinctions which Jackendoff makes.

Of the other two reasons, let us first discuss the occurrence of one. The formalisation of the ones-absorption transformation claims that every and the 'plural' quantifiers are marked items and that the 'singular' quantifiers are unmarked. This is because every must not undergo the transformation and the 'plural' quantifiers undergo the transformation obligatorily. In the other cases the transformation is optional. In terms of Lakoff (1970b) every is a negative absolute exception and the 'plural' quantifiers are positive absolute exceptions. Now while it seems correct to consider every a marked item, it is rather less obvious that this is the case with the 'plural' quantifiers.

In the first place, these quantifiers have exactly the same distribution with respect to one as do the quantifiers in Group III, which suggests that it is they which are normal. In the second place, the very term 'singular' quantifier is something of an apparent

contradiction, for, with the exception of one, which is clearly unique, we might expect quantifiers to operate over plurality, or, in the case of much, etc., which collocate with mass nouns, at least over non-singularity. Indeed, the referents of any NP including a 'singular' quantifier are always at least two, given the exception of mass nouns or not. In order to handle this, it would appear that we shall have to propose that (48) has an underlying structure corresponding to *every men, and that a later transformation accounts for the shift in number to singular. This is therefore another case of the distinction between semantic and syntactic number discussed in §6.3.

Now consider what happens in the case of (46b). If we choose the option without one, we have what appears to be a reasonable reflection of some underlying structure parallel to that for (48), at least with respect to the expression of plural reference. But it is impossible to make men singular here, for then we obtain:

(6.51) *Each of the man

The reason for this is presumably that the semantic plurality is disguised (although we shall discover more compelling reasons in Chapter 9), perhaps because two NP's, if we accept Jackendoff's analysis, are then singular. However, the exact explanation is not necessary here, only an exposition of the surface facts which show that men must remain. Now if men cannot carry the

marker of syntactic singularity, it seems reasonable to suppose that the quantifier should do so. But quantifiers show no apparent marker of number. The obvious explanation, therefore, is that one is inserted precisely to carry the syntactic singularity. This does not explain why every must collocate with one in such constructions, and each, either, etc. only optionally have one, but it does explain the various distribution of the Group II quantifiers in a revealing manner, especially in that it claims that every and the 'singular' quantifiers are more marked than the 'plural' quantifiers, with every the most marked of all, which accords with the intuitions of the native speaker; and we shall be able to observe in Chapter 11 that this solution produces a useful parallelism with the syntax of the 'indefinite article'. A further consequence is that it can now be stated that the appearance of one is not a justification for distinguishing between Group II and Group III quantifiers in the way that Jackendoff does. One-insertion does not require the underlying structure of (49), but works equally satisfactorily with (7).

Thus the only reason remaining for Jackendoff's claim that Group II and Group III quantifiers have the different underlying structures which he proposes is that only Group III quantifiers appear in postdeterminer position, cf. §§5.3 and 6.2. We have already noted that Lakoff (1970d) and Garden (1970c) use the same fact to

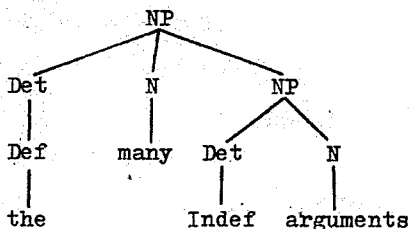
justify their claim that all quantifiers are predicates in underlying structure. In §5.3 we discussed the validity of that claim and concluded that although the evidence which had been cited did suggest that the 'absolute' quantifiers involved an underlying predication, the hypothesis that any or all quantifiers involved only an underlying predication had not been justified. Now Jackendoff (1968) denies that any predication at all is involved in the underlying structure of even 'absolute' or Group III quantifiers. And so we have to decide, firstly, whether or not this leads to any failure in generating the correct surface structures. Quite simply, the answer is that it does, for Jackendoff is unable to generate the occurrence of 'absolute' quantifiers in predicate position, and it is far from certain that he can emend his analysis to do so, cf. §6.2, above, for further discussion.

Perhaps, however, it is best that we attempt to find other reasons for concluding that Jackendoff's proposals are incorrect, especially in view of the marginal status of quantifiers as surface predicates. Therefore, let us consider the claims made by Jackendoff about a construction such as (3a), repeated here for reference:

(6.3) a The many arguments

This must have the surface structure of (52), according to Jackendoff (1968:429):

(6.52)



(52) shows quite explicitly that in (3a) Jackendoff considers that many is 'definite' and that arguments is 'indefinite'. There are arguments against each of these propositions. Firstly, it is far from clear what it can mean for a quantifier to be 'definite', a point which we made in §4.1. If we accept the hypothesis presented there, that quantifiers do not have underlying deictic characteristics of their own, then (52) will have to be excluded from the grammar as impossible. Secondly, all the evidence which we have lends weight to the belief that it is indeed arguments which is 'definite' in (3a). There is surely no difference in the scope of the deixis between (53) and (54):

(6.53) The arguments which were presented
in the previous section are all
equally specious

(6.54) The many arguments which were presented
in the previous section are all
equally specious

The problem would appear to be that Jackendoff has an excess of Determiner nodes, and he is therefore obliged

to make the wrong node 'definite'. Only one Determiner node is needed in (3a), and that must certainly be a sister node of the N dominating arguments. But in order to achieve that, Jackendoff would have to abandon the claims he makes about the nominal status of quantifiers such as many.

The conclusion which we must draw from this is that Jackendoff's underlying structures do not represent correctly the syntactic and semantic facts about Group III quantifiers. Further, we have seen that none of the three reasons for distinguishing Group II quantifiers in the way that Jackendoff suggests is satisfactory, for the only possible reason - that Group II quantifiers do not appear in postdeterminer position - has been misinterpreted by Jackendoff. On top of all this, we have already noted that Jackendoff's suggested underlying structures are simply not 'deep' enough, for both syntactic and semantic reasons. For example: his Group I is in some measure a rag-bag of quite distinct items; the status of of is more closely related to surface than to underlying structure, at least as far as structures like (6a) are concerned; he is unable to account for the distribution of few, and perhaps a few, correctly.

Interestingly, however, we have not found any evidence which completely excludes the possibility that Jackendoff is correct in claiming that quantifiers have

some of the syntactic (and semantic) characteristics of nouns. Certainly, we have noted that he fails to account for their predicate-like behaviour, but it may be that quantifiers are in some way a combination of nominal and predicate features. This would imply that we have to search for the means by which the more justifiable elements of the Lakoff-Garden hypothesis can be reconciled with Jackendoff's theory and within one theory. The reconciliation of these nominal and predicate features will be the subject of study for Part III.

6.5 Interpretive rules for quantifiers

It was mentioned in §6.1 that one contrast between Jackendoff's position and that of Lakoff and Garden was that Jackendoff claims that the underlying structures do not necessarily contain all the information which is needed for semantic interpretation of the surface structure. Therefore, Jackendoff states, rules of semantic interpretation which operate at at least one level which is not the level of underlying structure are required. A clear example of such a rule is to be found in Jackendoff (1969:232):⁸

⁸ For definitions of "affective" and "in construction with" see the article quoted. Jackendoff (1972b:348) gives an amended variant of this rule, intended to cohere with a wider range of semantic interpretation rules.

"In this formulation [of the some-any rule: RMH] we will consider some and any ... as separate lexical items differing by a feature, say [X] (some is [+X]). There will be rules of semantic interpretation which specify which value of the feature must appear in what environment, much as a selectional restriction specifies features of NP's in relation to verbs.

The rule will be stated more precisely as follows:

- (58) [+indeterminate] ----->
- | | |
|---|-------------------------------------|
| { | [-X] in construction with Affective |
| } | [+X] elsewhere |

The convention for application of this rule is as follows:

- (59) If an indeterminate is unspecified with respect to X, the rule fills in the feature according to the environment. If the indeterminate is already marked with respect to X, the sentence is marked semantically anomalous if the inherent feature and the feature assigned by the rule disagree."

As Jackendoff (1969:233) points out, there is no essential difference between the second part of his convention

and a filtering transformation which might block incorrect constructions. Only the first part, which adds semantic interpretation after the underlying structure has been generated, is in contradiction of the principles of generative semantics.

Let us, therefore, look at the first part. Its purpose is to avoid having to generate two lexical items with an identical phonological structure, as, for example, a_1 which would be [+X] and a_2 which would be [-X]:

(6.55) a John bought a_1 house

b John didn't buy a_2 monkey-wrench

But this case, which forms Jackendoff's prime example, rests on two assumptions which he does not prove. The first of these is that there is no other justification for two sources for a . We shall see at a later stage that Jackendoff is probably correct in this respect, so we may accept that assumption. The second one is that a does have the two meanings which he describes. It is unlikely that that assumption is correct, for it is more probable that it is the specificity of the whole noun phrase in (55) that is at stake, cf. Chapter 11 and our earlier discussions in §§1.5 and 2.3. This, of course, is not necessarily a counter-argument to Jackendoff's claim, but if it can be shown that the specificity can be determined by postulating quite different underlying structures, Jackendoff's position is rather weaker. We shall, however, leave this point here and return to it

when we discuss the syntax of the 'indefinite article' in Chapter 11. See too our discussion of any in §10.2.

Jackendoff presents another argument, which mainly concerns the correctness of generating both some and any and then having a rule such as his (58) - (59) which blocks their ungrammatical occurrences. This argument is based upon the lexicalist hypothesis presented in Chomsky (1970), and states, inter alia, that transformations should not be used to generate morphological changes. This is a complex problem, but we need only note that Chapter 4 was an attempt to show that transformations are required to do exactly that. In so far as that attempt was successful, the lexicalist hypothesis can scarcely be considered binding, cf. too Anderson (1968), Postal (1970) and the works cited in §4.3. A further point is that since Jackendoff (1969: 235; 1972b:336-37, 341-42) argues against transformations inducing morphological change in Contemporary English because "unsystematic and sometimes drastic changes in 'spelling' occur", cf. §6.2, would he then argue that transformations performing the same syntactic task in Old English are required, precisely because the 'spelling' changes are phonologically regular and not at

all drastic, cf. Campbell (1959:113, 147)?⁹

We may therefore conclude that Jackendoff's examples do not show for certain that an interpretivist hypothesis must be accepted, even if a closer examination of his argument must await a later moment, cf. §8.4. The implication of this fact for Jackendoff's proposed underlying structures is most probably that they are insufficiently abstract. This was also our conclusion at the end of §6.4, and therefore it scarcely marks any progression in our argument. Nevertheless, it is a further consolidation of that argument, and we can fairly claim that an adequate solution of many aspects of the quantifier problem will be rather different from the one proposed by Jackendoff in his various papers.

6.6 Further analyses and conclusion

With our analysis of Jackendoff's hypothesis concluded, we have ended our discussion of the two principal hypotheses concerning the status of quantifiers in recent

⁹ Interestingly, Jackendoff is joined by R. Lakoff (1969a) in a rejection of Klima's (1964) some-any rule. But R. Lakoff would also reject the interpretivist hypothesis, so this is not relevant at present. The relationship between some and any, and the validity of any transformation which relates them, will be discussed in §10.2.

transformational theory. This chapter commenced with an outline of the contrasts between Jackendoff's theoretical stance and that of Lakoff and Carden. As was hinted at then, the theoretical standpoints are not in fact primary, for they have to be substantiated by relevant syntactic and semantic evidence. Therefore in §6.2 we examined a conflict in analysis between the two hypotheses, with respect to the status of few, and decided that the more serious disadvantages lay on Jackendoff's side, and that they were quite fundamental in origin.

In §6.3 we examined Jackendoff's claims about his Group I and Group III constructions. Here we found that in several details Jackendoff's hypothesis was faulty, yet this did not contradict completely his assertion that the so-called 'absolute' quantifiers were rather like nouns in certain (but not all) aspects of their syntactic behaviour. However, it was found to be the case that the proposed underlying structures were inadequate for an expression of the correct generalisations, and that therefore some alterations were necessary.

In the following section the evidence which led Jackendoff to propose two different underlying structures for 'relative' and 'absolute' quantifiers was examined. There appeared to be three pieces of evidence for his proposal. Two of these - the surface appearance of one and the distribution of of - were shown to be

false, in that there was no need to postulate different underlying structures in order to generate the correct surface distribution. The third piece of evidence concerned the postdeterminer position, which is only possible for 'absolute' quantifiers. It was shown here that Jackendoff's structures made incorrect claims about NP's with postdeterminer quantifiers, and that the Lakoff-Carden hypothesis was much more satisfactory in this respect. But the hypothesis that quantifiers are closely related to certain nouns, advanced in Jackendoff (1968), was not totally excluded. It was merely stated that an adequate account of quantifiers must make room for both that hypothesis and the hypothesis advanced by Lakoff and Carden that quantifiers have some sort of predicate status.

Finally, in §6.5 we discussed whether or not Jackendoff was correct in claiming that rules of semantic interpretation, probably operating on an intermediate structure, were necessary for the explanation of the semantic properties of quantifiers. We saw that there were two possible cases, but that neither of these cases was indisputable and that therefore there was no decisive evidence to favour a theory of interpretive semantics. On the other hand, there was some evidence which strongly supported the theory that the underlying structure generated by the base rules should contain all the necessary information for the semantic comprehension of

eventual surface structures.

In Chapters 5 and 6 we have confined ourselves almost exclusively to a discussion of the theories of Lakoff, Carden and Jackendoff. This should not be taken as a denial that other scholars have worked on the problems surrounding quantifiers, either within the theory of transformational grammar or in totally different terms. But it seems fairly clear that these three scholars have been the originators of recent quantifier theory and that it is they who have provided the most extensive analyses. Just as it is impossible to discuss Jackendoff's work in the terms put forward by Lakoff and Carden, although they can be compared, so it is always more natural to discuss other work in terms of either Lakoff and Carden or Jackendoff.

Perhaps the most original extension of the theories of Lakoff and Carden is to be found in Anderson (1973c and forthcoming). Anderson agrees with Lakoff and Carden in that quantifiers are represented as some sort of superordinate, i.e., they stem from a higher S, but he disagrees in two respects. Firstly, he claims that quantifiers are the subject of an existential predicate. This naturally leads to the second difference, which is that quantifiers, Anderson claims, are either nouns, or nouns with a modifying predicate. We shall discover in Part III that this suggestion is not very distant from

the one that I wish to propose, and so the differences which do exist are discussed then, otherwise the proposed solution will be over-anticipated and thus prejudiced.

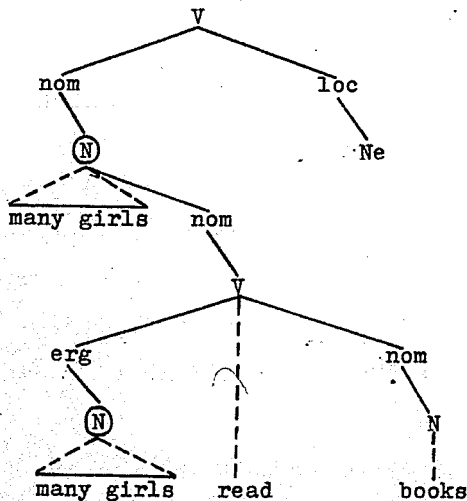
One point, however, is worth mentioning now, and that is that Anderson works within a theory of dependency case grammar, asset out in Anderson (1971b).

Therefore, for:

(6.57) Many girls read books

Anderson (1973c:125) gives the following structure:

(6.58)



At the present crude stage of grammatical theory it seems to make no great difference whether one works within a dependency or constituency framework, within an 'NP VP' or a 'case' framework. At least this seems to be true in respect of the grammar of quantifiers. Thus it does not seem to me that Anderson's statements are of

a different kind from those of, say, Lakoff, except in what status he ascribes to quantifiers, and since a 'translation' from the one system to the other should thus be possible without all the losses normally implicit in that process, we shall ignore the differences which are only a product of the basic theoretical division.¹⁰

There has been very little extension of Jackendoff's hypothesis by other scholars, with the possible exception of a paper by Force (1968) and the more definite one of two articles by Dougherty (1970, 1971), both of whom introduce an element Q into the base rules. In the former case, however, there seems to be little other than a notational variant of earlier work on quantifiers which we discussed in Chapter 3 and which Jackendoff (1968:429-32) rightly dismisses. The papers by Dougherty give insufficient evidence to determine the extent to which his position differs from Jackendoff's, but

¹⁰ A test of the relative adequacy of dependency case grammars and constituent NP VP grammars with respect to quantifiers can only be made if we first have reasonable analyses within both theories. Here we can only hope to attempt that for one of the two. That the choice is somewhat ad hoc has to be admitted, but only after the consequences of that choice have been worked out can its ad hoc-ness be evaluated.

whatever the extent of that difference is, it is not very great, and therefore there is no need to discuss his position separately. Dougherty (1970:864-66; 1971: 333-35) departs from Jackendoff in claiming that quantifiers collocate with any 'major category', i.e., S, NP or VP, although certain restrictions hold with a number of the quantifiers. But at present we are only concerned with quantifier - noun relations, so we shall omit discussion of that point too, apart from noting that although it may be the case that it does, Dougherty's hypothesis need not necessarily conflict with Jackendoff's analysis.

We can now, therefore, claim to have concluded a fairly extensive survey of the various analyses which have been proposed to explain the grammatical behaviour of quantifiers. Although this survey has been critical, that adjective should not be confused with negative, for we have uncovered, admittedly bit by bit, many of the more important facts about the grammar of quantifiers. Nor is the task of discovering the errors, if errors they be, of other scholars entirely fruitless, for they may help us to avoid similar mistakes when we draw the various strands together in an attempt to form a workable theory for ourselves. The construction of such a theory is the aim of Part III.

Part Three

English Quantifier Systems



Chapter Seven

Simple existential quantifiers

7.1 Heuristic preliminaries

In Parts I and II we have examined a considerable variety of analyses of the English quantifiers, often made quite bewildering by the fact that the survey was far from exhaustive and by the further fact that many, if not most, of the analyses reached conclusions which contradicted the conclusions of other analyses. Even the two most recent theories, which we might assume to have benefited from an examination of earlier theses, are startlingly opposed, for Lakoff and Carden have suggested that quantifiers are underlying predicates, Jackendoff that they are underlying nouns or, in some cases, nominalised 'articles'. We can reasonably suggest three fundamental factors at work in the production of these contradictions: (i) basic theoretical opposition, e.g., 'parts of speech' grammar vs. transformational grammar, is certain to produce opposing analyses; (ii) as quantifiers form a notoriously difficult part of the grammar of English, indeed, one suspects, of the grammar of any language, one ought to expect a wide range of opinions; (iii) there is such a wide range of grammatical distribution for each individual quantifier that a close analysis of one quantifier may lead to a

hypothesis quite different from that suggested by the grammar of another quantifier. Faced with such difficulties, it would be surprising if every student of the English quantifier system did not feel at least a twinge of sympathy with Kruisinga when he remarks (1932a:129):

"The chapter on indefinite pronouns [i.e., quantifiers:RMH] may be considered the lumber-room of the pronouns; and a lumber-room may be as convenient in grammar as it is in a house."

Fortunately, however, there are a number of heuristic devices available which will enable us to cut down the variations in the grammar of quantifiers to manageable size; of these we shall employ two. But before mentioning what they are, it is advisable to emphasise the modest status of these devices. One can never approach a scientific problem in vacuo; one must first have some notion of previous theories and one must also have some idea of the kind of hypothesis which is likely to be fruitful. In other words, in order to get the 'right' answers one has to know what the 'right' questions are. This implies that the kind of heuristic decisions which we make below are to some extent a product of the hypothesis which they themselves are supposedly to help us reach. Yet this does not have the seriously damaging consequences which might be suspected,

namely that the complete argument is a vicious circle. One important reason for this is that it is hoped that the heuristic devices have a certain grammatical justification in themselves. But there is still another reason: if the hypothesis suggested below is incorrect, then this can stem from three causes. Firstly, it may be that the grammatical theory employed, here a version of recent transformational grammar, could be inadequate. We have stated as an a priori that it is adequate, but it can hardly be denied that there is ample room for doubt. Secondly, it may indeed be the case that the wrong heuristic devices were used, the 'wrong' questions asked. Thirdly, it is all too possible that the argument constructed below will be incorrect. If we bear all these factors in mind, it will be recognised that the appropriateness of the heuristic devices employed, although important, is not necessarily crucial in determining the adequacy of the hypothesis eventually proposed.

In view of the wide range of quantifiers, the first device which we must employ is one that divides the quantifiers into various subsets. It is interesting to note in this respect that Lakoff and Carden agree with Jackendoff in making a bipartite division and that this division assigns the several quantifiers to virtually the same subsets. Lakoff and Carden make a distinction between 'absolute' and 'relative' quantifiers, this terminology being derived from Partee (1970:157). In

the first subset are included quantifiers such as many, five, few, cf. §6.2; in the second we find some, every, all, none. The semantic intuition here is that the first type of quantifier describes the 'absolute' size of a set, whereas the second describes a certain proportion of a set or the 'relative' size of a set, cf. Partee (1970:157-58), Lakoff (1970d:396). For a number of reasons, which it would be premature to consider at this moment, I find this terminology misleading, but terminological quibbles aside, there is certainly some evidence that such a division is justifiable. Much of the evidence stems from the fact that the semantic intuition closely accords with a syntactic distribution. As Lakoff and Carden both have pointed out, and as we have observed in §5.3 and elsewhere, 'absolute' quantifiers occur grammatically in postdeterminer contexts whereas 'relative' quantifiers do not. The reason why Lakoff and Carden are in general (but not complete) agreement with Jackendoff is that the latter also uses this syntactic criterion for subdividing quantifiers; thus Jackendoff's Group II quantifiers cannot appear in post-determiner position whereas his Group III quantifiers do appear there. If there are discrepancies in the placement of individual quantifiers, as is the case with few, cf. §6.2, this is largely due to differing claims about the grammatical distribution of the quantifier involved.

Despite the unanimity displayed above by otherwise contradictory theories, it is by no means certain that the division is as simple as that. The thinking behind this assertion is that the grammar of quantifiers is characterised not by one surface distinction with one semantic correlate, but by two surface distinctions with two semantic correlates. Thus I would claim that there are three fundamental groupings of quantifiers (this, of course, is to ignore for the present Jackendoff's inclusion of a group, etc. in his study, an inclusion which we shall show below to have much justification). Of course, it may be argued that it is not too difficult, if the linguist is sufficiently pedantic, to find n distinctions, which would lead to the creation of at least n+1 subsets. But I would claim that the two criteria named below are the only simple and discrete criteria to be found in surface structure (and heuristic devices cannot readily appeal to the theoretical construct of underlying structure) which are also directly associable with semantic intuitions or facts.

The two surface syntactic criteria of which we shall make use are: (i) whether or not the quantifier can appear in postdeterminer position, cf. above and Carden (1970c); (ii) whether or not the quantifier can be directly preceded by and be within the scope of the negator not. Both criteria, of course, are applicable only at surface structure. Applying these two criteria

to the three quantifiers some, many and all, we find the following paradigms of grammaticality, where some is the most restricted and many the freest of the three:

- (7.1) a Some cricketers write poetry
b *The some cricketers write poetry
c *Not some cricketers write poetry

- (7.2) a Many cricketers write poetry
b The many cricketers write poetry
c Not many cricketers write poetry

- (7.3) a All cricketers write poetry
b *The all cricketers write poetry
c Not all cricketers write poetry

Although the criteria applied here appear to be relatively clear-cut, there are some cases where classification of a particular quantifier is problematical - this is especially true of few. Thus, although we find:

- (7.4) The few cricketers write poetry

it is extremely difficult to ascertain whether this is an instance of few or a few, and consequently whether or not few appears grammatically in postdeterminer position.

Furthermore, (5) is ungrammatical:

- (7.5) *Not few cricketers write poetry

but if we accept Lakoff's claim (1970d:395) that few is derived from not many, then it is obviously the case that the ungrammaticality of (5) is independent of the criterion (defined linearly at the surface) of not-

precedence; not only that, but few ought to accept the paradigm of many throughout. This, of course, leads to a natural solution to the status of few in (5), but before we can follow that through we shall have to examine few and not many in more detail, cf. §6.2 and Chapter 8. Another case in which our criteria are not wholly adequate, it might appear, is in the relative unacceptability of:¹

(7.6) ?*All boys came to the party

But since (3a) is certainly acceptable this must be due to aspects of the syntax which are outside the rather narrow criteria we are using here; again, this is a matter to which we shall return later, in Chapter 9.

The first of the semantic criteria to which the above syntactic criteria relate is approximately that given by Partee (1970) and adopted by Lakoff: only those quantifiers which can appear in postdeterminer contexts describe the size of the set relative to one's expectations, cf. Lakoff (1970d:396, note 6); the other quantifiers simply describe the proportion of the full potential set of referents, cf. §4.2, whether that be the total proportion (all), a partial proportion (some) or

¹ American speakers especially find (6) considerably more acceptable than I do. Indeed, I find the sentence totally unacceptable, in the intended meaning, where all ≠ only. We shall return to this point in Chapter 9.

the null proportion (no/none). However, I would claim that it is the second criterion which is of greater importance. This criterion is that there is a set of universal quantifiers and a set of existential quantifiers. The terms used here are, of course, borrowed from symbolic logic, and an explanation of their meaning can be found in any elementary handbook on logic, e.g., Reichenbach (1947:87-91). Crudely speaking, and for linguistic purposes only, a universal quantifier indicates that the set of referents of the collocating noun is equivalent to the full potential set of referents; an existential quantifier indicates that reference is to some nonnull, but non-equivalent, subset of the full potential set of referents. Thus all is a universal quantifier; some and many are existential quantifiers. It should be noted that no/none has no surface position within this classification. Rather, it must be regarded as the negation of an existential quantifier, compare the position in logic, again outlined in Reichenbach (1947:91-96). To some extent this is in conflict with the findings of the first criterion, but we shall assume that the second criterion overrides the first. Regrettably, the second semantic criterion correlates with the syntactic criteria only in a rather complex manner: only those quantifiers which cannot appear in postdeterminer position but which can appear immediately after not are universal quantifiers. All the other quantifiers are existential quantifiers. Even these generalisations are

not wholly true: for example, each, although it is a universal quantifier, does not appear after not:

(7.7) *Not each cricketer writes poetry

But this, we shall suggest, is the product of some other syntactic restriction, cf. §9.5.

We are now in a position where we can set up a systematic classification of quantifiers on the basis of our first heuristic device. Combining the semantic and syntactic criteria we may call the three resultant types of quantifiers non-negatable existential, negatable existential and universal quantifiers. However, for reasons which will become apparent only as this study proceeds, we shall call the first two classes simple and compound existential quantifiers respectively. For heuristic reasons once more, we shall assume that there is a paradigmatic quantifier for each subset: for simple existential quantifiers it is some; for compound existentials, many; for universals, all. It should be understood that in claiming that these three quantifiers may act as models we are not further claiming that, for example, every universal quantifier behaves like all, or that many is the only compound existential. All that is being claimed is that any universal quantifier will behave in broad outline as all does, and we shall, indeed, have to spend some considerable time in explaining how and why individual quantifiers diverge from the proposed paradigm. In fact, we shall be able to indulge

ourselves with the paradox that a paradigmatic quantifier may itself diverge from the paradigm, cf. the unacceptability of (6) compared with (3a). Nevertheless, the basic facts can be clarified with the help of the following diagram:

(7.8)

	Context	
	Preceding Negative	Postdeterminer
some	No	No
many	Yes	Yes
all	Yes	No

Before we move on to the second heuristic device, it is essential to note that one quantifier (at least) does not fit neatly into the above discussion: this quantifier is any. Not only does this quantifier have a highly restricted distribution, as we have noted previously, especially in §§2.3 and 3.3, but it appears to share attributes of both existential and universal quantifiers. That it is connected with the former class can be exemplified by the following sentence:

(7.9) I didn't read any pre-papers

which is virtually synonymous with:

(7.10) I read no pre-papers

Only the negation of an existential quantifier could indicate reference to a null set. Any's relation to universal quantifiers is seen in the sentence:

(7.11) Any politician suffers from a
credibility gap

Although (11) is not paraphrasable by:

- (7.12) All politicians suffer from a
credibility gap

it is clear that in both cases reference is to the full potential set, and thus any is behaving there very much as if it were a universal quantifier. Further evidence that any has many of the attributes of a universal quantifier is to be found in the fact that its grammatical distribution is similar to that of either. However, since we have not shown that either is a universal quantifier this argument is rather premature. Because the grammar of any is so complex we shall not attempt an analysis of it until we have discussed the rather less complex quantifiers.

We can now proceed to a discussion of the second heuristic device, which, fortunately, is considerably clearer in outline than its partner. All quantifiers in English can occupy two positions: (i) immediately preceding a noun; (ii) as the head of a partitive construction with a 'definite' noun phrase. Examples are:

- (7.13) a Some men are bachelors
b Some of the men are bachelors
- (7.14) a Many men are bachelors
b Many of the men are bachelors
- (7.15) a All men are bachelors
b All of the men are bachelors

Although Lakoff and Garden make no significant distinction

between the two constructions, which causes considerable difficulty for the reader of their work, cf. Chapter 4, there can hardly be any doubt that such a clear distinction must be relatable to some important grammatical facts. Once again, it would be premature to discuss what these facts are now, but we shall assume that there are some, and therefore discuss non-partitive and partitive constructions separately. As this is purely an assumption it could be quite incorrect, but that is a problem we shall only consider in due course.

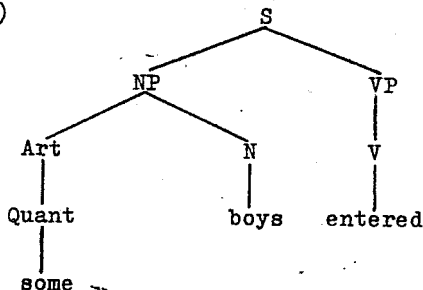
7.2 The structure of noun phrases

In the previous section we were able to establish, by means of two heuristic devices, a preliminary classification of the English quantifiers, and thus we can now proceed to an analysis of the structure of individual types. In the remainder of this chapter we shall concentrate on the grammar of simple existentials, of which we shall consider some the paradigmatic example. Although the restriction to one type of quantifier at a time is necessary in view of the complexity of the situation, it cannot ever be satisfactory to discuss simple existentials exclusively; obviously there will have to be occasional reference to other types of quantifier.

We have already observed that many different underlying sources have been postulated for quantifiers, from 'indefinite pronouns' to verbs and from nouns to 'straight-forward' quantifiers. Whatever the respective merits of these proposals, it can hardly be denied that in surface structure some is a quantifier or determiner. Thus an appropriate phrase marker for the surface structure of (16) might be (17):

(7.16) Some boys entered

(7.17)



In order to justify the claim that some has exactly that same status in underlying structure we have to satisfy two conditions. The first of these is that such a structure, or at least one which does not depart from that in (17) to any significant extent, is adequate as an analysis of noun phrases. The second, which is only applicable in the case that the first condition is satisfied, is that it is adequate to consider some as being dominated in underlying structure by the branch of the NP which dominates it in surface structure.

Since it is the first condition which is clearly prior, that must be the one which we examine firstly. We may take as the standard formulation of base rules to generate constituents of NP's that given in Chomsky (1965:107):

(7.18) NP -----> (Det) N (S')

where Det and S' are optional and the prime function of S' is to introduce embedded relative clauses. However, Bach (1968) gives a detailed critique of Chomsky's formulation and he concludes (1968:121):

"It is reasonable to propose that all nouns come from relative clauses based on the predicate nominal constituent."

Bach's alternative to Chomsky's formulation contains two rules, which will not be significantly different from (1968:92):

(7.19) a NP -----> Det + one + S
b S -----> Det + one + Aux + be
+ Predicate nominal

Thus the sentence:

(7.20) I spoke to the anthropologist
has an underlying structure which closely corresponds to:

(7.21) I spoke to the one [_Sthe one was
an anthropologist]_S

The rules in (19) form an exclusive alternative to that in (18) for otherwise, as Bach points out, (20) would be derivable from two sources, and that is quite undesirable,

although it cannot be ruled out on a priori grounds. Bach (1968:93-104) lists six arguments in favour of deriving nouns from a relative clause. It would be prodigal of space to repeat these arguments in full here, but a brief summary is necessary in order to judge the validity of his claim.

Firstly, there is the fact that:

(7.22) The idiot called me up yesterday is ambiguous. As Bell (1972:25) points out, the ambiguity is describable in logical terms by the contrast attributive - referential. Attributive usage is referentially successful if there is just one object which satisfies the description; referential usage is successful if the speaker and hearer know of just one object being referred to - although the description need not be true. Bach shows that this distinction can be made by distinguishing between restrictive (= attributive) and nonrestrictive (= referential) sources for idiot, thus:

- (7.23) a The one who is an idiot called me
up yesterday
b The one, who is an idiot, called
me up yesterday

The second argument derives from the grammar of negation; a sentence such as:

(7.24) The professors didn't sign a petition is three-ways ambiguous (at least). Negation of the following three elements can occur, as is shown by

possible stress and intonation variations: professors, sign, petition. If Bach is correct in arguing that negation is always underlying sentence negation, then the nouns must be derived from an underlying sentence, as he suggests. Thirdly, there is the fact that nouns may contain tense elements, 'although admittedly only traces of them remain. Thus consider the tense references of wife in:

- (7.25) a Before I met my wife she worked in
a library
b After I divorced my wife she went
to Afghanistan

Perhaps more obvious evidence of this is to be found when the prefix ex- occurs:

- (7.26) My ex-wife is living in a commune
in Afghanistan

Fourthly, Bach explains the grammar of attributive adjectives, which cannot be derived from an underlying relative clause where the adjective is predicative, by positing (28) as the source of (27):

- (7.27) I saw the alleged killer
(7.28) I saw the one [_Sthe one was allegedly
the killer]_S

Bach's fifth argument is that 'definite' NP's occur grammatically in predicate nominal constructions only under very restricted conditions, as is partly exemplified by (29), cf. further Bach (1968:103):

(7.29) a He is the person I was talking
about

b ?John is this philosopher

Bach claims that his proposed derivations for nouns permit justifiable generalisations to be made about such phenomena. The last of Bach's six arguments is that if I own no marsupial then (30) fails for lack of reference:

(7.30) My marsupial scratched me yesterday
The failure of (30) to refer, Bach claims, must be explained by postulating:

(7.31) Something which I have is a marsupial
as part of the underlying structure of (30).

Our problems would be considerably eased if it were the case that all of Bach's arguments were equally correct, but unfortunately this is not so: arguments 3 and 6, at least, seem false. Counter-examples to argument 3 can be found with a number of nouns which are not capable of having tense markers or some other indication of tense, as is the case with, for example, mother:

(7.32) a *My ex-mother has remarried.

b *My future mother is pregnant

Anderson (1968) suggests that my mother must be related in an adequate grammar to the woman who bore me, and some further justification for such an underlying representation, extended to father and some instances of parent, is to be found in Hogg (1973b). Observe that there is no way of altering the tensed verbal in who

bore me in order to produce *my ex-mother or *my future mother. On the other hand, with my wife we can postulate the following three forms:

- (7.33) a my wife : the woman to whom
I am married
- b my future wife: the woman I am to
marry
- c my ex-wife : the woman to whom
I was married

This means of accounting for the difference between my mother and my wife, although superficially not very different from Bach's, is in fact sufficiently distinct for it to provide no direct support for his exact derivation. A preferable derivation only necessitates the introduction of lexicalisation rules along the lines suggested for both in Chapter 4, which do not obviously contain predicate nominal constructions. With respect to his sixth argument, (30) could also be derived from:

- (7.34) The marsupial which I have scratched
me yesterday

and this will explain the failure of reference equally well. For some justification of that derivation see Lyons (1967).

Of the remaining arguments, I believe that only the first is totally convincing. Thus: argument 2 is dependent on the claim that element negation is inadmissible in underlying structure; argument 4 is admitted by Bach

(1968:102-3) to be dependent on some rather obscure elements in the grammar; argument 5 is possibly the strongest of these three, but it must be noted that Bach's case depends on the equivalence of the N is N relation in sentence (29a) and that in:

(7.35) John is a philosopher

Whereas (29a) is an expression of identity, this is not the case with (35), which expresses a membership relation. It is not at all certain that Bach's analysis explains or clarifies this distinction, cf. Plötz (1972: esp. 82ff.) for further discussion. But even if only one of the above six arguments is wholly convincing, this is nevertheless sufficient ground for supposing that noun phrases have a considerably greater complexity in deep structure than is supposed by Chomsky (1965) and other studies within that framework. And we shall certainly be able to see in Chapter 12 that the first argument, concerning attributive and referential sources for NP's, is neatly paralleled by the distinction between cataphoric and anaphoric reference which is drawn there. Furthermore, this distinction is related to the occurrence of restrictive and nonrestrictive relative clauses.

The problem arises, however, that it appears to be the case that Bach's arguments only justify deriving the from some position outside the noun phrase (for the examples supporting argument 1 only relate to 'definite' noun phrases). Yet, and this seems only reasonable,

Bach wishes to extend his hypothesis about the position of the to the quantifiers, as the following statement shows (Bach, 1968:106):

"It is natural to think about adapting a system of operators (quantifiers) like those used in logical systems and allowing these operators to function with the variables in the deep structures of sentences. The class of operators will include the more abstract elements underlying such forms as articles, some and the like."

Furthermore, Bach (1968:106-7) shows that if we abstract quantifiers from noun phrases in underlying structure, then we account for the ambiguity of:

(7.36) She wants to marry a man with a big
bank account

The two interpretations of (36) can be clarified by the representations of (37):

- (7.37) a Some x [_gx has a big bank account
and she wants to marry x]
- b She wants [_gsome x [_g x has a big
bank account and she marry x]]

The difficulties facing us are now fairly clear. We have seen that the behaviour of the is best explained by assuming that in underlying structure it is not a component part of the noun phrase with which it is associated in surface structure, contra Chomsky (1965)

and others. Furthermore, there is evidence that the same is true of quantifiers such as some. Therefore we must look for some position in underlying structure where the, some, etc. may be placed outside their collocating NP, yet which guarantees their being generated at the correct point in surface structure. Also, it has to be determined whether or not the underlying position of the is the same as that of some. This latter question must be delayed for the moment, until we turn to a consideration of the grammar of the in Chapter 12, but that must not deter us from searching for an adequate analysis of some.

7.3 Some underlying requirements

By far the simplest and most appealing answer to the question "To which grammatical category does some belong?" is: "Some is a quantifier", and this is precisely the answer exploited by Force (1968) and also, it would appear, Lakoff (1971c). Indeed, the correctness of that answer can hardly be denied if we accept, further, that some is the 'basic' or paradigmatic quantifier. But observe that the above answer is a definition and that by itself it gives no explanation. What we need to do is relate some, or the category of quantifiers, to other grammatical categories, so that we may observe both distinctions and equivalences. Answering by definition is of no help here; it is the relationship

of some to previously well-defined categories which is crucial. Let us accept, approximately following Chomsky (1965), that those categories are N, V and A (noun, verb and adjective), although this ought not be taken as a denial that verbs and adjectives may have the same underlying source, as is suggested by Lakoff (1970b: 115-33).

We have already examined, in Chapter 5 and especially §5.3, the claims of Lakoff and Carden that some is an underlying predicate, and concluded from this examination that there is rather more negative than positive evidence concerning those claims. There is therefore no need to repeat those arguments here. In Chapter 6, however, we examined the suggestion by Jackendoff that some is a 'nominalised article', in other words that it is an 'article' in deep structure which is converted by some transformation into an NP in surface structure, cf. Jackendoff (1968:439). This proposal, it will be recalled, was rejected for three reasons: firstly the consequent phrase marker had too much 'structure'; secondly, there was insufficient motivation for the transformations which were proposed; thirdly, the underlying structures assigned to some a status which was felt to be too distant from that assigned to quantifiers such as many.

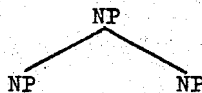
It is this third point which is most important here, for Jackendoff suggested that many was a noun in underlying structure. One of Jackendoff's arguments in favour of such a hypothesis was the similarity between the behaviour of some noun phrases expressing quantity, such as a number, and the behaviour of some quantifiers. In this context it is therefore extremely interesting to note the very close semantic relation between (38) and (39):

(7.38) A number of men came to the party

(7.39) Some men came to the party

Although it would probably be an exaggeration to claim that the two sentences are paraphrases of one another, cf. §8.1, it is certainly the case that they are almost so, and therefore we should attempt to suggest analyses which are very similar, in order to reflect this fact. In §6.3 we already disposed of one objection to closely parallel analyses, namely that in (38) men is preceded by of, whereas this is not so in (39); in that section we showed that this difference is a purely surface one, caused by the operation of the Adjacent NP Constraint, repeated below:

(7.40) *



This explanation only holds, of course, if a number is an NP in surface structure, whereas some is not; but

there is very little reason for believing otherwise, and so (40) may be deemed adequate, but see the further discussion referred to above.

Perhaps the primary criterion which Jackendoff invokes to justify assigning different underlying structures to number and some is that the former can be preceded by the, the latter cannot, cf. Jackendoff (1968: 437). Thus we find:

- (7.41) a The number of men
b *The some men

However, in contradiction of the validity of this criterion, there is an interesting restriction on occurrences of number when preceded by the: this is that the predicate phrase associated with the number must refer to the size of the quantity referred to by the number. This restriction does not apply to most other nouns which Jackendoff relates to number, for example, group. So we find:

- (7.42) a A number of men came to the party
b A group of men came to the party
(7.43) a *The number of men came to the party
b The group of men came to the party

Conversely, it is only the number which permits quantity-referring predicates:

- (7.44) a The number of men who came to the party was five

- b *A number of men who came to the party was five
- c *The group of men who came to the party was five

Revealingly, however, we do find:

- (7.45) The group of men who came to the party were five in number

An explanation of this last sentence will be offered in Chapter 8.

The above distributions strongly suggest that it is incorrect to consider a number and the number as differing only in 'article': there appears to be some more deep-seated distinction which may be applicable only in terms of two different lexical items, number₁ (with a) and number₂ (with the). To what extent such an analysis ought to be pushed is a difficult question, but it does seem reasonable to suppose that number₁, if such an ad hoc distinction is permissible, has a distribution much closer to that of some than Jackendoff is suggesting: some can never be preceded by the and number can only be preceded by the at the cost of significant syntactic and semantic changes, changes more radical than are normally found with the alternation between the and a. Finally, it is also the case that the number has another restriction which the group does not have, for the former cannot occur with a 'definite' NP following of:

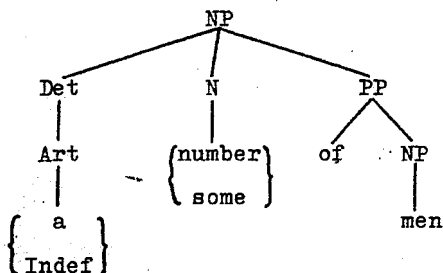
(7.46) a *The number of the men who came to
the party was five

b The group of the men who came to
the party were disgusted by the orgy

At the very best, Jackendoff's distinction between some and a number is on dubious foundations.

Let us suppose, therefore, that a number and some can be assigned similar underlying structures. If we accept Jackendoff's structures for Group I quantifiers (cf. 1968:426), then both a number of men and some men will have the approximate structure of:

(7.47)



Of the problems which were raised in Chapter 6 about such a structure, that which is most relevant here is the question of subject-verb concord. The facts which we know about concord suggest that the most likely candidates for determining concord are number and some; certainly, men is in quite the wrong position. But number is a singular count noun; although it is somewhat perverse to try and assign a status of countability to some, (47)'s structure demands that this be done, and

the only possible answer is that it is uncountable. Whether it is singular or plural for matters of concord is impossible to answer, and the reason for this, one suspects, is that it is totally the wrong question to ask.

Fortunately this latter point is not too important (as might be predicted), for the behaviour of a number is sufficient to show the inadequacy of (47) for purposes of assigning concord. It was already observed in §6.3 that when a Group I word is involved there is often variable subject-verb concord, i.e., sometimes the verb is singular, sometimes plural. How that is to be explained is not easy to see, but need not worry us unduly at present, for when that word is a number the concord is stable. But it is, as it were, stable the wrong way: only plural concord is found, as is exemplified by:

(7.48) a *A number of men is standing on the
corner

b A number of men are standing on the
corner

Although this is not what would be predicted by looking solely at a number, it is of course predictable from the behaviour of some men, so there is absolutely no reason to suppose the concord to be in some way irregular.

Given that the plural concord of (48b) is quite regular and that (47) combined with our present knowledge of concord predicts otherwise, there are two

possible options, apart of course from the eternal hope that the problem will just go away. Firstly, we could alter the rules for concord when an NP of NP structure is involved. But this change would be completely ad hoc and it offers no generalisations, especially as the majority of such structures do not show such concord. In this context compare, for example, (44a) or:

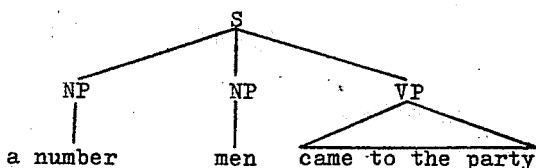
(7.49) A wagonload of potatoes has crashed
into the pub

Secondly, we could alter the underlying structure of the noun phrases in question, a suggestion which has already been broached a couple of times. This is indeed a much more plausible alternative, since it offers a motivated explanation of why a number of men has different syntactic functions and is semantically different from other phrases which have a similar surface structure: a number of men has a rather different underlying structure.

It seems fairly clear that the alterations in the underlying structure will have to perform two tasks: firstly they will have to remove a number from the scope of the NP dominating men; secondly they will have to ensure that the new position of a number is not one which potentially allows it to 'interfere' with the concord relation between men and the predicate. Both requirements lead to one unavoidable conclusion, which is that a number cannot be in the same sentence in underlying structure as men will be. Any reader who is

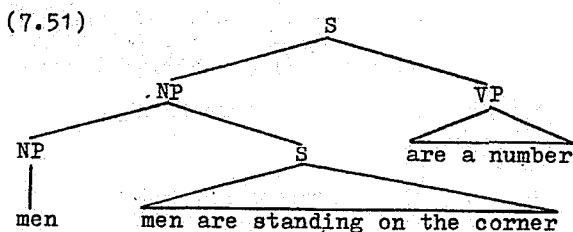
sceptical of this claim may wish to construct for himself structures which would fulfill the above two conditions and yet have the two NP's in the same sentence. The most plausible might be something like:

(7.50)



It may be correct to claim that this satisfies the conditions which are set out above, but the cost is considerable. To name but one point, if we accept (50) then it is very difficult to define the notion of logical subject in structural terms (and the problem is worse if we accept the claim of McCawley (1970) that English has underlying Verb-Subject-Object order). Even if (50) is the most plausible solution, there is still a large credibility gap.

We have already examined, in Chapter 5, one hypothesis which would permit a number to be in a different sentence in underlying structure from that in which it appears at surface structure: this is the hypothesis presented by Lakoff and Carden. If they are correct in claiming that some is an underlying higher predicate, then there can hardly be any objection to assigning the same status to a number. In that case (48b) would have the following underlying structure:



Consider the status of the higher sentence in (51):

(7.52) *Men are a number

Doubtless neither Lakoff nor Carden would wish to claim that (52) is grammatical: 'relative' quantifiers do not appear in predicate position in surface structure; nor do I wish to discuss the meaninglessness of (52), for that is a general property belonging to many underlying sentences within the Lakoff-Carden theory, compare (53a), although (53b) is slightly better:

(7.53) a *Men are some

b ?Men are many

Both the above problems are serious, but we have noted them before, and therefore I merely want to discuss at present the rather trivial matter of number concord in (51). Generally speaking, the subject and complement of the copula be must be identical in number (this is a 'refined' version of an adage well-beloved of primary schoolteachers and which there is no reason to doubt). Thus (54) is ungrammatical:

(7.54) *Whales are a mammal

Admittedly there are some instances where that rule is broken, for example:

(7.55) Men are a swine

(55) seems a not inappropriate remark for a more extreme feminist, but note that it has a meaning rather different from that of:

(7.56) Men are swines

Nor is it possible to get round the objection to (52) by modifying it to look like (56):

(7.57) Men are numbers

That is a perfectly acceptable sentence, even if it is highly restricted and metaphorical, and it has nothing whatsoever to do with (51).

It would therefore seem to be the case, given the above facts, that an underlying structure such as (51) runs into the problem of contradictory number concord. The most appealing solution is to claim that concord is irrelevant at the stage of (51), but it must be dubious whether this does more than sweep the difficulties under the carpet. Nevertheless, I would not wish to claim that the objection outlined here is in any way sufficient to discredit the Lakoff-Carden theory, for then it would have been more appropriate to discuss it in Chapter 5. Rather, given that there are several other, and more serious, objections to that theory, this is a minor point which may help to bring one to a final rejection of Lakoff and Carden's proposals for the underlying source of quantifiers.

Although we have been able to determine fairly conclusively, by means both of the evidence given by Bach and of other facts concerning the distribution of some and a number given above, that some is not in underlying structure in the same NP and probably not even in the same sentence as the noun with which it collocates in surface structure, it is also quite obvious that the current hypotheses which have attempted analyses of the syntax and semantics of quantifiers cannot be justified and thus even if they do meet some of the conditions which an adequate analysis of quantifiers must meet, they have to be rejected. We must therefore examine other aspects of current analyses of noun phrases to ascertain the status of quantifiers. Indeed, we have already done this to some extent, by considering the claims made in Bach (1968), and it is to an extension of his position that I now wish to turn.

In §7.2 we observed that a sentence such as (36), which is repeated here, for convenience:

(7.36) She wants to marry a man with a big
bank account

is ambiguous, and that this ambiguity provides some justification for the claim of Bach (1968) that noun phrases have a source outside of the NP's in which they occur. The kind of source which they might well have, and the type of structures which would necessarily be involved, are discussed in more detail by McCawley

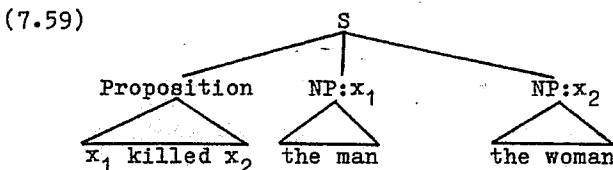
(1971). He concludes (1971:224):

"that it is necessary for semantic representation to separate a clause into a 'proposition' and a set of noun phrases, which provide the material used in identifying the indices of the 'proposition'."

Thus for:

(7.58) The man killed the woman

McCawley suggests the following semantic representation:



The question which immediately presents itself is what justification is there for and what status have the indices \underline{x}_1 , \underline{x}_2 . It is, of course, quite clear what their purpose is: as McCawley (1971:223) states, sentence (58) asserts that \underline{x}_1 participated as agent in a certain event \underline{y} of killing and that \underline{x}_1 is a man.² The first of these assertions is contained under the node

² (58) asserts more than this, as McCawley points out, but that is taken for granted here. More important, however, is whether one assertion is that \underline{x}_1 is a man or that \underline{x}_1 is the man. The latter problem is explicitly ignored by McCawley, yet it seems to me that it is essential for his argument that he investigate it.

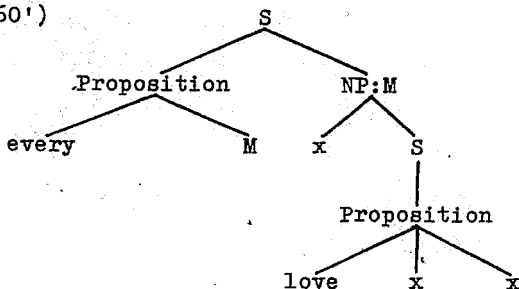
'Proposition' and the second within and under the node 'NP: x_1 '. In other words, x_1 and x_2 are indices which designate the referent or intended referent of the noun phrases which each dominates.

It is not my intention to object here to the goals which McCawley hopes to reach by his introduction of indices, for we have surely seen enough evidence to suggest that they are largely correct. The real question is: is the introduction of indices in the manner of (59) justifiable and have they significant surface correlates? This is an important problem because of the undoubted fact that one of the principal tasks of the derivation process from (59) to (58) will be to delete all indices. In this respect McCawley's indices are remarkably akin to the "disappearing quantifiers" of Carden (1968) discussed in §5.2. Therefore it is hardly surprising that the use of indices is justified on similar grounds. For example, McCawley (1971:229) claims that (60) and (61) ought to have the semantic representations of (60') and (61') respectively:

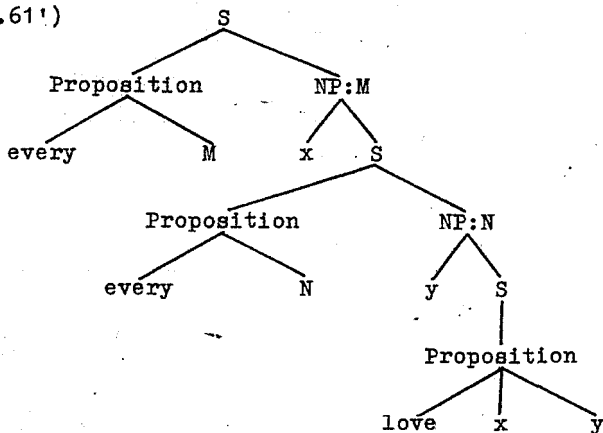
(7.60) Everyone loves himself

(7.61) Everyone loves everyone

(7.60')



(7.61')

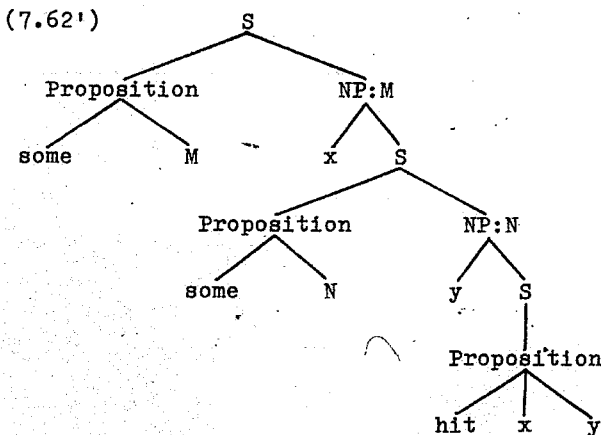


The problem (and McCawley's solution) is only a variant on the problem (and 'solution') of Equi-NP which was discussed in §5.2, and therefore our objections must be the same; the only distinction is that here we are discussing coreference within one underlying sentence, whereas in the case of Equi-NP the coreference is over two sentences. It is clear that the introduction of indices in (60') and (61') is intended to show that there is coreference which leads to reflexivisation in

(60) but not in (61). But this is done at some considerable cost, and not only in terms of the highly complex so-called semantic representations which have to be transformed into relatively simple surface structures. There is also the point that (61') does not clearly show that both instances of everyone refer to the same set of potential referents. But ought not this to be handled by a statement that $\underline{M} = \underline{N}$?³ Such a statement will have to be carefully phrased, in order to avoid false claims about a sentence such as:

(7.62) Some men hit some men

If we follow McCawley this will have the structure:



³ It so happens that in the case of (61) it is also probably true that $\underline{x} = \underline{y}$. At least, this is my understanding of McCawley's highly inexplicit argument. See below for further discussion of this possible equivalence.

Furthermore, although $\underline{M} = \underline{N}$ in (62'), this would not be so for the underlying structure of:

(7.63) Some men hit some women

Yet, in both (61') and (62') the identity statement is necessary, for only from that can the equivalence or non-equivalence of the referents of the surface NP's be assessed. Thus in (61') \underline{x} and \underline{y} , as mentioned in note 3, are equivalent. This is deducible as follows: \underline{x} indicates reference to \underline{M} ; \underline{y} indicates reference to \underline{N} ; every potential referent of \underline{x} is designated; every potential referent of \underline{y} is designated; \underline{M} and \underline{N} are identical. In other words \underline{x} and \underline{y} have equivalent reference due to the properties of universal quantifiers and the identity relation between \underline{M} and \underline{N} . If \underline{M} and \underline{N} were not identical, as in (63), \underline{x} and \underline{y} would not be equivalent. Similarly, in (62') \underline{x} and \underline{y} are not necessarily equivalent due to the replacement of universal quantifiers by existential ones.

Now there are a great many difficulties entailed by the necessity for an identity statement of the order described above. Firstly there is the question of how it is to be handled in the grammar. However I shall assume that McCawley is able to overcome that point, since it appears to me that there is a much less trivial problem involved. Let us assume that in (61') \underline{M} and \underline{N} are identical and can be stated to be so. We have now to consider whether it is possible to substitute salva

veritate. The possibility of such substitution is clear from the remarks of Quine (1960:142):

"When a singular term is used in a sentence purely to specify its object, and the sentence is true of the object, then certainly the sentence will stay true when any other singular term is substituted that designates the same object. Here we may have a criterion for what may be called purely referential position: the position must be subject to the substitutivity of identity."

The positions of M and N are purely referential, in that they are not in opaque contexts (to follow Quine's terminology). But are they singular terms? The question is virtually impossible to answer, because McCawley does not explain what status he would wish to assign them, but if we accept Quine's view that 'mass terms', usually water, etc. but perhaps here M and N, are singular terms when before the 'is' of predication, cf. Quine (1960: 97), then it seems most probable that in (61') M and N can be substituted for one another salva veritate.

Whether or not McCawley is willing to accept such substitution I cannot tell, and I suspect that if substitution is not permitted there will be some mechanism available to block it. In other words, McCawley's proposals are too implicit to be decidable. In the context of the present discussion, especially, it is

worth observing that "M" and "N" are first introduced in the trees (60') and (61') (McCawley, 1971:229) and that there is absolutely no explanation of their status. And so one simple objection to McCawley's proposals is that the manner of their introduction is such that the implications of the analyses cannot be wholly foreseen. Further, some implications which can be deduced, including the example we have discussed above, do not appear to be totally desirable. Yet another objection is that McCawley's analyses are simply too complex. This, of course, is not a decisive counter-argument, but it does seem to be true that much of the obscurity about the above proposals is due to their initial complexity.

Let us consider but one example of this. We have already shown that it is necessary to state that $\underline{M} = \underline{N}$ in the underlying structure or semantic representation of (62), but that in the equivalent structure for (63) it has to be stated that $\underline{M} \neq \underline{N}$. How else would it be possible for the semantic equivalences or distinctions to be captured? The question, I fear, is not terribly profound. The crucial distinction between (62) and (63) is that in the former the nouns in the noun phrases are identical: men and men, whereas in the latter they are not identical: men and women. Now there appears to be a significant generalisation, all the more significant because it is so elementary, which can be made here: it

is that identical lexical items⁴ refer to the same potential set of referents, cf. §4.2. Because of the philosophical problems surrounding the notions of reference and referring, for which see, for example, the collections of articles in Olshewsky (1969:Ch. 4) and Steinberg and Jakobovits (1971:76-154), the linguist must tread very carefully when discussing the referential power of nouns. However, I shall assume, contra Lyons (1968:424-27), that all (common) nouns have potential referential power, given the proviso that "physical existence", which Lyons asserts is "fundamental for the definition of the semantic relationship of reference", has as fully an extended meaning as possible. Further, it ought to be noted that while it is claimed that potential referential power is a necessary property of (common) nouns, it is not claimed that it is a sufficient property; it is undeniable that other semantic elements are involved.

Although it may not be immediately apparent, we have moved some considerable way towards a solution of

⁴ Of course we must be on our guard against homonymy and similar phenomena, nor is it disputed that certain lexical items may have complex semantic structures, cf. the discussion of mother in Anderson (1968) and parent in Hogg (1973b). Reference does not appear to be affected by the latter phenomenon.

some of our difficulties, because of our emphasis on the potential referential properties of nouns. Consider firstly a description of the semantic properties of (64) which makes use of the claim that identical lexical items refer to the same set of potential referents:

(7.64) ~~Ba~~chelor~~s~~ are ~~ba~~chelor~~s~~

Since the two nouns are identical we can claim that each refers to the same set of potential referents; therefore the sentence will be predicted to be tautologous, quite correctly. McCawley's proposals discussed above appear to require an intermediary statement of identity and is thus less simple. We ought now to observe that the referential property is not confined to nouns, but appears to be extendable to noun phrases containing identical items. If it were otherwise it would be impossible to explain the tautology of:

(7.65) Unmarried men are unmarried men

Yet the referential properties observed in (64) and (65) do not extend over the whole noun phrase as it appears in surface structure, as we have noted in discussing (62). More precisely, the presence of a quantifier destroys, or, rather, potentially destroys, the referential equivalence. This of course is not true in the case of universal quantifiers, but we shall examine the reasons for that at a later stage. It is sufficient to note at present that existential quantifiers do have such power. The obvious conclusion to draw from this is

that underlying structures must be so conceived as to account for this fact. And this, once more, suggests that some is outside its collocating noun phrase in underlying structure.

We now have a considerable amount of evidence about the kind of underlying structure which will prove adequate, and this can be formulated in terms of a number of requirements: (i) the structure for some ought not to be radically different from that for a number; (ii) some must be outside the noun phrase to which it belongs in surface structure; (iii) it is more economical to conceive of underlying structure containing lexical items rather than indices, although this last point has to be qualified, for it is no more than a claim that the use to which indices are put in representations such as that offered by McCawley (1971) is uneconomical. It is not a claim that all surface lexical items are derived from identical underlying items, for that can hardly be substantiated. Let us now examine the relative adequacy of some alternative proposals which have some claim to satisfying these requirements and the consequences of each proposal. In all cases we shall assume that some and a number have similar underlying analyses.

7.4 Some alternative structures

In the discussion below we shall present four alternative underlying structures for some, which in

turn make the following claims: (a) some is contained in a relative clause dependent on the collocating noun phrase; (b) some is a noun with a restrictive relative clause containing the collocating NP dependent upon it; (c) some is a noun directly dominating the collocating NP; (d) some is a noun in a higher existential sentence. In each case we shall base our discussion upon an analysis of a sentence containing two some's, namely:

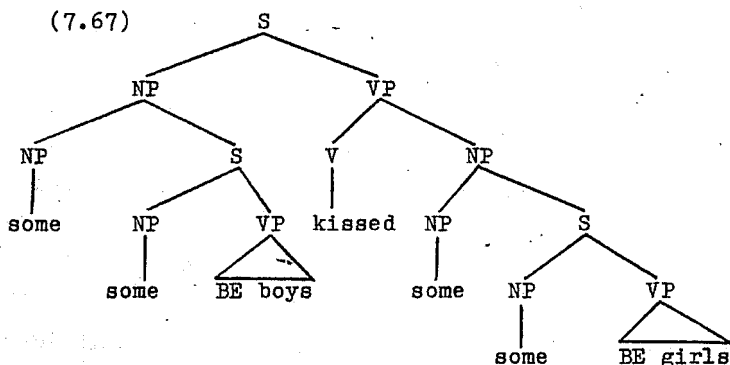
(7.66) Some boys kissed some girls

Although this example might be objected to upon the grounds that it is over-complex, it has the advantage of showing simultaneously the structure of S-dominated and VP-dominated NP's containing some; further, we have to be aware at all times of the problems raised by sentences containing two quantifiers, and thus it is best to keep these problems at the focus of our attention.

Alternative (a) is a slight and obvious modification of the Lakoff-Carden proposals; more precisely, it looks like the intermediate stage through which post-determiner quantifiers must be derived in that theory. As such it suffers from all the inadequacies of those proposals and because it no longer uses the notion of a higher predicate it has even more inadequacies. Therefore it cannot possibly be justified and must be immediately rejected. We can now, therefore, concentrate our attention on the three remaining choices. All of these, it will be observed, have in common the fact that some

will be higher in the underlying tree than the collocating NP - although not necessarily higher than the main or matrix verb of the sentence. Further, each will assign to some a structure not markedly different from that of a noun like a number. Both these points are desirable in the light of the discussion above.

If (b) is to be the preferred choice, then (66) ought to have the following underlying representation (which ignores non-essential or irrelevant points):



Given the already existing transformational apparatus it should not prove difficult to derive the correct surface structure, provided that Adjective Shift is not spuriously applied to boys, girls, etc. to give:

(7.68) *Boys some kissed girls some

The above analysis has a considerable number of advantages. Firstly, it assigns a noun-like status to some, and that is desirable not only on account of that quantifier's close resemblance to a number, but for other

reasons which we shall discuss in Chapter 8. Secondly, it is probably correct that boys (etc.) is in the type of subordinate relation to some which a restrictive relative clause demands. One reason for claiming this is the status of of, which we discussed in Chapter 6. There it was claimed that an Adjacent NP Constraint blocked derivations such as:

(7.69) *A number men

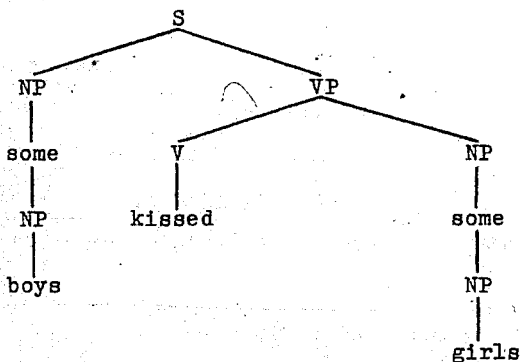
This constraint was typically avoided by the introduction of either and or of, cf. too §7.3, above. The former of these is most probably the marker of a coordination relation whereas the latter is a marker of subordination. If (67) is correct, then we are now able to see exactly what that subordination relationship is.

Another interesting argument in favour of (67) is that it accounts for many of the points which Bach (1968) discussed. Indeed, there are only two differences between Bach's position and the one outlined in (67). The first of these is that indices have been dropped, but we have already pointed out that the use of indices is not necessarily productive. The second difference is one that has been silently introduced, but which must now be stated explicitly. It will be recalled that both Bach (1968) and Bell (1972) allow either restrictive or nonrestrictive relative clauses to be associated with the; on the other hand, we have introduced only a restrictive variant to account for the grammar of some.

In fact this would seem to be consonant with Bach's remarks on the subject (1968:95), but it does mean that there will have to be an ad hoc restriction with existential quantifiers which will block associated nonrestrictive relative clauses. The restriction will be ad hoc because there will be no other distinction between the and some.

In spite of this latter difficulty, it is reasonable to reach the pro tempore conclusion that proposal (b) has considerable appeal and that it should not be dismissed out of hand. Before going further than that, however, we must look at the two remaining proposals and see what merit they have. Any discussion of (c) immediately runs into the problem that it is difficult to tell what the underlying structure should look like. Perhaps the most reasonable suggestion (once again using (66) as the base sentence) would be:

(7.70)



The alternative possibilities all appear to be similar to the analyses proposed in Jackendoff (1968) and are thus open to the same criticisms, criticisms which entail their rejection. The one advantage which (70) has over (67) is that there is no danger that the ungrammatical (68) will be derived. This is so because (70) dispenses with the need for the transformations associated with restrictive relatives. But in gaining this one very slight and possibly illusory advantage it is clear that at the same time the relatively useful generalisations which are made possible by the presence of a restrictive relative in (67) are lost. Furthermore, it is far from certain that it is permissible to generate an underlying structure for (70) in the first place, although perhaps this could be remedied in some type of dependency framework. The relative inadequacy of (70), however, is undeniable, and therefore there seems no reason why we should not reject it at once.

The final alternative which we have to examine raises a number of important issues, primarily revolving around the question of what an 'existential sentence' is. But the following quotation from Quirk et al (1972: 956) seems to me to provide a valuable working definition:

"Existential sentences are principally those beginning with the unstressed word there, and are so called because when unstressed

there is followed by a form of the verb BE, the clause expresses the notion of existence:

There is nothing more healthy than a cold shower

('Nothing more healthy exists than a cold shower')

But whereas the meaning and the outline structure of existential sentences (there - be - NP, with certain permissible variations, especially connected with the verb) is relatively uncontroversial, there is much controversy over the exact status of the elements of structure. The correct analysis of each of the three principal elements is still a subject of debate and therefore we must examine the merits of competing hypotheses about them before we attempt to construct an underlying structure for (66) which involves an existential sentence. In order to do this as briefly as possible I shall ignore the status of be, not because it is easily resolvable, but because it does not seem crucial to the questions at hand.

Let us firstly consider what kind of noun phrase can occur after be. We are at once faced with the tricky question of whether sentences like:

(7.71) There's the oddest-looking man standing at the door

should be considered as straightforward existential sentences. But as Quirk et al (1972:957) reasonably

point out, this type of sentence does not completely parallel what might be judged, from their definition quoted above, to be a 'normal' existential sentence. There appear to be at least three differences. Firstly, sentences like (71) are often grammatical only by virtue of the context of discourse, i.e., as answers to questions; secondly, in other situations the intonation pattern of the putative existential sentence is considerably distinct from the pattern usually associated with undeniably existential sentences; thirdly, and perhaps most importantly, there are quite radical differences in meaning between pairs of sentences such as:

- (7.72) a There's always the cars in the
garage
b There's always some cars in the
garage
- (7.73) a There's the snow on the hills
b There's snow on the hills

As Allan (1971:16) says:

"The function of the existential operator [realised as there be:RMH] is to introduce the referent of the noun-phrase in which it is a constituent as a 'new' theme of discourse."

Only in the (b) sentences above is this possible. Further, we might observe that if we change always to never, only the (b) sentences are acceptable (although

in (72b) some changes to any):

(7.74) a *There's never the cars in the
garage

b There's never any cars in the
garage

(7.75) a *There's never the snow on the
hills

b There's never snow on the hills

It therefore seems correct to exclude sentences of the type (71), (72a), (73a) from our future discussion. This, of course, is in accordance with current grammatical thinking, for most grammarians agree with Quirk et al (1972:956) and Roberts (1964), cf. the discussion in §6.3, that the NP following be must be 'indefinite'. Even if we accept the term 'indefinite' at face value, that claim is only misleadingly true, for although the following NP must indeed be 'indefinite', it is not the case that every 'indefinite' NP can follow an existential verb. Thus compare the following:⁵

⁵ It will occasionally be the case that sentences asterisked below are acceptable in a non-existential interpretation, cf. the discussion above. To keep the argument to a reasonable length, I henceforth ignore such alternative interpretations, unless it is explicitly stated to the contrary.

- (7.76) a There's some prisoners on the roof
b There were many philosophers hoping
to find the meaning of 'truth'
c *There were all prisoners on the roof
d *There was every philosopher hoping
to find the meaning of 'truth'

From this we might conclude that the following NP must contain, in surface structure, an existential quantifier. This, however, appears to be over-rash, in view of the acceptability of sentences such as:

(7.77) There are cricketers who write poetry

But notice that (77) is rather different in meaning from the sentence of which it might be supposed to be a transform:

(7.78) Cricketers write poetry

In (77) it is claimed only that some cricketers write poetry, whereas in (78) the predication is claimed to be true of the class of cricketers as a whole. In other words, in (77) the reference of cricketers is similar to the reference of some cricketers, in (78) to the reference of all cricketers. Our intuitions in this respect are reinforced by the different degrees of grammaticality in the following group of sentences:⁶

⁶ (79b) is only fully acceptable, for me, under certain stylistic conditions, namely 'vivid' use of the present continuous, especially as found in narration.

(7.79) a Some children are playing by the
river bank

b ??Children are playing by the river
bank

c *All children are playing by the
river bank

(7.80) a There are some children playing by
the river bank

b There are children playing by the
river bank

c *There are all children playing by
the river bank

The interesting point is that (79b) and (79c) fall together in (lack of) acceptability but that in (80) it is the (a) and (b) sentences which fall together in (presence of) acceptability. What conclusions can we draw from these facts? It would be premature just now to discuss at length the relation between quantifierless NP's and NP's with a collocating universal quantifier, but we can observe that neither type of NP is fully acceptable as the subject of a sentence which is not capable, by virtue of tense and aspect, of a generic interpretation. This is why (79b) and (79c) are at best marginally acceptable. NP's with a collocating existential quantifier are not subject to this restriction, hence the acceptability of (79a). Now this shows that there are two reasons why (80c) is unacceptable: firstly,

the sentence of which it is a putative transform is unacceptable; secondly, as we have already observed with regard to the sentences of (76), there must be no universal quantifier in the complement NP of an existential sentence. Yet neither of these reasons are applicable in the case of (80b). Given the relationship between quantifier-less NP's and NP's with a universal quantifier, this can only be explained by the hypothesis that there is in fact an underlying quantifier associated with the NP of (80b). This quantifier must be deletable only under strict conditions, otherwise we fall into the trap of "disappearing quantifiers". The syntactic and semantic evidence suggests that this deletable quantifier, which is presumably realised in (80b) as part of the there be construction, must at the very least be relatable to some. In this way we would be able to account for the close meaning relation between (80a) and (80b).

The above argument is of interest not only because it shows that, since there is a distinct meaning difference between (79b) and (80b), those two sentences ought not to be transformationally related; there is also the more significant point that it looks as if (80b), an existential sentence, has some kind of underlying existential quantifier. From that it follows that we can make a much more precise statement about the kind of surface NP which can function as the complement of an existential sentence: that NP must include an underlying

existential quantifier. That there is such an intimate relationship between the acceptability of existential sentences and the presence of an underlying existential quantifier strongly implies that the case for our fourth alternative - that existential quantifiers are derived from an underlying existential sentence - is supported by more than p rely nominalist considerations.

Now let us turn to the status of there. It is undeniable that in many contexts, for instance:

(7.81) There's the boy, -swinging from a
 lamppost

it has a purely locative function. However, Allan (1971) has suggested that in existential sentences there should not be considered as a locative, for as he shows quite convincingly, pace Sampson (1972), cf. too Allan (1972), the two types of there have different distributions and can be distinguished in apparently identical sentences by contrasting stress and intonation patterns. We therefore accept here Allan's designation of there₁ - the existential there - and there₂ - the locative there. The question now resolves itself into what kind of status in underlying structure ought to be assigned to there₁. One possible solution is obviously out of the question: given the differences between there₁ and there₂ it is impossible to derive there₁ directly from an

ordinary locative source.⁷ Allan (1971:11) himself proposes that there₁ and be are together derived from a single source, and it would be plausible to set up as this source an abstract verb such as EXIST, cf. §5.2 and Garden (1968). Although in Chapter 5 we found that that abstract verb had little to recommend it, I shall adopt it here, and this is for two reasons. Firstly, Allan's arguments that there₁be is derived from a unitary source and is only transformationally realised is appealing. Secondly, there is a reasonable case that the underlying subject of an existential sentence is not there₁, but the surface NP complement. If this were not so, it would be difficult, as even Sampson (1972:116-17) admits, to account for the concord of:

(7.82) There are lions in Africa

although we ought to remember that such concord is not obligatory, for compare:

(7.83) There's lions in Africa

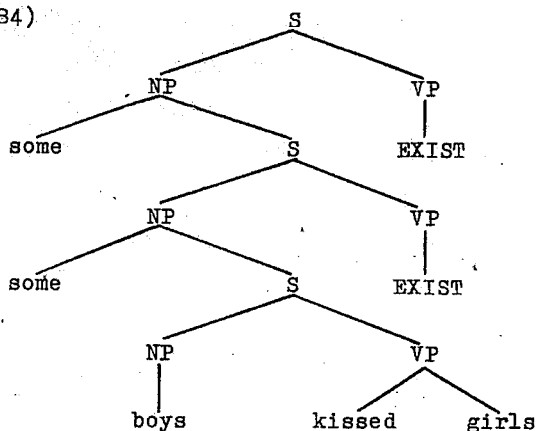
Both the above points suggest that an underlying

⁷ Within the terms of a localist theory, as proposed in Anderson (1971b), it would, of course, be necessary to derive there₁ from a locational source. It is not entirely clear that such a theory would be able to distinguish consistently between the two types of there, which may thus constitute a rather minor objection to that theory.

predicator is the source of there, be and EXIST is the most likely candidate. Nor are all the arguments against the general use of an abstract verb such as EXIST valid here, for, as we shall see, it has specific realisations and is recoverable even if deleted by the time surface structure is reached.

In the preceding paragraphs we have examined a great deal of evidence all of which supports alternative (d), namely that quantifiers are derived from an underlying existential sentence. This mass of evidence, I believe, is quite sufficient to show that this fourth alternative is the only serious competitor to (b), and therefore it is only appropriate that we now attempt to show what the underlying structures corresponding to (d) ought to look like. The most plausible candidate for the underlying representation is one which is clearly a rather simple adaptation of the Lakoff-Carden theory, in which some is a noun, not a predicate, and the abstract verb EXIST is the higher predicate. Again using sentence (66) as the base sentence, the representation corresponding to alternative (d) must be on the lines of:

(7.84)



The close resemblance in basic structure between (84) and the representations proposed by Lakoff and Carden can easily be observed by comparing (84) with the phrase markers in Lakoff (1971c:239), where it is also the case that two quantifiers are involved. However, in those cases, we ought to note, the quantifiers involved are compound existentials; properly speaking the comparison between (84) and the Lakoff-Garden structures only applies to simple existentials.

As observant readers will have realised for themselves, (84) cannot be accepted just as it stands, for it presents a number of difficulties for the generation of correct surface structures. However I want to pass over these for the moment (as they rightly belong to §7.5) and merely note here that it is not easy to see in what other way Allan's observations, discussed above,

can be captured. More precisely, Allan (1971:15-16) suggests that existential sentences are dominated by an NP node, and this runs into insuperable difficulties when the object NP is derived from such a sentence. Thus the following paradigms would not be explicable:

- (7.85) a John saw there were girls in the
park
b John saw (some) girls in the park
- (7.86) a John realised there were girls in
the park
b *John realised (some) girls in the
park

Whatever the difficulties of (84), that is not one of them.

7.5 Conflation

It therefore appears to be fairly certain that the correct choice of underlying representation for (66) lies between (67) and (84) or whatever elaboration and modification of these two structures may be deemed necessary. This certainty stems from the already observed fact that both (67) and (84) are able to account for a number of syntactic and semantic phenomena which are inexplicable in terms of the other structures discussed in §7.4, and, furthermore, neither are they satisfactorily accounted for by a hypothesis, such as that of Lakoff and Carden, which claims that quantifiers are

underlying higher predicates, or by one, such as that of Jackendoff, which, although it claims that some quantifiers (but not some) are underlying nouns, does not derive quantifiers from a sentence higher than the sentence which contains the collocating NP.

But even though we can now show that the kind of decision which we now have to make can be very clearly defined, there are a number of caveats which must be made with respect to the discussion in the rest of this chapter. Most notably, neither (67) nor (84) purports to give an analysis of any quantifier other than some. At first sight this might appear to be unfortunate, and on deeper investigation such fears can be seen to be well-founded. To consider but one point, there is virtually no interaction between processes of element negation and simple existential quantifiers. Thus, whereas not many is an acceptable sequence, *not some is not. In Chapter 8 we shall see that structures such as (67) or (84) provide excellent explanations of this point, but the present disadvantage is that we cannot use evidence from quantifier negation to support either of the above structures. Of course, after we have agreed on an underlying structure for simple existentials on the basis of the present evidence and then moved on to quantifiers such as many, if we then find that the evidence from, say, negation supports the already agreed structures, such independent confirmation will lend

greater strength to our proposals. A second point, but this is more general, is that there is no claim that either of the above structures, or a future modification, is correct on all details. Thus we shall observe in Chapter 12 that the exact status of restrictive relative clauses is uncertain and that this has an effect on our suggestions here. Another instance is that even the status of nodes such as S, NP or VP is open to dispute, as is the even more fundamental question of whether or not a constituency framework is preferable to a dependency framework, cf. Anderson (1971a; 1971b:29-32) and §6.6, above. It would be best to remember here that it has been the notionalists, with their reluctance to attach themselves too closely to one formalism, that have contributed a great part of our knowledge of determiners and quantifiers, and it seems preferable to believe that the correct formal framework, while eminently desirable, cannot ever be a prerequisite to accurate linguistic explanation. Indeed, as we often noted in Part I, formalism all too easily leads to a deadening dogmatism. Bearing in mind these reservations, therefore, we can now move on to the discussion proper.

One definite advantage which is held by both (67) and (84) over other proposals is that in both these analyses some is assigned a noun-like status; therefore it is fairly simple to account for the relation of some to a number. We have already observed that these two

items have many similar syntactic and semantic properties, and we are now able to explain these similarities by claiming that a number is derived from the same source as some; this is perfectly consistent with both (67) and (84). In addition to the examples of similarity which we have already given, cf. §7.3, the discussion of Jackendoff's proposals in Chapter 6, and further examples in Anderson (1973c, forthcoming), we might mention such parallels as:

- (7.87) a Many are the men who like oranges
- b *Some are the men who like oranges
- c *A number are the men who like oranges.
- (7.88) a The many men like oranges
- b *The some men like oranges
- c *The number of men like oranges.

Furthermore, there are several environments which permit a number but not some, for example:

- (7.89) a A large number of men like oranges
- b *Large some men like oranges

But these cases, I would claim, are only explicable in terms of an analysis which derives a number and some from virtually identical sources, paradoxical as this may appear. However, the explanation of (89) properly lies in Chapter 8, and we shall only claim here that the other distributions which we have observed are sufficient to show the near-equivalence of a number and some.

The above correspondences are, indeed, so strong that it is tempting to suggest that some is derived from exactly the same underlying structure as a number. This suggestion needs some modification, however, as can clearly be seen from the following:

(7.90) a Some snow fell yesterday.

b *A number of snow fell yesterday

Yet that is easily explicable: we could claim that some plus countable noun = a number and that some plus mass noun = a quantity:

(7.91) A quantity of snow fell yesterday

Although these proposals strike this writer as very near to the 'truth', I have some hesitation in putting them into practical effect. One reason for this hesitation is that the claim that two sentences, purely stylistic variants apart, can have identical underlying structures, although common, is hard to maintain, cf. the remarks on Neg Transportation in §5.4. In fact it does seem to be the case that a very slight meaning difference, perhaps of the order of that which crops up in Neg Transportation, does exist between some and a number. Another reason is that there are several candidates apart from a number and a quantity which might equally well be used, e.g., a set, an amount. As each of these is a discrete lexical item, each must have a slightly different semantic representation.

Nevertheless, it seems desirable that the correspondences which we have discussed should be reflected in some way in the underlying structures (and in Chapter 8 we shall see that there are several further reasons for doing so), and therefore the following ad hoc decisions will be taken, none of which, it must be stressed, make any implicit claim about some which is not already implicit in (67) or (84). Any resultant claims about some will be stated explicitly below. The most important of these decisions will be that in underlying structure some will be variously represented as A NUMBER or A QUANTITY, the upper case letters being used to distinguish between these abstract underlying strings and the surface lexical items. More specifically, A NUMBER will be used for some in some boys, etc., whereas A QUANTITY will be used for the some in (90a). This enables us to distinguish, although perhaps in an ad hoc manner, between collocations with count and with mass nouns, without having to use the at least equally ad hoc feature [count]. We might wish to distinguish between a number and some by that feature in any case, saying that a number (a quantity) is derived from [A NUMBER, +count] ([A QUANTITY, +count]), whereas some is derived from [A NUMBER/A QUANTITY, -count]. Unfortunately, that is not a particularly appealing suggestion; our present state of knowledge is too inadequate to suggest that as a suitable way to distinguish a number and a quantity from some, and we may have to resort to ad hoc devices.

But as the previous paragraph pointed out, the distinctions may be much more purely semantic, in which respect see the further discussion in §8.1.

We have moved, it would appear, some way from the original objectives of this section, but this is not so, for the above considerations have clear implications for the respective adequacies of (67) and (84), implications which we ought now to discuss. In §6.3 we were able to show that the invariable presence of of between a number and its collocating NP was due to an Adjacent NP Constraint, cf. examples (6.32) and (6.41). Furthermore, we then stated that (p.255):

"It is not relevant at the present time to consider why particular items are inserted between adjacent NP's. It seems quite certain, however, that and and or are markers of coordination. This leaves the way open for of to be the marker of non-coordinating relationships, including, but not exclusively so, subordination."

If we now reconsider our earlier remarks in the context of (67) and (84), it is noticeable that (67) is not only compatible with but also strongly supports those previous statements, for (67) claims that a number (like some) is a noun controlling an underlying restrictive relative clause which contains its collocating NP in surface structure; in other words, (67) states that

there is a subordination relation between the two nouns in a number of boys. Of course, no of occurs after some, but this need not worry us for, as we were able to show in §6.3, the insertion of of is a surface phenomenon and, further, a phenomenon which is by no means universal in its application. We can therefore observe that an underlying analysis of the form of (67) is able to explain a surface feature for which we previously had no consistent explanation. On the other hand, it is not immediately obvious that (84) would predict of-insertion, for it does not appear to be necessarily the case that a number and boys would be in a subordination relation, despite the fact that a number would have a source in a sentence higher than that containing boys. Although the situation here can hardly be described as clear-cut, there does seem to be some evidence that (67) is preferable to (84).

Some further evidence which supports (67) over (84), which we have already mentioned but which nevertheless bears brief repetition, is that (67) is totally consistent with Bach's (1968) claim that nouns are derived from relative clauses which are based upon some predicate nominal structure. (84), as it stands, is not consistent with that claim. On the other hand, (84) is able to give some explanation of existential sentences (although admittedly we have not yet shown exactly how this is done), whereas (67) is unable to do so. In other

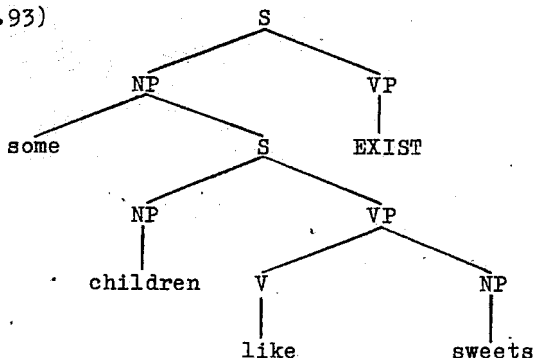
words, we are faced with a situation where two competing structures offer partially overlapping, partially complementary, explanations; luckily it does not appear to be the case that these explanations are contradictory. That this is so prompts the expectation that it might be possible to conflate the two structures and thus bring under one roof (or the shade of one tree) the full range of generalisations which each structure separately affords. The only question, of course, is how this is to be done.

Towards the end of §7.4 it was admitted that (84) had serious deficiencies, although at the time it was left to the reader to discover these for himself. It is now opportune to discuss these matters, and we may start by considering what the underlying structure of:⁸

(7.92) Children like some sweets
might be in terms of (84)'s approach. Presumably we ought to find something like:

⁸ I leave out of discussion the possibly dubious status of (92). Here it is only essential that there be some possible reading of (92); what that reading is precisely is rather less important.

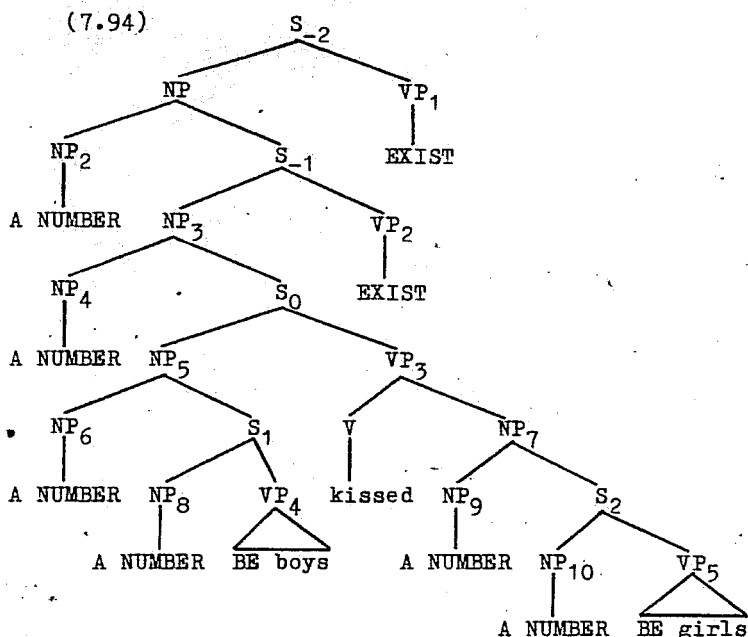
(7.93)



•But (93) is completely unjustifiable, for one very simple reason: it is not possible to ascertain whether some should be lowered onto children or onto sweets. This is a quite basic defect, which is equally observable in (84). The source of this defect is that in (84) there is no type of identity relation holding between the quantifier-noun and the NP into which it is lowered. This clearly causes trouble of the type described in (93), and the only reason why it might not be so obvious in (84) is that the global derivational constraints described by Lakoff (1971c:238-52) condition one into accepting that the higher of two quantifiers is, under normal conditions, the left-most in the surface sentence, cf. §8.4. But we ought not to ignore the simple fact that the lack of identity relations in (84) makes that structure untenable; it is not the global derivational constraints mechanism which attaches quantifiers to the correct NP, it is an identity relation which does that.

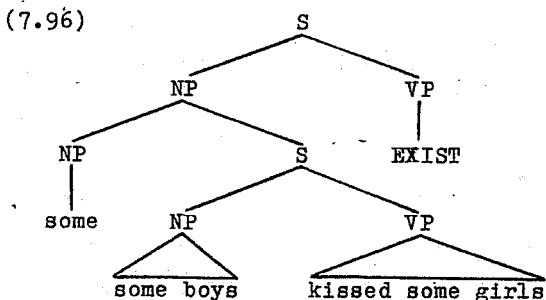
If (84) is correct in asserting that the quantifier must be the subject of a higher existential sentence, then there must be an occurrence of the identical quantifier in the matrix sentence; but (84) does not provide such an occurrence. On the other hand, if we look once more at (67) we can see immediately that exactly such an occurrence is provided by that structure. Now we already know that the basic claims of (67) and (84) are non-contradictory, yet each structure is insufficiently powerful. One way round this would be to build the higher existential sentences of (84) on top of (67), for then the totality of claims of (84) would be added to the claims of (67). What we have so far lacked is some independent motivation for such a strategy. But now the basic structural inadequacy of (84) provides us with such a motivation: what (84) lacks is the occurrence of the appropriate quantifiers in the matrix sentence (the sentence in (94), S_0 , which contains the main verb); what (67) provides is exactly such occurrences. Thus if (84) is to work correctly it must be hooked up with (67) - any further generalisations are by way of a bonus.

The type of structure which we shall end up with (still using (66) as our starting point) is along the lines of:



Despite the fact that at first sight (94) looks rather complicated, this is not really so. Firstly, it can easily be divided into three blocks: (a) S_{-2} and S_{-1} are the higher existential sentences for the two quantifiers; (b) S_0 is the matrix sentence; (c) S_1 and S_2 are the restrictive relative clauses containing the predicate nominal underlying forms of the surface nouns. Secondly, the process of deriving the surface sentence (66) and various related forms is quite simple: let us examine it step by step. Commencing with the lowest sentences, according to the standard cyclical conventions, S_1 and S_2 are removed by the normal transformations associated with relative clauses. This gives us:

shall consider a case where the passive transformation has not operated. It is also probably at this point that lexicalisation of A NUMBER takes place, although this problem will be more fully discussed in the following chapter. In the examples below we lexicalise A NUMBER to some. If no lowering of the appropriate (left-most) quantifier takes place, then we shall obtain:



This gives us - with the correct realisation of EXIST - the structure:

(7.97) [_SThere were some[_Ssome boys kissed some girls]_S]_S

There then follows a rule which is the direct opposite of the existential-lowering rule, which raises the subject of the matrix sentence into the complement of the existential. We call this rule "Subject Existentialisation", and it transforms a string such as (97) into:

(7.98) [_SThere were some boys[_Ssome boys kissed some girls]_S]_S

Then one of two transformations takes place: either the second instance of some boys is identity-deleted, by

normal processes, to give:

(7.99) There were some boys kissed some girls
or wh-formation takes place:

(7.100) [_SThere were some boys [_Swh-boys
kissed some girls] _S]

and the normal relativisation rules follow to generate:

(7.101) There were some boys who kissed some
girls

That (101) is not derived from an underlying structure involving a restrictive relative clause explains why it is only an optional alternative to the wh-less form in (99). Identity-deletion is the optional rule in this context, but the other rules are later and must apply if the structural descriptions are met. Of course, if the derivation from (95) contains the double application of existential-lowering, as it may well do, then we immediately obtain the surface structure of (66).

Although the above certainly constitutes a prima facie case for adopting (94) as the underlying representation of (66), it would be foolish to assert that (94) has definitely been established. Two points need to be made clear. The first is that there are a number of wrinkles in the derivation which have not been ironed out and which must be if (94) is to be shown to be adequate. The second is that we have not yet considered how (94) can be modified to handle quantifiers such as



many. To some extent these questions are interdependent, and therefore we shall discuss both of them in Chapter 8. Only then shall we be able to make a careful evaluation of the hypothesis presented above.

Chapter Eight

Compound existential quantifiers

8.1 Some semantics

By the conclusion of the preceding chapter we had been able to establish a fairly plausible underlying source for the quantifier some, and this source had been justified by both semantic and syntactic arguments. However, there can be no doubt that apart from the initial semantic equation of some with a number, the weight of the semantic arguments was minimal. It is fitting, therefore, that we now have a closer look at the semantics of some, despite the fact that the primary object of study in this chapter is many and, more generally, the whole class of compound existentials, including few, a few, little, a little and much. There are at least two good reasons for this approach. The first of these is that the semantic status of some is interesting in itself; an adequate analysis of some cannot escape talking about the many semantic problems involved. The second is that it is easily predictable that if we get the semantics of some wrong then we are going to get the semantics of many, etc. wrong too. And so we must examine further the semantic credentials of the simple existential.

In the above paragraph and also in Chapter 7, especially §7.5, we have, sometimes hesitantly, talked about the equation of some and a number. Even if we restrict ourselves to collocations with countable NP's, which does not seriously distort the issue, it can quite easily be shown that this hesitancy is undoubtedly justified. We need only consider the following:

- (8.1) a For a number of reasons this
approach has to be rejected
- b ?*For some reasons this approach has
to be rejected

It is only under exceptional circumstances, and even then some would have to be heavily stressed, that (1b) would be taken as at all acceptable. Yet if a number and some are derived from the same underlying source, it ought to be the case that both the sentences of (1) are equally acceptable. Is there any way to account for this contradiction without radically altering our hypothesis? To find an answer to this question we shall have to look more closely at the reasons for accepting (1a) and rejecting (1b).

Unfortunately there seems to be absolutely no reason for supposing that there is any syntactic misbehaviour in (1b), as the following similar constructions are grammatical:

- (8.2) a Because of a number of faults in the design, the supersonic plane cannot get off the ground
- b Because of some faults in the design, the supersonic plane cannot get off the ground
- c For a number of people Marxism signposts the road to Utopia
- d For some people Marxism signposts the road to Utopia

This means that we must rely completely on our intuitions about the meaning of some, and our intuitions are notoriously hazy and imprecise. Nevertheless, some headway can be made. We can start by observing that (1b) is least unacceptable when some has heavy stress but that this heavy stress is not the result of a derivational constraint but of the fact that the some which is roughly equivalent to certain is being used, cf.:

- (8.3) ?For certain reasons this approach has to be rejected

This suggests that one reason why (1b) is unacceptable normally is that it is too vague: to reject an approach one needs (at least morally) specific reasons, and some reasons is too unspecific; yet a number of reasons is not. One explanation for this offers itself immediately: it is that the latter phrase indicates that it is possible for the speaker/writer to enumerate the reasons; the former phrase, on the other hand, gives no such

indication. And if it is possible for the speaker to enumerate the reasons, then the reasons must be specific, although unspecified.

Therefore it appears to be quite plausible to suggest that the contrast between some and a number should not be formulated as it was in §7.5, but rather by a structure in which the former is derived from underlying [A NUMBER, -specific], the latter from [A NUMBER, +specific], with appropriate modifications for collocations with mass nouns. It would be nice if the [+specific] contrast could be connected with the [+count] contrast hypothesised in §7.5, but it is most probable that such a connection would be spurious and indeed it seems likely that the latter ought to be discarded. Furthermore, we have not been able to formalise an explanation of the unacceptability of (1b); but in the light of the other examples given it might be claimed that the steps we have taken are not in contradiction of any eventual analysis of that sentence and that that analysis might be based on a combination of what we have said together with a complex analysis of reasons, which noun might be said to be at the heart of the trouble.¹

¹ The answer may be that reasons is also too non-specific, and that the combination [-specific] + [-specific] is, as it were too much. In all the other cases either the quantifier or the head noun is [+specific].

The most relevant point as far as we are concerned, however, is that (1b), despite exemplifying crucial differences between a number and some, by no means shows that it is necessary to provide quite different underlying sources for the two items, and thus it is not a counter-example to the structures suggested in (7.94).

The reader may recall that in §4.1 it was stated that the prime function of some in a sentence such as:

(8.4) Some children like cream

is to indicate that the reference of the subject NP is to at least two but no more than $n-2$ members of the full set of potential referents of children, where that set has n members. This statement now needs refining in two directions, the first of which concerns the number of referents indicated. Although I believe the above claim is correct, and indeed necessary, as we shall see later, as a theoretical statement about competence, it is certainly incorrect as a claim about performance utterances of some. What is wrong is that the assertion is much too wide-ranging. Thus, if n is a sufficiently large number it would seem to be the case that if only two members of the set satisfied the predication then some would be being used inappropriately, although theoretically correctly. We can handle this and yet save the structure of the claim by introducing m , where m is some small number greatly less than half of n . If we replace all the instances of two with m , then we

obtain a more accurate representation of the actual usage of some.

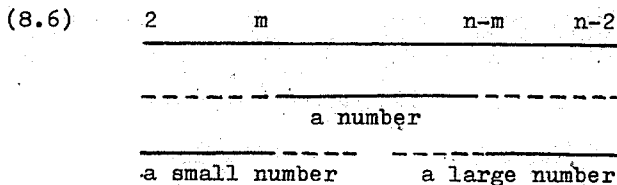
The substitution of m for two is both an advantage and a disadvantage. The advantage is that m is a rather amorphous concept but that, as we have seen, the performance meanings of some have precisely that amorphous characteristic. The disadvantage is that m is so amorphous that it is in danger of failing to explain anything. To solve this problem let us now return to the purely nominal quantifier a number. If we substitute a number of for some in (4) then, as we would expect, the range of size of the set of indicated referents is the same as that for some.² Now notice that there is a method by which we can alter that range, namely by adding either the adjective large or the adjective small:

- (8.5) a A number of children like cream
b A large number of children like cream
c A small number of children like cream

In the former case the upper extreme of the range of sizes is appropriated, in the latter the lower extreme. We can even say that (5b) indicates a set whose size is

² There may be a tendency to use a number over a range of slightly smaller sets than some. If this is so, it can most probably be ascribed to the claims about enumerability which seem to be made by a number, cf. above.

between $\underline{n-m}$ and $\underline{n-2}$; (5c) indicates a size of set between 2 and m . This can be represented graphically:



A solid line indicates that the relevant phrase appropriately indicates reference to the size of set to which it corresponds. Where there is no line no reference may be indicated: thus, a small number cannot refer at all to a set within the range of size $\underline{n-m} < \underline{n-2}$. But it is the dotted line which is most interesting. for this is intended to show that, for the given size of set, although the relevant phrase may be used, it is not the most appropriate. The phrase we are most concerned with at the moment, of course, is a number, and (6) shows, rightly I believe, that although that phrase can be used to indicate that reference is to any size of set between 2 and $\underline{n-2}$, its appropriate range is between \underline{m} and $\underline{n-m}$. Outside that range it is preferable to qualify number with an adjective.

The above remarks are, of course, hardly original; indeed they are quite commonplace. Nevertheless they are worth emphasising for the results which, if it is carried further, this line of reasoning can obtain. We have seen that the appropriate range of a number is

quite naturally limited by the fact that it can be modified by the adjectives large and small. Now we have already observed that the appropriate range of some is the same as that of a number. This could be accounted for simply by pointing out that since the two items have roughly the same underlying source this semantic fact is exactly what one might expect. But that scarcely explains anything. A much more adequate explanation would involve a hypothesis that just as unmodified a number is, crudely speaking, the source of some, so adjectivally-modified a number is the source of some other quantifier(s) which restrict(s) the appropriateness of some in exactly the same way as the appropriateness of a number may be restricted. We shall see below that this hypothesis can be fully justified.

We have now dealt with our first refinement of the statement about some which was made in Chapter 4 and so may proceed to the second. Unlike the first, this looks back to (7.94) rather than forward to the discussion of compound existentials. Consider an interesting distinction between (4) and:

(8.7) Children like cream

This latter sentence asserts that it is a property of each member of the potential set of referents of children

that he or she likes cream.³ As such it can be truly generic and timeless and therefore it is meaningful even if at the actual moment of utterance there is no object which satisfies the reference of children; there need only be some such objects at some time or another. This is what is meant by the term "potential referent". In contrast, (4) seems only to be meaningful if at the time of the assertion being made there are actual objects which satisfy the reference of children. This change in meaningfulness conditions can only be due to some as there is no other distinction between (4) and (7). Therefore we shall have to modify our original statement further so that it reads: appropriate usage of some indicates that the reference of its collocating NP is to at least m but no more than n-m actually existing members of the full potential set³ of referents of that collocating NP, where n is the number of members of the full potential set of referents and m is some small number greatly less than half of n but more than two.

To the reader of this study the above statement may seem slightly reminiscent of one made much earlier, and

³ Some modification of this is necessary: perhaps it would be better to say: "that there is a tendency for it to be a property of ...". We shall return to a discussion of this point in Chapter 9, since it is not relevant to the present discussion.

it is indeed the case that such suspicions are justified. Let me repeat a quotation from Guillaume (1919: 305), first given in §2.2:

"On passe ... d'un plan où les noms existent virtuellement à un plan où ils se réalisent effectivement. Dénoter les cas généraux de cette transition constitue le rôle de l'article, simple signe de relation entre une idée et un fonds d'idées."

This statement is not very different from our own attempt above to suggest that some 'actualises' potential referents. One contradiction between Guillaume's remarks and my own can be quite easily resolved. Guillaume claims that 'actualisation' or "concrétisation", cf. Hjelmslev (1928) and §2.2, is a property of the 'articles' only, but we are claiming that it is also true of some, at least. But we have already shown quite conclusively that Guillaume was mistaken in believing there to be a separate category 'article' and therefore there is no a priori reason why the claim cannot be extended to some.

There is another point, however, which is perhaps more serious. When we first discussed Guillaume's theories we were forced to conclude that such statements as that quoted above were at best obscure, at worst meaningless. One fundamental reason for reaching this conclusion was that it was extremely difficult to see

how his claims could be placed within a formal and testable hypothesis. Therefore if we wish to save any part of the above statement, and it would seem preferable that we do in fact do so, we shall have to overcome that difficulty. In order to achieve this, let us now return to (7.94) and consider part of the function of the higher existential sentence. Apart from its role in deriving structures like (7.99) and (7.101) where there is a surface existential predicate, it is clear that it makes a certain semantic assertion. Not only does (7.94) state that a number of boys kissed a number of girls, but, by virtue of the two top-most VP's, it states that those boys and those girls actually existed. Now this is precisely what we have claimed to be one of the properties of some and it seems also to be what Guillaume regards as the function of the 'articles'. And because the EXIST predicates are present only because the quantifier some/a number is there, (7.94) formalises the claim of Guillaume and our own intuitions exactly.

We can therefore see that the hypothesis of a higher underlying predicate containing the abstract verb EXIST is not only justified by the syntactic evidence given in §7.5, but also by the semantic intuitions which were initially outlined, in an altogether too vague fashion, by Guillaume (1919) and which we noted that it was necessary to formalise in order to account adequately

for the semantics of some. In this section as a whole we have been able to observe that there is a substantial amount of quite varied evidence to support (7.94), and thus it is now possible to move on with confidence to a discussion of the status of many and the other compound existentials within such a hypothesis.

8.2 The adjectival status of 'many'

Garden (1970c) lists fifteen constructions which, he claims, prove that postdeterminer quantifiers, in opposition to predeterminer quantifiers, behave syntactically and semantically like 'true' nonrestrictive adjectives. He states (1970c:423):

"Examples I through XII show that Post-Determiner Q act like true adjectives, suggesting that Post-Determiner Q and adjectives share a deep structure. In this section we consider whether the appropriate deep structure is that of restrictive or of nonrestrictive adjectives. Examples XIII and XIV show that Post-Determiner Q, like nonrestrictive adjectives, are derived from the predicates of deep-structure nonrestrictive relative clauses. Example XV confirms this analysis by showing that Overt-Predicate Q never appear in restrictive relative clauses, thus explaining why Post-Determiner Q are always

nonrestrictive."

We can accept completely Carden's argument up to the point where he show that postdeterminer quantifiers are derived from a source which is at least similar to that of nonrestrictive adjectives, but no further than that point. Beyond there we encounter a number of objections to his thesis. Some of these were discussed in §5.3 and need not be repeated here, but there are others which we must now spell out in some detail. Basically our objections centre upon the fact that Carden does not even begin to attempt to explain why many and the other compound existentials can appear in postdeterminer position, whereas neither some nor all can. As we observed earlier, he does give a formal mechanism which will ensure that only the desired surface structures will be generated, but that mechanism should not be mistaken for an explanation. Given that postdeterminer quantifiers behave similarly to nonrestrictive adjectives, it would surely be most satisfactory if it could be shown that exactly those quantifiers which can appear in that context are always derived from a deep structure

structure involving a nonrestrictive relative clause.⁴

It will be noticed that such a hypothesis would differ from Carden's in two essential points. The first is that since we have already been able to show that some, at least, is not derived from such an underlying structure, then, if we are successful in our attempt, quantifiers will be derived from at least two sources, whereas Carden derives all quantifiers from one predicate structure type. Carden might therefore object that we have lost a generalisation which his proposal is able to establish. But there is an adequate reply to this objection available. It is that Carden's generalisation forces quantifiers into a wholly inappropriate straight-jacket; it claims that all quantifiers have the same underlying structure (although in the case of postdeter-

⁴ It should be noted that a very similar conclusion to this is reached by Anderson (forthcoming), and his arguments for such a source for compound existentials are parallel to our own. Further, the semantic elements of which his proposed underlying structure is composed are partially identical to those suggested in the course of this chapter. However, his structures are quite different in that his work is based upon a theory of case grammar, as found in Anderson (1971b). A comparison of Anderson's proposals with those presented here would be of some theoretical interest.

miner quantifiers originating from a different point in the underlying phrase marker). Yet, as we have observed in §7.1, there are at least three different distribution patterns associated with quantifiers. It makes rather more sense to account for this in terms of different underlying structures rather than by means of dubious concepts such as obligatory versus optional quantifier-lowering, even if the purported generalisation is lost in the process. However this is not to deny that it would be most satisfactory if it were possible to make some generalisation; our point is merely that the one which Carden wishes to make is too sweeping and ignores quite clear distinctions between various quantifiers.

The second difference is that our hypothetical underlying structure only partially involves a non-restrictive relative clause in the derivation of the quantifier, whereas for Carden a quantifier in post-determiner position is wholly derived from the predicate of a nonrestrictive clause. In other words, we are suggesting that the underlying structure of such a quantifier is rather more complex than that which Carden proposes. It is worth repeating, in view of the complexity of many underlying structures which have been suggested in the literature, that this is not an advantage for our proposal. What we have to prove, therefore, is that such complexity is necessary. In this paragraph I only want to discuss one set of facts which helps to do

this, but we shall examine more below. In §7.5 we were able to conclude that some is derived from an underlying NP in a higher existential sentence, but if we now accept Carden's claims about postdeterminer many we shall have to conclude that it is derived from an underlying VP or predicate. The two positions are not a contradiction, but they get very near to being so, for if they are accepted the result will be that it will be impossible to make any generalisation of the kind hoped for in the previous paragraph. However, this will not necessarily be so if the underlying structure of many partially involves a nonrestrictive relative clause, for it is quite easy to see that such a clause could be associated with the kind of NP which underlies some, even if the precise structure is at present obscure. It may therefore be concluded that this second difference between Carden's hypothesis and our own putative hypothesis shows that it is only if we accept his proposals that the possibility of an interesting generalisation is lost.

Nevertheless, before we can state with confidence that many, and the other compound existentials, are always derived from an underlying structure which involves a nonrestrictive clause, it is necessary to show that in positions where many is not a postdeterminer it still has several of the properties which would be predicted by such a derivation, namely that it has

certain adjectival properties. Furthermore, these properties ought not to be shared with some. Before we move on to give such evidence it should be noted that we are not attempting to prove that Carden (1970c) is wrong in claiming that predeterminer many is not a 'true' adjective; we have already suggested that many is never a 'true' adjective, although, for reasons which we hope to clarify below, it does approach that status in post-determiner contexts. We shall consider four points which demonstrate that many always has adjectival properties, three of these being syntactic and the final one semantic.

Firstly, it is well-known (and, indeed, we have mentioned this point several times previously) that only the compound existentials can appear as 'overt predicates', albeit that even when they do they often have a dubious (what Lakoff and Carden call 'archaic') ring to them. Thus we obtain the following pattern:

- (8.8) a The men are many
- b The problems are few
- c *The accidents are all
- d *The houses are some

In this respect the compound existentials follow a pattern which is undeniably to be associated with adjectives and only with adjectives. Compare with (8):

- (8.9) a The men are brave
- b The problems are difficult

c The accidents are serious

d The houses are ugly

It is even the case that many and few (and, indeed, the other compound existentials) may be found coordinated with an adjective in predicate position:

(8.10) a The problems are few but difficult

b The accidents are many and serious

The above examples provide good evidence that many and few must be considered to be adjectival, but the rather awkward status of (8a) and (8b) suggests that these compound existentials are not 'true' adjectives as are the examples in (9). Of course that is exactly what we might hope for.

The second piece of evidence concerns a difference between many and some, but not all, cf. Chapter 9 for further discussion, which is that many but not some may be directly preceded by a negator:

(8.11) a Not many people came to the party

b *Not some people came to the party

This difference can be accounted for in two ways: either many, but not some, is a main verb, which is a position we have already rejected, or many involves a nonrestrictive relative clause which is not found with some. Carden (1970c:418-19) also observes this phenomenon, and he concludes that the second option is impossible, because of the fact that an overt negative cannot modify a true prenominal adjective. Thus:

(8.12) *Not happy inmates escaped

However this position appears to be surmountable, especially because, as we have already pointed out, we are not trying to claim that many is a true prenominal adjective. If many is in fact a compound of an element rather like that from which some is derived together with a nonrestrictive relative, then it seems quite plausible that an overt negative such as in (11a) will be grammatical in contrast to that in (12). The factor which rules out (12) can be explained as a constraint against an overt negative appearing in the same NP as a noun at shallow structure. If we look at the derivational history of some and guess, for the moment, that many's derivational history is similar, then we can see that even in the case of not many this constraint would not be violated. But the constraint must be violated in the case of post-determiner many, as Carden (1970c:419) exemplifies with:

(8.13) *The not many inmates escaped

It will be necessary to show how this is accounted for within the kind of hypothesis we have tentatively proposed.

It cannot be denied that the second argument is rather weak and hypothetical, but it can be fairly claimed that once an underlying structure and consequent derivation has been given for many then its strength will become more apparent. In the meantime we can console ourselves with the fact that the third point to

be discussed is a very strong one indeed. It is an elementary fact of English grammar that only two 'parts of speech' have comparative and superlative forms, namely adjectives and adverbs. Since no one would wish to deny that quantifiers modify nouns (or NP's) rather than verbs, it must be conceded that if such quantifiers were to have comparative and superlative forms, then they would have to be related to adjectives. And, of course, it is the case that they have such forms, cf.:

- (8.14) a Many Scotsmen wore kilts
b More Scotsmen wore kilts (than didn't)
c Most Scotsmen wore kilts
- (8.15) a Which team lost few games?
b Which team lost fewer games (than Celtic)?
c Which team lost fewest games?

Not only that, but no other quantifiers have such forms:

- (8.16) a *Someest houses have central heating
b *Aller cows remain outside during winter (than are brought indoors)

In the light of such examples it is quite clear that we shall have to postulate a source similar to that for adjectives as being involved in the derivation of compound existential, and only compound existential, quantifiers.

The final argument is rather different from the other three, but not only because, as has been previously stated, it is a semantic one; it is also the case that its purpose is more obviously to lead the way to an adequate underlying structure than to characterise in any way the surface features of many. In Chapter 7 we were able to observe that there was an extremely close relationship between some and a number, and this relationship was further defined and supported in §8.1. Even more interestingly, we were able to argue plausibly that given the semantics of some and a number, there ought to be a quantifier corresponding to adjectivally-modified a large number, in order to explain the restricted appropriateness of some. From what we have said in this section it is clear that many is partially derived from an underlying adjective. Therefore it is not too difficult to predict what is in fact the case, namely that many and a large number are in precisely the same kind of semantic relationship as are some and a number:

- (8.17) a A large number of workers went on
strike
b Many workers went on strike

And even more predictably, given (17), we find:

- (8.18) a A small number of presidential
aides will have to be dismissed
b A few presidential aides will have
to be dismissed

Just as it was observed that a number and some were not synonymous, but perhaps differed by only one feature, so it is possible to see that the (a) and (b) sentences of (17) and (18) are not synonymous. Indeed I would wish to claim that the semantic difference between a large number and many or a small number and a few is exactly the same as that between a number and some. In support of that claim we might note that (18b) is perhaps a slightly more natural sentence than (18a), although the latter, of course, is perfectly grammatical. Slight as the difference between the two sentences is, it is explicable. It will be recalled that in the previous section it was discovered that a number indicates - that, perhaps, is to put the matter too strongly - that the set of actual referents is now enumerable, whereas some is much less specific about the size of the set. Moving to (18a) we can observe the implication that the set of to-be-dismissed aides is both small and enumerable, whereas in (18b) it is small but not (yet) enumerable. If the set is both small and enumerable, why not use a cardinal number or perhaps an approximative numeral such as a dozen in place of a small number? This objection cannot be made of (18b), and I would suggest that this is why we may find the latter sentence a 'better' one than the former. The above may be a very minor point, but it does help to support the case that given the derivation proposed for some in §7.5 there is every reason to suppose that many will be derivable from

an underlying structure similar to that for a large number. Furthermore, with the variations in surface structure which such an analysis ought to be able to explain, similar underlying structures should be discoverable for the other compound existentials.

8.3 More underlying structures

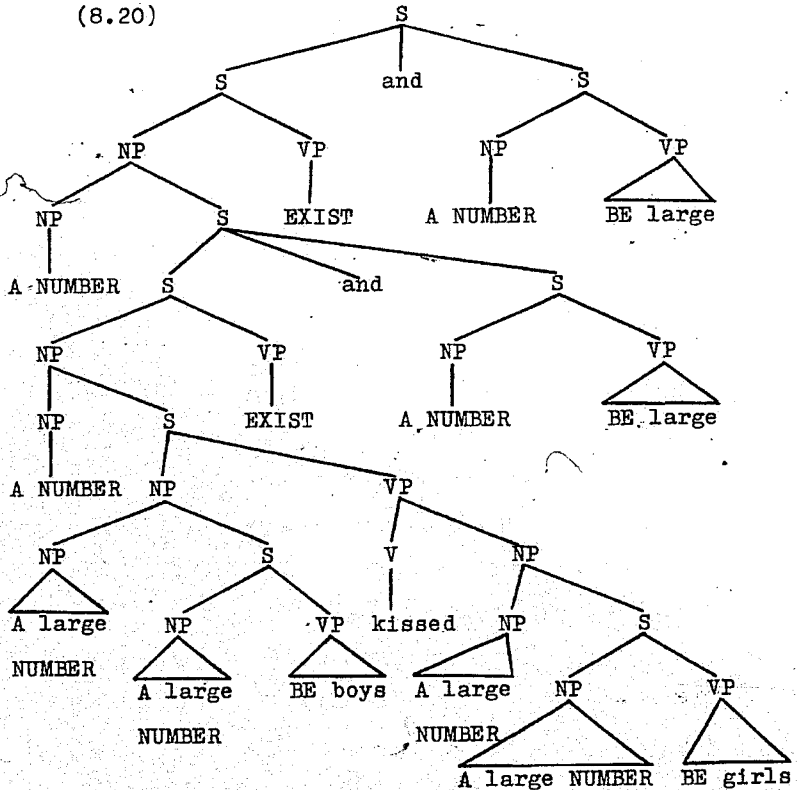
With the above evidence in mind it is fairly simple to deduce what the underlying structure of many ought to look like. It is undeniable that many must have, in all its occurrences, a derivation involving a nonrestrictive relative clause, for otherwise we would not be able to explain its adjectival behaviour in every position. But, with the possible exception of postdeterminer contexts, many does not behave like a 'true' adjective. Therefore it cannot simply be a nonrestrictive adjective modifying the collocating NP in surface structure. If many is derived from a structure resembling a large number where large is nonrestrictive, not only does the consequent derivation satisfy all these demands but it has two additional advantages. Firstly, the underlying structure will be similar to some, which permits us to make the generalisation that all existential quantifiers are derived from a higher sentence of the form A NUMBER EXIST or some modification of that sentence. That is precisely the kind of generalisation we have to make in order to counter Carden's possible objection that we

have lost a generalisation. Secondly, by demonstrating that many is an adjectivally-modified form of some we have also explained why some has the restricted semantic appropriateness which was discussed in §8.1.

Therefore we can state with a fair amount of confidence that (20) is a plausible candidate for the underlying structure of (19), which, of course, is an obvious modification of (7.66):

(8.19) Many boys kissed many girls

(8.20)



The derivational process to reach (19) will be the same as that for (7.94) to (7.66) except that here, additionally, there will be conjunction reduction of the coordinated sentences to derive, in the normal fashion, the nonrestrictive adjectives. This has the desirable consequence that we shall be able to generate (21) and (22), parallel sentences to (7.99) and (7.101):

(8.21) There were many boys kissed many girls

(8.22) There were many boys who kissed many
girls

There are, however, two distributional patterns of many which are not found with some and which we have to show are explicable in terms consistent with (20) before we can assert that that structure is indeed correct. These are that many is directly negatable and that many collocates in a variety of ways with 'definite' NP's. As the former is a rather complex issue we shall leave it aside for the moment and discuss only the second point. But even on this second point we have to make one reservation, namely that as we have not yet suggested what the underlying structure for 'definite' NP's might be, cf. Chapter 12, much of the argument below will have to be founded upon an unproven assumption concerning that structure. However the assumption seems plausible enough, I would maintain, to ensure that no great harm is done to the validity of the following argument.

The first type of collocation with a 'definite' NP which I wish to examine is exemplified by:

- (8.23) The boys that kiss girls are many
(in number)

A variation of this seems to be:

(8.24) Many are the boys that kiss girls
Although it may be objected that (23) is of dubious acceptability, it nevertheless must be discussed, especially as Lakoff (1971c:238) has claimed that (24) is synonymous with:

- (8.25) Many boys kiss girls

While it may be correct to claim that (23) - (25) are always assigned the same truth values under the same conditions (at least, we shall make no attempt to prove otherwise), it seems unfortunate if we must then extend the claim to complete synonymy.⁵ One objection to the claim that (23) and (24) are synonymous with (25) is the presence of a 'definite' NP in the first two examples versus the absence of any such NP in the last example.

⁵ With this point we once more return to the old chestnut of whether synonymy ought to be defined in terms of truth values and the related tools of logic, cf. §5.5. That generative semanticists have often had too restricted a view of semantics is one of the arguments advanced in this thesis; it is also to be found in the works of many other linguists, see especially Bolinger (1971).

It is not at all clear that Lakoff's account is able to explain this. What is worse is that if in (24), for example, we make boys 'indefinite', then we obtain:

(8.26) ??Many are boys that kiss girls

It is doubtful that (26) is acceptable, but with marked intonation it may be made so. But the interpretation of (26) is then something like:

(8.27) Many of the people that kiss girls
are boys

Again, it is difficult to see how Lakoff can account for this.

To view the question of alleged synonymy from another angle, consider what the information content of (23) or (24) is: both sentences give as their major point of information the size of the set of boys who kiss girls. But in (25) the major point of information is that a large set of boys kiss girls. It may be possible, as Lakoff (1971c:260-63) claims, to handle matters such as topic, focus and comment by means of a global constraint, but it is undeniable that if such a constraint is used to explain the above examples, then they are going to change meaning, albeit in a rather subtle way which is not always recognised or accepted by generative semanticists with a logical bias, cf. again note 5. It would be preferable if, instead, we could account for the differences between (23) and (24) on the one hand and (25) on the other by means of some

difference in underlying structure. A first attempt at this, making use only of the fact that many is derivable from a large number, gives us an underlying structure corresponding to:

(8.28) [_S[_SThe boys[_Sboys kiss girls]are
A NUMBER]and[_SA NUMBER BE large]]

But this can be rejected immediately since by adjectivalisation of the nonrestrictive clause we shall obtain the ungrammatical:

(8.29) *The boys that kiss girls are a large
number

We might attempt to save (28) by suggesting that in such cases a large number is obligatorily lexicalised to many, but this is an ad hoc solution which omits to take account of the fact that such lexicalisation is not obligatory elsewhere.

Now let us recall the observation made in §7.3 that the number can occur only with quantity-referring predicates, cf. (7.44a). Parallel to such a sentence we also find:

(7.44a) The number of men who came to the
party was five

(8.30) The number of boys that kiss girls
is large

Bearing in mind the restrictions which we have already had cause to note, (30) can be considered as a reasonable paraphrase of (24). We may thus conclude that the

underlying structures for the two sentences are virtually identical, and continue from there to see whether or not it is possible to derive (24) from such a structure. If we ignore the restrictive relative clause for one moment, we can see that (20) is not a suitable underlying structure for the matrix (31), for two reasons:

(8.31) The number of boys is large

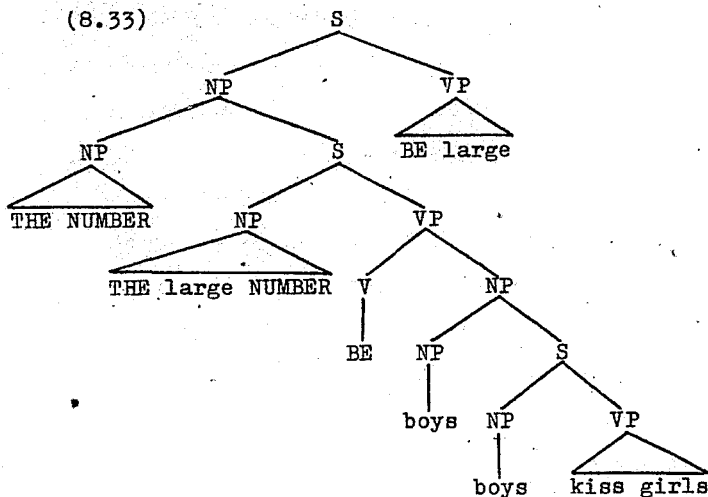
The first is that there is no related existential sentence such as:

(8.32) *There is the number of boys (is) large

The second is that the concord in (31) is between singular number and the verb, not plural boys and the verb.

Both these facts suggest that there is no justification for postulating a higher existential sentence in the underlying structure of (8.30). We shall, nevertheless, want to retain the pred cate nominal source of boys, for that hypothesis has already been seen to be useful.

There will thus be some departure from the surface structure of (30). Ignoring the exact status of girls, which would only irrelevantly complicate the present issues, we can suggest the following underlying structure for (23), (24) and (30):



By the derivational processes which we have already observed to be necessary in connection with (7.94) and (20), but this time applied to THE NUMBER, we obtain (30). The stage immediately before that sentence, when lexicalisation has not taken place, will be:

(8.35) THE NUMBER boys that kiss girls BE
large

There must be provision at this stage for lexicalisation to many, and thus NUMBER and large must be brought together. Obviously there are two options. The first is to move large:

(8.36) *The large NUMBER boys that kiss
girls BE

That is ungrammatical even if we lexicalise to many, nor is it saved by deletion of BE:

(8.37) *The many boys that kiss girls

Let us therefore see what happens if we shift NUMBER, the second option open to us:

(8.38) The boys that kiss girls BE large
NUMBER

Notice that two very strange things happen here. Firstly, it now appears as if the is attached to boys; secondly, boys becomes the subject NP of BE. The first can perhaps be justified on the grounds that the seems to be obligatory in constructions such as (23), see too (26) and (27). If it were not the case that the derivation was from THE NUMBER this could hardly be explained. There is also the fact that otherwise one the would have just disappeared and another one mysteriously taken its place. The second can only be justified on the evidence of subject - verb concord. It has to be admitted that we are on slippery ground here, and this is perhaps even truer if we do not lexicalise to many in (38), for then we obtain:

(8.39) The boys that kiss girls are large
in number

That, of course, is an eminently desirable result, but I am at a loss as how to explain the appearance of the preposition. The situation is eased only if we do not lexicalise to many, for then we generate (23), but without the parenthesised addition. The latter could be permitted by optional retention of number, and this has one advantage. It is simple to see that in number is

is redundant in (23) and we are now able to explain that this is because NUMBER is already contained in many and that its retention is mere repetition. Further, (24) is derivable by a transformation which, unusually, permutes the subject and complement, but as Plötz (1972:148), among others, has shown, the rule is necessary even if it is of restricted applicability. In the present case it appears to be the case that lexicalisation to many is at least preferable, if not necessary.

In the preceding paragraphs we have been able to explain, if only partially, one type of collocation between many and 'definite' NP's, but we still have to provide an explanation of a second and more common type, that is the occurrence of many in postdeterminer position, as in:

(8.40) The many boys kiss girls

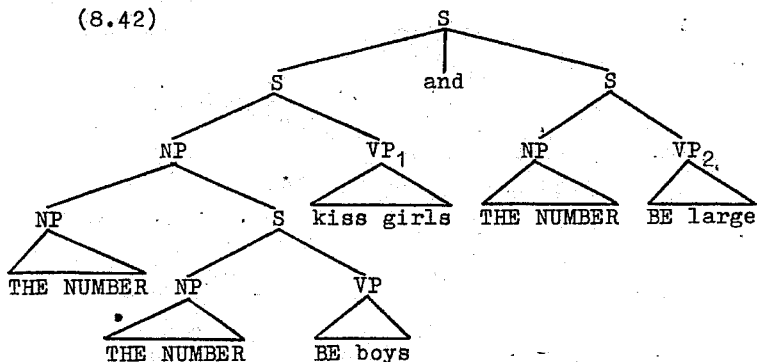
Resorting immediately to semantics, we find the expected near-paraphrase in:

(8.41) The large number of boys kiss girls

This sentence points out one major difference between postdeterminer many and the first type of collocation, for here kiss is the matrix verb and it cannot come from an embedded relative clause; if it did we would be able to derive (37) and that sentence would be synonymous with (40). The highest sentence, therefore, must contain kiss, and its subject must be NUMBER if we are to retain the generalisation that nouns are derived from

predicate nominals. These demands point to an underlying structure of the form:

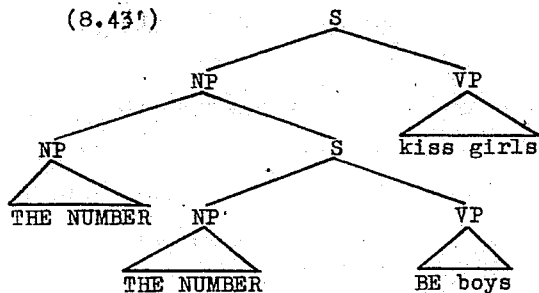
(8.42)



Following the rules outlined in connection with previous derivations it is simple to derive (40) or (41). But one possible criticism of this proposal has to be refuted: it is that (42) violates the previously mentioned selectional restriction for the number that that NP must have a quantity-referring predicate, for although VP₂ meets that restriction VP₁ does not. It has to be admitted that no fully satisfactory reply can be given, but the following answer seems worthy of a little consideration. Let us suppose that the restriction is modified to permit the number to have a non-quantity-referring predicate only under the condition that there is a coordinating predicate which is quantity-referring, in other words, at least one predicate of the number must be quantity-referring. If this restriction is correct, not only will it permit (42), but it will also block:

(8.43) *The some boys kiss girls.

as this would have the ungrammatical structure:



If the above account is correct, then we have been able to provide underlying structures for all occurrences of many except those where it is directly negated. Furthermore, we have shown not only how many is to be related to some, but also why some cannot occur in certain environments where many is grammatical. Basically these are two so far, which have been represented in underlying form by (33) and (42)/(43'). In the last paragraph we saw why some does not appear in postdeterminer contexts: the structure demands a quantity-referring predicate which some does not have. Similarly, it is the lack of the predicate BE large which blocks some collocating with 'definite' NP's of the first type: since there is no such predicate the highest VP in (33) will be empty, and so the structure will be ungrammatical. And we have already noted that the predicate BE large could not be replaced by EXIST, because the subject NP is 'definite'.

There seems good reason, therefore, to believe that the underlying structure for many is as has been presented. Before moving on to examine the interaction of negation with this compound existential - compound because two coordinate sentences form its basic underlying structure - it might be best to see whether or not the other compound existentials can be explained in terms consistent with this basic hypothesis. Let us firstly consider the cases of more and most, which ought to be regarded both synchronically and diachronically as the comparative and superlative forms respectively of many. Within the hypothesis we have been proposing more and most are easily explicable. For example, the difference between many and more in underlying structure will be that for the latter quantifier the predicate of the sentence coordinating with the existential sentence, i.e., S_2 in (20), will be BE larger. Similarly, most will be derived from a superlative form largest. Because (20) demonstrates that many is derived from a noun modified by a nonrestrictive adjective, it is then able to account for the relation between many, more and most and to state explicitly that the latter two are derived from comparative and superlative forms of the nonrestrictive adjective. These quite simple facts are inexplicable in a theory which treats many (and hence more and most) as an unanalysable predicate, such as proposed by Carden (1970c) even when many is a post-

determiner and therefore, in surface structure terms, most adjectival.

One objection to our analysis might stem from the fact that generally superlatives appear to be grammatical only if the NP is 'definite':

(8.44) a Jane is the prettiest girl

b *Jane is a prettiest girl

On the other hand, the quantifier most regularly appears without a 'definite article':

(8.45) Most girls passed the exam

But this ignores the interesting fact that if a superlative is formed by most, then the 'definite article' is not always obligatory:

(8.46) Jane is a most pretty girl

In such cases most seems to be functioning as an intensifier, cf. Quirk et al (1972:287), Bolinger (1972:22). It appears reasonable to claim that the quantifier most is functioning similarly: (45) can be better paraphrased by (47a) than by (47b):

(8.47) a A majority of girls passed the exam

b The largest number of girls passed
the exam

Of course, when most appears in postdeterminer position then it does function as a 'true' superlative:

(8.48) Celtic won the most games that season

(48) is best paraphrased by (49b), not (49a):

- (8.49) a Celtic won the majority of games
that season
- b Celtic won the largest number of
games that season

The above objection, therefore, does not hold, for the grammaticality of most in contexts other than post-determiner can be related to a similar usage of the superlative. But what is perhaps more interesting is that there is some evidence to suggest that certain usages of the comparative and especially the superlative are to be explained in terms of the underlying structures of more and most. This is not simply a matter of the fact that these quantifiers are called in to play a part in comparison formation of adjectives, revealing as that is; what is especially fascinating is that, for example, most pretty is not, pace Quirk et al (1972: 286), a "periphrastic equivalent" of prettiest, as can be seen by comparing (44b) and (46). The grammaticality of the latter, which otherwise would be a mysterious deviation from the rules for superlatives, is explicable because of the fact that most is derivable from, crudely

speaking:⁶

(8.50) A largest N exists

We have been able to demonstrate that the underlying structures hypothesised for many permit an interesting extension to more and most. Let us now consider the case of much, which is in a suppletive relation to many, as can be demonstrated by two facts. Firstly, many only occurs with countable nouns, much only with mass nouns; secondly, much has exactly the same comparative and superlative forms as does many. - Therefore with a mass noun we find the following forms:

(8.51) a Much snow fell yesterday

b More snow fell yesterday (than on
Tuesday)

c Most snow fell yesterday

⁶ The use of N in (50) can be justified in spite of its vagueness. We have already noted in §7.5 that we need both A NUMBER and A QUANTITY, see too below. In the case of comparison perhaps something like DEGREE or EXTENT is needed. But the question of comparison is too complex to be discussed in any detail here; for references to recent work in that field see Hale (1970). Hale's own solution to the problems of comparison is consistent with the remarks made here, see especially his introduction of a "quantifier element" (Hale, 1970: 32).

We can explain this suppletion by postulating A QUANTITY as the underlying noun rather than A NUMBER, on the lines already justified in §7.5. However, much has gaps in its distribution which are not found with any of the other compound existentials and which are totally unaccounted for by our hypothesis so far. The first of these is that (51a), although grammatical, is rather unusual; the second is that much cannot normally appear in postdeterminer position, although that was apparently acceptable in the earlier history of the language, cf. the OED entry for much:

(8.52) *The much snow fell yesterday

And the issue is further confused by the acceptability of:

(8.53) The large quantity of snow fell
yesterday

These facts suggest that it is impossible to account for the distribution of much in terms of the hypothesis which we have constructed above. But there are some slight semantic indications which point the way to an admittedly vague and not wholly justifiable explanation of what is going on. We have already noted, cf. §8.1, that the appropriateness of a number is sharply restricted by the adjectival modifications large and small. But with quantity this does not seem to be so clearly definable. As a result a quantity is often somewhat unacceptable as the subject of a declarative

sentence:

(8.54) ?A quantity of beer was drunk by the
soldiers

Even if (54) is not wholly unacceptable it is certainly
less acceptable than:

(8.55) a A large quantity of beer was drunk
by the soldiers

b A small quantity of beer was drunk
by the soldiers

And alongside (54) we find that substitution by some
improves the acceptability:

(8.56) Some beer was drunk by the soldiers
However, it seems to me that the amount of beer referred
to in (56) is slightly less than that referred to in
(54), although larger than in (55b). This might be
connected with the notion that a quantity is poorly
defined; because of this it seems preferable to give a
more clear-cut notion by deviating slightly from a
theoretical norm. Because the deviation appears to be
insufficient to justify the postulation of an adjective,
some is an excellent candidate for handling the devi-
ation, which it does in a downwards direction. But what
about the possibility of a deviation upwards, towards a
larger quantity? This, it appears, is the task of much.
If this is so, then we can state that much is not always
a suppletive form of many but is sometimes a necessary
semantic deviation allied to a quantity and some.

Even if the above argument from intuition is accepted, there still remains the question of whether its conclusions are formalisable or not. But the following approach seems not without merit. Let us claim that there are two sources for much: (a) from A QUANTITY; (b) from A large QUANTITY. Further, the two sources are mutually exclusive: if one is permissible the other is not. Then we must state conditions on the acceptability of (b), which are that for (b) to be acceptable the underlying adjective must be modified in some way, for example by comparison (which gives more, most) or negation (not much). For some unexplained reason modification by very appears to be dubious. It also has to be stated that much from source (a) is the result of a choice between it and some, where much is the marked choice. From this the following conclusions can be drawn: (i) the unusual character of (51a) is due to the choice of marked much over unmarked some; (ii) the ungrammaticality of (52) is due to the fact that much is there derived from A QUANTITY, which is not adjectivally-modified, and thus cannot appear in postdeterminer position for reasons already given.

So far we have only discussed compound existentials which have as part of their underlying structure a predicate BE large; yet it would be unusual, indeed worrying, if there were not a parallel group with a predicate BE small. Fortunately there is such a group,

and the quantifier in this group which is analogous to many is a few. Since it has exactly those characteristics which would be predicted from the fact that the major distinction between it and many is the change of underlying adjective, it is unnecessary to prolong the discussion of a few, see, however, §§6.2 and 8.4. But we may note that a few, exactly like many, has comparative and superlative forms, namely fewer and fewest. And further there is a corresponding set for collocations with mass nouns: a little, less, least. This last set is rather different from much, more, most, however, and perhaps, therefore, it would be useful to discuss it briefly.

Firstly we should note that a little has a wider range of acceptability than much, as is exemplified by the complete grammaticality of the following:

(8.57) a A little snow fell yesterday

b The little snow fell yesterday

But this can be explained in terms of the account of the relation between a quantity, some and much discussed above. Much has a restricted distribution because it can be used to handle necessary deviations from a norm; but a little is not so used, because downwards deviation is handled by the quantifier some. Therefore a little is always available for derivations from the source A small QUANTITY and these derivations are never blocked by competing derivations from A QUANTITY, which is the

cause of the ungrammaticality or rather dubious character of much in certain contexts. As for the second point of difference, there seems little option other than to note it, for there is no obvious, and even, it would appear, no obscure reason why it should occur. It is that although the comparison of a little is not formed with the same items as the comparison of a few, it is nevertheless the case that less, least can be used, apparently synonymously, instead of fewer, fewest:

(8.58) a Fewer students passed the exam
than failed it

b Less students passed the exam than
failed it

(8.59) a Celtic lost fewest games that season

b Celtic lost ~~least~~ games that season

But the interchange is not reflexive:

(8.60) a *Fewer snow fell yesterday

b *John ate fewest bread

8.4 Negative remarks

In the previous section we noted that it would be necessary to discuss the fact that many, and indeed, all the compound existentials discussed so far, may be negated. The interaction between negation and quantifiers is complex, but nevertheless we ought to observe that given underlying structures like (20) it is not a matter of surprise that such interaction occurs. Without

Lakoff (1970d:395) has argued convincingly that few is to be derived from not many. We can now see that in detail this is not correct, although the principle of Lakoff's claim does appear to be correct. Instead of claiming that few is derived from not many, which is rather difficult to justify since, as we pointed out in Chapter 5, the Lakoff-Carden analysis gives no proper explanation of why many should differ from some in any way, we can claim that few is derived from the same source as not many, with the additional transformation of negative absorption to produce few, which is needed by Lakoff in any case. Thus (61) is also the underlying structure of:

(8.63) Few boys kissed few girls

Similarly, if we are dealing with the mass quantifier much, we shall be able to obtain both not much and little. Note that, as observed in §8.3, much is in this case derived from an adjectival source. In the cases of a few and a little there appears to be no absorbed negative quantifiers, but this is probably due to the fact that not a few and not a little are stylistically marked, being examples of litotes, cf. Bolinger (1972: 123) and below:

- (8.64) a Not a few self-proclaimed socialists
send their sons to public schools
b Not a little money has been wasted
on Concorde

Before moving on to discuss some more complex problems, it is necessary to return at this point to a problem raised by Carden (1970c) and discussed in §8.2. It is that not many can appear in some contexts where not + adjective is ungrammatical. Thus we find the following examples, repeated here for convenience:

(8.11a) Not many people came to the party

(8.12) *Not happy inmates escaped

But we can now see, by referring to (61), that there is a crucial distinction between the two occurrences of not. In the first instance not is associated with an adjective and thence with a noun which together have been lexicalised into many; in the second instance not is associated with happy and thence with inmates, and neither of these has been absorbed. It seems quite clear, and consistent with Carden's own remarks (1970c: 418), that this absorption process, together with the fact that not is only in the same NP as people in (11a) at surface structure, accounts for the difference between the two sentences.

It is also possible to account for the ungrammaticality of Carden's postdeterminer example, again repeated here:

(8.13) *The not many inmates escaped

Although in this case large NUMBER is collapsed into many, the required modification of (42) shows quite undeniably that not is in the same NP all the way from

the adjectivalisation of the nonrestrictive clause right up to surface structure. If our earlier claim that the constraint blocking an overt negative from appearing inside an NP is a shallow structure constraint, then this will account for the ungrammaticality of (13). Carden further claims (1970c:419), however, that negative absorption is possible in (13), giving:

(8.65) The few inmates escaped

The acceptability of (65) may be parallel with the acceptability of:

(8.66) The unhappy inmates escaped

but it is not certain that the negative absorption process is identical in (65) and (66), which it clearly needs to be in order to uphold the parallelism. We return to this question below.

In an important paper Lakoff (1971c) claims to demonstrate that not only must an adequate grammar of a language contain transformational rules or "local derivational constraints", but that it must also contain "global derivational constraints". Just as transformations define possible derivations by constraining pairs of successive or adjacent phrase markers, so global derivational constraints define possible derivations by constraining pairs of non-successive or non-adjacent phrase markers, cf. Lakoff (1971c:233-34; 1970a). This perhaps would not be so relevant to our present concerns were it not for the fact that Lakoff attempts to show

that global constraints (or rules) are necessary in order to obtain correct and adequate derivations for sentences containing a negator and a quantifier. It therefore behoves us either to show that our proposed hypothesis can be accommodated within Lakoff's theory or to disprove that theory. The problem and solution which is suggested by Lakoff is so complex that we can only attempt to provide a partial resolution of the question and we shall only discuss one of the several problems analysed by Lakoff in the hope that our own answers might provide a useful programme for study.

Lakoff (1971c:244) discusses a dialect in which (69) is synonymous with (67) but not with (68):

(8.67) Not many arrows hit the target

(8.68) Many arrows didn't hit the target

(8.69) The target wasn't hit by many arrows

Following §5.4 we may say that in all dialects (67) has only a neg-Q reading and (68) only a neg-V reading, and that in the dialect with which we are concerned (69) has only a neg-Q reading (there are other dialects in which (69) may have either a neg-Q reading or a neg-V reading, see below). Lakoff suggests that (67) and (68) have the readings of (70) and (71) respectively:

(8.70) [_Snot[_Sarrows_i[_Sarrows_i hit the target]were many]]

(8.71) [_Sarrows_i[_Snot[_Sarrows_i hit the target]]were many]

The global constraint which handles the derivations from such structures is as follows (Lakoff, 1971c:244, 246):⁷

(8.72) Let: $C_1 = L^1$ commands L^2

$C_2 = L^2$ commands L^1

$C_3 = L^1$ commands L^2

Constraint 1': $P_1/C_1 \supset (P_a/C_2 \supset P_a/C_3)$

Translated into ordinary language (72) states that if the first S-node higher than some negator or quantifier dominates some other negator or quantifier at underlying structure and the first S-node higher than that other negator or quantifier dominates the first negator or quantifier at shallow structure, then the first negator or quantifier precedes that other negator or quantifier at shallow structure.

Let us examine how this constraint works in relation to (67) - (69). Firstly, (67) ought to be derived from (70). In (70) not commands many; in (67) many commands not and not precedes many; therefore the derivation from (70) meets the constraint. Similarly, a derivation from (71) to (68) meets the constraint, for in (71) many commands not and in (68) not commands many

⁷ Abbreviations are as follows: L = quantifier or negator; C = tree condition; P_1 = underlying structure; P_a = shallow structure; P_1/C_1 = tree condition 1 is satisfied at underlying structure.

and many precedes not. In (69) not commands many and many commands not, also not precedes many. Therefore in the dialect where the constraint applies not must command many in underlying structure. That is true of (70) but not of (71), and therefore (69) is derivable from the former only. This explains precisely the questions at issue, and since it does so in a manner which is both revealing and capable of extension, cf. Lakoff (1971c), it seems desirable that our own hypothesis be consistent with it.

It is indisputable that (61) as it stands is not completely consistent with the constraint, for the underlying structure of (67) would then be, somewhat simplified:

(8.73) [_S[_SA NUMBER EXIST[_SA NUMBER hit the
target[_SA NUMBER BE arrows]]]and[_SA
NUMBER neg BE large]]

(73) does not meet tree condition 1 which states that not (or neg) commands many in underlying structure, as the first S-node higher than neg does not dominate many but only a part of it, namely the coordinate partner of the higher existential. Also, and more seriously, that part of many commands neg. Let us, however, take another look at (70), for then we can see that neg is to be taken as a higher predicate. Therefore (73) must be amended to:

- (8.74) [_S[_SA NUMBER EXIST[_SA NUMBER hit the target[_SA NUMBER BE arrows]]]and [_Sneg[_SA NUMBER BE large]]]

This still doesn't help a great deal, it seems, for although neg now partially commands many in underlying structure and many does not at all command neg there, the command is partial, because of the compound structure of many. If we now examine the underlying structure entailed by our hypothesis for (68), we find that much the same problem exists there:

- (8.75) [_S[_SA NUMBER EXIST[_Sneg[_SA NUMBER hit the target[_SA NUMBER BE arrows]]]and[_SA NUMBER BE large]]]

The only difference here is that many commands neg in underlying structure, but again the command relation is only partial. What is worse, it is the existential partner, not the quantity-referring partner, which does the commanding this time.

We are in all the more serious trouble for there does not appear to be any scope for modifying our hypothesis to make it fully consistent with the constraint and yet not at the same time change it beyond recognition. The only possibility would be to move neg into the same sentence as the conjunction marker in (74), but this is open to two objections. The first is that both Lakoff (1970c:150ff.) and R. Lakoff (1971:145) have shown that neg is dominated by the S-node which dominates

only the matrix sentence which is negated and sentences embedded in that lower S, i.e., that neg has precisely the position exemplified by (74). But even if we ignore that point then the second objection still holds, for it concerns the dubiety of making a command relation between the existential partner, rather than the quantity-referring partner, and neg crucial. We could attempt to circumvent this in two ways: one is to say that the command relation need only hold between the existential sentence and neg regardless; the other is to permute the existential and quantity-referring sentences. But the first way still remains suspect, especially because it is the relation between the neg and the quantity-referring predicate that is important; it is only through such a predicate that neg can be introduced into the underlying structure of existential quantifiers. The second way out is suspect because if it is adopted there will be two mechanisms for introducing existential quantifiers into sentences: for some it will be from a higher existential; for many from a higher quantity-referring sentence with an existential sentence attached. Not only is that uneconomical but the second structure is totally incomprehensible.

It might not be altogether immodest, however, to suggest that it is not our hypothesis which is incorrect but Lakoff's. And there is one very good reason for this, namely that Lakoff's hypothesis rests on one

assumption which we have observed to be fundamentally mistaken. This is that quantifiers form unanalysable (logical) predicates. It is this which is the major source of conflict for it has become clear that the difficulties which we are discussing stem from the fact that we have postulated two (coordinate) higher sentences for the source of many. The global constraint suggested by Lakoff only works if many is derived from a single higher sentence and thus it provides a potential counter-example to our hypothesis. But it is the only possibly valid counter-argument which we have encountered, and that makes it suspicious. Would we not therefore be justified in modifying the constraint to accommodate a complex source for many and then seeing whether this modification was plausible or not. The modification which I would propose is that "L" is either a quantifier or a neg element or part of such an element, where "a part of" means one member of a coordinately-conjoined structure. This must be regarded, however, with considerable suspicion. What is really required is ^{between} that the constraint applies not ~~being~~ underlying structure and shallow structure, but between the point at which coordinate conjunction of the higher sentences applies and shallow structure. But global rules require two well-defined levels of structure and that solution would clearly fail to provide these. Therefore the most probable outcome is that our solution is incompatible with Lakoff's proposal.

If Lakoff's constraint is correct then it is indeed the case that our own position is very weak, because although it is consistent with a modification of that constraint the modification is not very plausible and it makes the constraint very complex. But the correctness of the constraint is by no means certain, and not only for the reasons given in the previous paragraph. Consider firstly the three sentences below:

(8.76) Not a few arrows hit the target

(8.77) A few arrows didn't hit the target

(8.78) The target wasn't hit by a few arrows

According to the constraint (78) ought to have as its primary (or even only) reading one which is synonymous with (76), because in surface structure the negative element precedes the quantifier and each commands the other. But in fact the primary (and probably only) reading of (78) is equivalent to that of (77), as the absence of litotes in the former clearly indicates. Further evidence on this point is given by Bolinger (1972:123):

"The diminishers differ from the other intensifiers [e.g., a little as opposed to very: RMH] in that the negative tends more strongly to show itself as an immediate constituent of the intensification, and not be absorbed by the verb. This is to say that it adjoins the intensifier, with only the indefinite article potentially intervening.

Few and little show this particularly:

If it is worth not a few sacrifices,
it is worth a great deal.

If it isn't worth a few sacrifices,
it isn't worth much.

I have spent no little time in trying
to convince them.

*I haven't spent a little time in
trying to convince them. "

Whatever the reason, Lakoff's constraint gets exactly
the wrong answer in the case of (78).

Another point which we might note is that since the
global constraint is almost wholly concerned with dis-
cussing active - passive correspondences, the impression
might arise that the constraint is designed to handle
derivations where an intermediate transformation, such
as the passive, reorders NP's. But this is not entirely
so, for as can be seen it is (79a), not (79b) or (79c),
which causes difficulty:

- (8.79) a John didn't buy many arrows
b Not many arrows were bought by John
c Many arrows were not bought by John

This prompts the feeling, reinforced by Bolinger's
remarks above, that it is when the quantified phrase is
a surface object that the possible ambiguity or failure
of underlying scope relations arises. This is a point
originally made by Jackendoff (1969:222-31) and repeated

in Chomsky (1971:103-6) and Jackendoff (1972b:325-36). Both authors argue that the scope of negation is determined at surface structure and that it ranges over the structure dominated by the node which is then immediately above neg. Thus in (79a) the scope of the negation is over the VP, including the quantifier, and in (79c) the scope is also over the VP, but naturally excluding the quantifier; in (79b), however, the scope is over the whole sentence, thus including the quantifier. The objection to such a solution, of course, is that the passive transformation will be meaning-changing, or, rather, it will relate (79a) and (79c) which are different in meaning, and it was this objection that Lakoff's postulation of a global constraint was designed to overcome. It should also be pointed out that Jackendoff's analysis fails to account for dialects, such as Lakoff's and my own, where sentences such as (69) and (79a) are ambiguous. And, of course, Jackendoff and Chomsky are committed to semantic rules which occur at other points than underlying structure, which is a basic complication of the grammar.

We now appear to have reached a dead end, for neither the hypothesis that scope is determined at underlying structure, the derivations from which are constrained globally, nor the hypothesis that scope is determined at the surface, is able to account for the variations we have observed. Unless we are able to

discover further evidence which supports one or other of these proposals or yet another hypothesis we shall be in an impossible situation. Therefore consider the following pair of sentences:

(8.80) a Not many stamps are not collected
by John

b *John doesn't collect not many stamps

Since the former is a passive transform of the latter both ought to be equally grammatical, but in fact the active partner is ungrammatical. At first sight this seems explicable in terms of the surface constraint suggested by McCawley (1969), cf. Langacker (1972:234). This constraint blocks the appearance of two negatives in one VP, thus accounting for the alleged ungrammaticality of:

(8.81) Max doesn't not like music

But Carden (1972:39) has produced some data, for American English at least, which shows that (81) is marginally acceptable, and even if the degree of acceptability is low, it certainly seems to be much higher than for (80b), which is totally unacceptable. Thus although we might well agree that McCawley's constraint is reasonable, if not always totally fulfilled, it would seem incorrect to apply it equally to (80b) and (81), since the two sentences show perceptibly different degrees of acceptability.

Suppose, therefore, that we attempt to account for (80b) by a rather different constraint, which we shall state provisionally as:

- (8.82) No negated quantifier may be lowered
into a negated VP

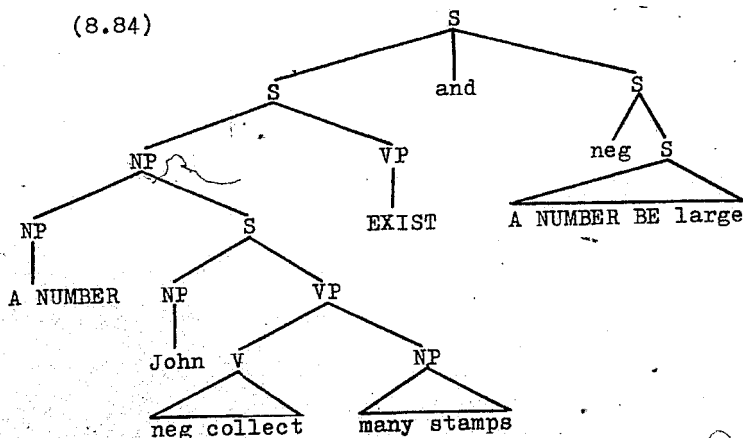
Not only will this account for (80b), but it will also account for the relation between the following pair:

- (8.83) a Not many arrows didn't hit the
target

- b *The target wasn't hit by not many
arrows

It is not possible to account for (83b) by claiming that in this case the passive transformation must not apply, for if we compare (80) we find that it is the active partner which is ungrammatical. Therefore in one case the passive transformation must apply, in the other it must not, and this is determinable solely in terms of whether the negated quantifier ends up in the negated VP or not. To state this fact in terms of conditions on the passive transformation would be cumbersome, and also to hide the true nature of the syntactic processes at work with an ad hoc formulation. On the other hand, as long as existential-lowering applies after passivisation, and since the latter is a cyclical transformation this can be predicted, our putative constraint (82) is perfectly adequate.

But nevertheless (82) seems unsatisfactory, since although it purports to be a general constraint on all possible transformations it appears merely to operate with regard to existential-lowering. We shall see below that this statement has to be modified, but at present let us try to resolve this by considering a simplified structure of the underlying form of (80b) in which we accept that the neg on many originally dominates the S containing the quantity-referring predicate, and with the neg on the VP already lowered:

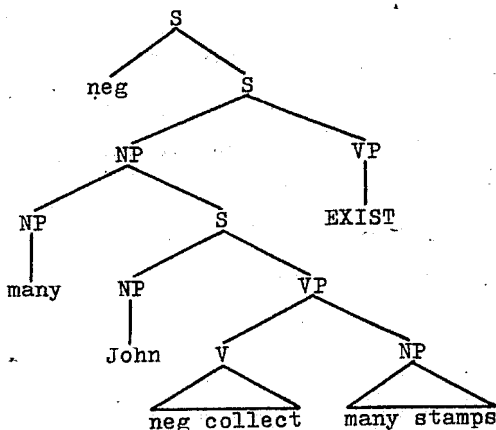


Let us now accept that the two sentences which constitute the underlying elements of the quantifier are conjoined; this process seems to be necessary if we wish to account for the grammaticality of sentences where existential-lowering does not take place, as in:

(8.85) There are not many stamps, John
doesn't collect.

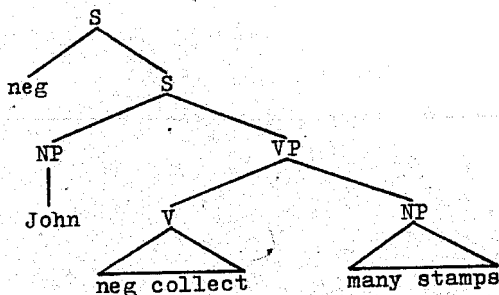
(85) also suggests that our supposition in §7.5 that lexicalisation to many takes place before existential-lowering is substantially correct. Therefore after both coordination conjunction and lexicalisation have taken place we shall find:

(8.86)



If we now suppose, but, as we shall see, not entirely correctly, that existential-lowering follows, the question is: what exactly happens to the highest neg? I would suggest that the lowering transformation operates to produce the following structure:

(8.87)

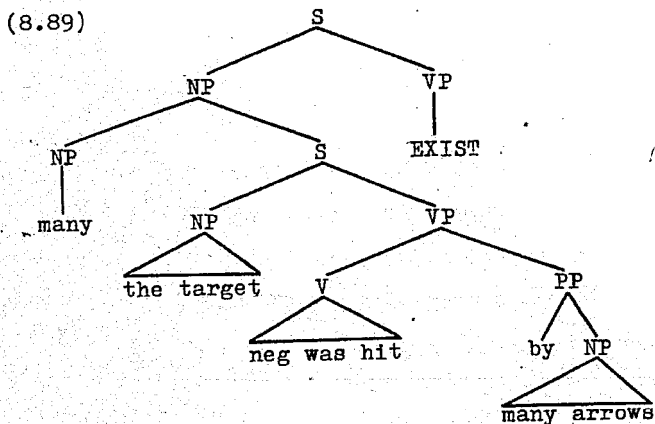


There are two justifications for claiming that the transformation has this result. Firstly, the transformation will then preserve the structural relations between the higher neg and many as closely as possible, crucially, command relations are not distorted and the same shape of tree relation is found both before and after the transformation. To some extent this echoes various remarks of Emonds (1970), although the parallel cannot be pursued too closely. Secondly, as can be seen from the above together with a brief demonstration below, no new transformations will be needed; this point is partially vitiated by a further point which we come to rather later in this discussion, but it will then be clear that it is not an absolute objection. No new transformations are needed because in the first place existential-lowering in the instance of (86) to (87) is no different from that in instances where there is no negative, and in the second place we can now lower the higher neg in accordance with normal rules. But these have the effect of (eventually) placing the neg exactly in the position already occupied by a neg. Therefore the structural description for neg-placement cannot be met and the derivation is blocked. It will never then be possible to apply a further rule which might move the appropriate neg over the verb and into the quantified NP, giving the surface structure of (80b).

Assuming that the above account is correct, how does it enable us to explain the facts of that dialect in which (69) has only the reading which is synonymous with (67)? This is especially important in the light of the evidence given by Johansson (1974:26) that this is the most common dialect. Now although we have explained the ungrammaticality of (80b) simply in terms of a transformational rule, this has the same effect (approximately) as constraint (82). Let us suppose, therefore, that in the dialect with which we are concerned there is a generalisation of (82) to:

- (8.88) No quantifier may be lowered into
a negated VP

Thus in the case of (69) it would have the structure immediately before existential-lowering of (89), if it had a neg-V reading equivalent to (68):



But existential-lowering fails because of (88). This

need not be stated as a constraint but could be given as part of the structural description if this were desirable. Of course, in that dialect in which (69) is ambiguous no such restriction is to be found. It is interesting to note that the constraint outlined in (88) would apply in a quite different case, so that in the dialect where (69) is synonymous only with (67), but in that dialect only, (90) will be ungrammatical:⁸

(8.90) John didn't buy some arrows
for the underlying negative cannot command a simple existential, as we observed previously. Unfortunately, probably because of the ease with which stress patterns can be changed to allow grammatical interpretations, as:

(8.91) John didn't buy some arrows
it is difficult to estimate whether or not this prediction is correct.

The above argument is largely a modification of a proposal which I made in Hogg (1974) and which is criticised in Johansson (1974). But there are four objections by Johansson which still need to be answered. Three of the objections concern the ordering of neg-lowering and neg-placement so that, for example:

(8.92) ??John bought not many arrows
would be derived from an earlier structure corresponding to:

⁸ That is, if normal stress patterns hold.

(8.93) John didn't buy many arrows

But of course in (93) the not may be, in some dialects, ambiguous between a neg-V reading and a neg-Q reading; in contrast, (92) unambiguously has a neg-Q reading. Therefore the neg-placement rule to generate (92) will have to have access to information about earlier structures, that is, it will have to be global. Naturally this is only an objection if we are trying to dispense with global rules, but since that is one of our objectives we must find some alternative solution. And we can indeed do so by proposing that in those dialects in which (92) is acceptable there is an optional neg-lowering transformation which may apply before existential-lowering, ~~but~~ after coordination conjunction. In such instances, therefore, the neg will be lowered onto many before many itself is lowered, and then the negative and quantifier will be moved together into the v rb phrase of the matrix sentence with the deletion of EXIST. This has the effect that (82) and (88) will now have to be retained as constraints, and the justification for (87) will be primarily that of motivating the constraints in question, rather than in avoiding their necessity.

There are two strong arguments against Johansson's counter-proposal that the reading of (93) equivalent to (92) should be derived from a structure corresponding to (92). The first of these is that (92) is only very weakly acceptable and therefore it is unfortunate that

the fully acceptable and, indeed, preferred reading of (93) should have to be thus derived. The second is that there does not seem to be any other transformation which moves a neg leftwards over a (surface) verb and preserves meaning. The only other rule which approximately corresponds to this is Neg Transportation which, as we have seen in §5.4, is meaning-changing. If we reject Johansson's suggestion we can postulate a useful constraint on neg-movement rules to this effect. Further, there is a good argument against the proposals of Hogg (1974) and in favour of the above, which is analogous to our second argument against Johansson. Our original proposal moved a neg rightwards over a verb, but there does not appear to be any other rule (in Modern English) which does so. Since this does not happen in the case of a derivation such as we have now suggested, this implies that we can also restrict neg-placement so that it never performs such a task either. All these arguments, with their interesting restrictions on movement transformations, cf. Emonds (1970), combine in favour of our new hypothesis.

The second objection given by Johansson (1974:25) is that my proposals would apparently relate such non-synonymous pairs as:

- (8.94) a He left not many minutes later
b He didn't leave many minutes later

This is very probably a valid criticism of the proposals

in Hogg (1974), but note that all of Johansson's examples involve a quantifier in an adverbial phrase, and so far we have been concerned with verb phrases. But if we accept, and this seems to be correct, Lakoff's (1970c: 157) conclusion that adverbials originate in a higher sentence, then (94a) will have at no point a structure which will permit the neg to find its way into the verb phrase, given the process of derivation for adverbials suggested by Lakoff. It is also useful to compare in this respect the grammaticality of (95) and (96):

(8.95) But not many years ago (for once) he
did not visit the US

(8.96) *Not many people doesn't she take into
her confidence

If (95) had the same underlying structure, approximately, as (96) we would expect it to be ungrammatical also, since it would at some stage contain a negated quantifier in a negated VP. There thus seems no reason for rejecting our present analysis on the basis of evidence from examples such as (94).

Example (96) above is involved in Johansson's third objection. If our own proposals are accepted then the ungrammaticality of that sentence can only be accounted for if we accept that existential-lowering takes place before the Y-movement transformation which preposes the object NP. Now this is in fact welcome, for Postal (1971:142-49) has shown that Y-movement is most probably

noncyclical and therefore occurs after a lowering transformation of the type we have discussed. On the other hand, Johansson (1974:25) claims that:

(8.97) Many people she doesn't take into
her confidence

is not ambiguous and certainly never has the neg-Q reading predicted as possible by ordering Y-movement after existential-lowering. If Johansson is correct then existential-lowering must both precede and follow Y-movement. But there is a conflict of opinion here, for although Johansson's claim is shared by Jackendoff (1972b:333), Lakoff (1971c:246) claims that (97) has exactly the same set of readings as:

(8.98) She doesn't take many people into
her confidence

If Lakoff is correct, then no ordering conflict is found. Unfortunately there is no clear resolution of this contrast in opinion and therefore we cannot be certain that there is a valid objection on this point (nor, of course, can we be entirely happy about the adequacy of our own proposals). It is even more strongly the case that there is no definite reply to Johansson's fourth objection, which is that when the quantifier in the VP is in a partitive construction there is a stronger tendency to a neg-V reading, see Johansson (1974:26-27) and compare (99) with (69):

(8.99) The target wasn't hit by many of the
arrows

It has to be admitted that we have provided no explanation of why this may be so.

Notwithstanding these last points, we may conclude that the constraints on existential-lowering into VP's suggested above provide a more adequate explanation of the sentence types illustrated by (67) - (69) and consequent examples than is provided either by Lakoff (1971c) or Jackendoff (1969, 1972b). Furthermore, these constraints are local constraints, in other words they are the product of or operate on normal transformational rules, and these rules do not change meaning. Therefore they do not provide counter-examples to the hypothesis that meaning is determined wholly at underlying structure and consequently they suggest that the introduction of global rules or rules of semantic interpretation into the theory of transformational grammar may be quite unnecessary. Finally, they are completely consistent with our proposed source for quantifiers, partly because conjunction reduction will have operated on the quantifier structures in question before the transformations and constraints apply. As far as quantifiers in subject position are concerned, there is absolutely no problem, since then existential-lowering is always followed by neg-lowering in such a way that the neg is always lowered into a surface position where it precedes the quantifier itself, and so no difficulties of scope assignment or 'interference' from a V node arise.

8.5 Conclusion

The major import of these two chapters is that it has been demonstrated that all existential quantifiers ought to be derived from a higher existential sentence where the quantifier is a noun; in the case of compound existentials this sentence has a coordinate partner which contains a quantity-referring predicate. With such a basic structure we are able to account for the discrepancies in distribution between simple and compound existentials, such as, for example, the ability of the latter to appear grammatically in postdeterminer and negative contexts, or the fact that the latter are subject to the adjectival process of comparison. Further, we have been able to account for a large range of sentences, notably surface existential sentences, which are inadequately explained by other hypotheses. We have also been able to observe that the interaction of negator and quantifier elements is only fully explicable in terms of a theory which is consistent with our proposed underlying structures, and that alternative suggestions which to a greater or lesser extent contradicted our theories about the underlying structure of quantifiers, especially compound existentials, did not explain the full range of the problem satisfactorily. One point which we have barely touched upon, but which lends further support to our proposals, is that, whereas some is almost, but not quite, the only simple existential,

there is a fairly large group of compound existentials. This is easily explained, for it is undeniable that it is the quantity-referring sentence, with its scope for negation and the introduction of alternative predicative adjectives, that is the productive element in the structure. The existential sentence is scarcely capable of generating a whole range of quantifiers.⁹

It would be foolish to claim that we have given an adequate explanation of the behaviour of all existential quantifiers. For example, we have not explained why the simple existential several can appear in postdeterminer position, although that is probably because it is then not the same lexical item as the quantifier, having, as it does, a rather different meaning in that context. Also, the distribution of enough has not been accounted for, but we can only plead in mitigation that it is extremely difficult to determine what kind of quantifier it is. No doubt there are several other such 'problem cases'. Nevertheless, it does seem reasonable to claim

⁹ But we ought not to forget surface NP's such as a group, which derive from a structure similar to that for some. They do not appear, however, to be available for the lexicalisation process associated with NUMBER. And as we have seen, there is some small scope for nouns other than NUMBER to appear as the subject of the higher existential.

that we have established with some degree of certainty the basic underlying structures which must be assigned to existential quantifiers, and so we may now proceed to an analysis of universal quantifiers before returning in Chapter 10 to some residual problems. In Chapter 9 we shall attempt to prove that the underlying structure of universal quantifiers is rather different from that of the existentials with which we have so far been concerned. This may not be too difficult in view of the quite different surface behaviour of the universal quantifiers; but we must wait and see.

Chapter 9

Universal quantifiers

9.1 Are universal quantifiers quantifiers?

The heuristic procedures which were employed in §7.1 give the impression that all, which we shall take for the moment as the paradigmatic universal quantifier, occupies, as it were, a half-way house between the simple and compound existentials. Very simply, this is because, as we observed in examples (7.1) - (7.3), some can neither be negated nor occur in postdeterminer position, many can be negated and can occur in postdeterminer position, and all can be negated, following many, but cannot occur in postdeterminer position, following some. If there were no other evidence, then all that we would need to do would be to formulate a possibly rather simple rule which would block occurrences of all in postdeterminer contexts and this, one suspects, might be the only difference between the derivation of compound existentials and the universals. Of course the rule would appear to be ad hoc, but it would be extremely difficult to falsify.

But the universals display other surface characteristics which rule such a proposal out of court, and therefore we need not consider it seriously. Perhaps the most striking feature here is that all (but not

necessarily the other universals, see below, §9.5) can occur in positions other than those immediately preceding the NP with which it (putatively) collocates. This well-known fact has been commented upon extensively, cf. Garden (1968), Dougherty (1970:866-7; 876-78) and Anderson (1973c). The following examples are included in the data:

- (9.1) a All cricketers write poetry
b Cricketers all write poetry

For reasons which we shall shortly discuss, we can obtain a wider range of constructions by considering collocations with 'definite' NP's and therefore to (1) we may add:

- (9.2) a ~~The~~ cricketers are all writing poetry
b The cricketers have all written poetry

Such a distribution can obviously be compared with the distribution of adverbs, cf. Fiengo and Lasnik (1973: 465):

- (9.3) a Cricketers rarely write poetry
b Cricketers have rarely written poetry

However it would be foolish to jump too rapidly to the conclusion that the universal quantifiers are in fact adverbial in character. This is not because all etc. supposedly collocate with nouns, for it might be that some explanation of that could be found; nor is it because all is ungrammatical if moved to a post-object

NP position, compare:

(9.4) a *Cricketers write poetry all

b Cricketers write poetry rarely

After all we might be able to explain this by forbidding a universal quantifier, unlike a 'true' adverb, to move across an NP boundary which is not a boundary of its collocating NP. A similar restriction holds between adverbs and VP's, for (5a) is only acceptable if loudly is construed with awoke, just as in (5b) loudly can only be construed with snored:

(9.5) a John snored and awoke loudly

b John snored loudly and awoke

More telling than either of these points, which might permit of some ingenious explanation, is that those adverbs whose distribution in large measure parallels that of all are in fact exceptional. By far the largest class of adverbs have a distribution similar to that of, for example, quickly:

(9.6) a ?Cricketers quickly write poetry

b ??The cricketers are quickly writing poetry

c ??The cricketers have quickly written poetry

d The cricketers have written poetry quickly

With such adverbs the preferred position is clause-final, the other positions are to some degree or another unacceptable. This is in contrast to the evidence of rarely,

given above. Now it is extremely interesting to note that the adverbs most clearly resembling all can be analysed as containing an underlying quantifier: for example, rarely approximates to on few occasions; often approximates to on many occasions. This leads one to suspect that the distribution of rarely, etc. is to be partially explained by the properties of quantifiers, rather than that the distribution of all is to be explained by the properties of adverbs. Nevertheless we shall see below that this statement needs to be modified.

Yet it naturally follows that we must now return to the hypothesis that universal quantifiers are indeed quantifiers. But there are at least three facts which imply that they are not all to be derived from a source identical in basic structure to that for either simple or compound existential quantifiers. The first of these facts is that which we have already noted, namely that all (at least) can appear in surface positions which are closed to all of the existential quantifiers. This must be a product of a difference in underlying structure, unless we are to be hopelessly ad hoc. Merely accepting that point, since we have failed to find its immediate explanation, let us proceed to the second fact, which is that an NP containing a universal quantifier cannot function as the complement of a surface existential sentence, cf. §7.4. Therefore sentences such as those

in (7) are ungrammatical:¹

- (9.7) a *There were all cricketers writing
poetry
- b *There was each cricketer writing
poetry

This suggests that it may be impossible to justify an underlying structure for all which contains as its primary element an existential sentence of the type which is the source of some, even if that sentence is modified by a nonrestrictive relative clause such as is necessary for the derivation of many. The third fact is rather less certain than the previous two, but nevertheless it seems worthy of attention. It is that although it is quite simple to paraphrase (at least in a crude fashion) some, many and the other existentials by substituting a number with appropriate adjectival modification, such a paraphrase of all is not to be found. The most obvious stratagem would be to modify a number by an adjective such as total or complete, since they have the necessary semantic implications, but the resultant 'paraphrases' are ungrammatical:

¹ For this writer there are two factors contributing to the ungrammaticality of (7). But the second factor, which is discussed in both §6.3 and §6.4, may be ignored for the present.

(9.8) *A total/complete number of cricketers
write poetry

Since the reason for the ungrammaticality of (8) appears to be that a number, unless it has the meaning of "an integer", cannot be modified by an adjective of the same type as complete, it is reasonable to claim that any such source for a universal quantifier will be impossible.

Although we may conclude from the above argument that all is not to be derived from a structure only trivially different from that for, say, some, we are not therefore entitled to claim that all is not quantifier-like. We have already discussed one reason for this in connection with adverbs, and despite the fact that the other reasons are rather obvious it is useful to reiterate them. Firstly, all does occur regularly, indeed most commonly, before the NP with which it collocates. Secondly, the interaction of negation and all is in most, but not all, respects almost identical to that between negators and the compound existentials. Thirdly, all occurs in partitive constructions of the form Q of the N, although again we have to note that after all of may be omitted. Therefore if we conclude that the underlying structure of universal quantifiers is completely different from that of the existential quantifiers, we shall be able to be fairly certain that our conclusion is incorrect, for the common features which indisputably bind all quantifiers together will be seen

as purely haphazard, and their communality will be unexplained by any generalisation about their linguistic structure. On the other hand, we cannot merely claim that the underlying structure of, say, all is the same as that of, say, some, which is approximately the position held by Carden (1968) and Jackendoff (1968), for then the differences are only accounted for by ad hoc transformational rules, see below, especially §9.5.

Before accepting that this is the case, however, it is necessary to note that Dougherty (1970:864-71) has put forward an account which will, apparently, explain the differences between not only existential and universal quantifiers, but also the differences between the various universal quantifiers. Unfortunately for us, Dougherty does not discuss existential quantifiers explicitly, but it seems probable that he would introduce all quantifiers by a phrase structure rule of the form:

(9.9) $X \rightarrow (Q) X^n (\text{Adv})$

where Q is a quantifier, X is a major category (S, NP or VP) and Adv is an adverb. There would then probably be a selectional restriction whereby existential quantifiers are constrained to collocations with some NP rather than S or VP. As can be observed, this will distinguish properly, but only partly, between existentials and universals as far as the ability of the latter to occur in contexts closed to the former is

concerned. We may legitimately question, however, whether it does more than this. Thus it does not give an explanation of why the different types of quantifier react differently to negation, nor why only existential quantifiers are associated with simple existential sentences, underlying or derived. Also we may note in general that Dougherty's suggestion is inconsistent with the type of underlying structure which we have claimed is necessary if we are to hope to explain the semantics and syntax of the existentials. Of course this does not per se deny the validity of Dougherty's analysis, but it does mean that if we accept his account of universal quantifiers (broadly but not exactly equivalent to his category of 'distributive' quantifiers), then we shall be unable to relate them to the existentials without dismissing the arguments which we have put forward in Chapters 7 and 8. Since that seems undesirable, we shall not follow Dougherty's account, but we must note that several of his observations are most interesting and to these we shall occasionally return below.

9.2 Do two negatives make an 'all'?

Anderson (1973c) observes that although the lack of an overt existential when all collocates with the complement NP, cf. (7) above, is a major obstacle in the way of generalising the existential structure to universal quantifiers, there is in fact an overt existential

paraphrase which meets the condition of at least near-synonymy which is necessary. Thus compare the following triple, where, following Anderson's discussion, we use the partitive structure with all in examples; we ignore the degree of acceptability, if any, of (10c):

- (9.10) a All of the girls came
b None of the girls didn't come
c There were none of the girls who
didn't come

As Anderson points out, this paraphrase relation opens the way to an account of the universal quantifiers in which they are derived from a structure identical to that for the derivation of some, but cf. §10.2, except that there are two negations, one on the higher existential sentence, one on the matrix sentence.

In discussing Anderson's work it will be necessary to make some adjustment to it, since he uses, as has been stated previously, a case grammar - more properly, 'localist' - framework, cf. Anderson (1971b), but this does not appear to lead to any serious misreading of his analysis. We shall also have to accept that the higher existential sentence may be negated and that this negation produces the surface quantifier no/none. Even if we may disagree in detail with Anderson's remarks on this question, cf. again §10.2 where the problem is discussed more extensively, the principle of the operation seems correct and we may thus accept that it is a justifiable

procedure. Finally, it seems useful to employ a simplification of the structures proposed in §7.5 as the underlying representation of some, for this will clarify the discussion without distorting the various analyses. From this it follows that we can propose (12) as the appropriate underlying structure of (11) as a fair compromise between Anderson's theories and our own:

(9.11) Some of the girls came

(9.12) [_Ssome EXIST[_Ssome of the girls came]]

where we further ignore the problems of the partitive quantifier constructions, cf. §10.3.

It is relatively uncontroversial to suggest that either the higher or the matrix sentence may be negated, that is, that both (13) and (14) are possible underlying structures:

(9.13) [_Sneg[_Ssome EXIST[_Ssome of the girls came]]]

(9.14) [_Ssome EXIST[_Sneg[_Ssome of the girls came]]]

(13) will be the underlying structure for (15), (14) that for (16):

(9.15) None of the girls came

(9.16) Some of the girls didn't come

Anderson's basic hypothesis is that it is possible for both negations to occur simultaneously, as in:

(9.17) [_Sneg[_Ssome EXIST[_Sneg[_Ssome of the girls came]]]]

It is clear that (10b) may be derived from (17), and so too may (10c), provided that it is possible to retain the underlying existential sentence at the surface. As it is indisputable that some underlying representation must be provided for (10b) - for similar sentences see Carden (1972) - and since (17) meets all the requirements which such a representation ought to meet, there can be no argument about the validity of the hypothesis so far. Two questions, of course, remain. The first of these is a semantic one: is the meaning relation between (10a) and (10b) sufficiently close to justify the postulation of a transformational relation between them? The answer to this, pace the apparently contrary argument of Jackendoff (1971b:295-96), seems to be fairly certainly that there is. It is worthy of note, however, that there are other types of sentence which have a similar reading, and it is unclear how they might be related. Thus compare (10a) with the following:

(9.18) Without exception the girls came.

And we may also note that without exception has a freedom of movement similar to that for all, e.g.:

(9.19) The girls without exception came

In favour of Anderson's proposal we ought to observe that, given the nature of the lexical items involved, it is tempting to derive without exception from a double negative.

The second question is syntactic, or, and perhaps better, formal: is it possible to construct a derivational process which will generate the correct surface structures? In attempting to answer this question an important warning has to be uttered again, which is that since we have 'translated' Anderson's proposals from a 'case' framework into an 'NP VP' one, the remarks below are only valid with respect to the translated result. Thus they ought not to be seen as an immediate criticism of the original proposal, and, furthermore, they only have validity in so far as the translation is valid. But as long as we bear these points in mind the discussion below can be treated seriously on its own terms. Let us consider firstly the derivation from (14) to the surface structure of (16). When existential-lowering has taken place it appears that there will be two possible derived structures:

(9.20) a [S_{neg}[S some of the girls came]]

b [S some of the girls[VP_{neg} came]]

The first alternative would result if existential-lowering occurs after neg-placement, the second if the reverse ordering is correct. Similarly, application of existential-lowering to (13) could give either (20a) or:

(9.21) [S[_{NP} neg some of the girls]came]

If we continue with such alternatives, then we can obtain either of the following from (17):

- (9.22) a [S_{neg}[S_{neg}[S_Ssome of the girls came]]
 b [S[_{NP}neg some of the girls][_{VP}neg
 came]]

But it ought to be clear that the availability of alternatives is spurious, for there are at least three arguments against the ordering of existential-lowering before the placement of a lower neg. Firstly, the constraints on the occurrence of (negated) quantifiers in surface VP's which we pointed out in §8.4 are only fully explicable if, as was reasonably assumed, neg-placement is ordered before existential-lowering if, as is the case there, the neg is lower than the quantifier; otherwise global rules will be needed. Secondly, we should surely assume a normal cyclical patterning of rules, and thus if the quantifier is higher than the neg, the neg is lowered into the matrix sentence and then correctly placed in preverbal position before we move on to the next higher sentence, when existential-lowering takes place. The third argument is the most obvious: if existential-lowering is ordered before neg-placement in the derivation of (14), then the surface sentences (15) and (16) will have the same structure at the point immediately after existential-lowering, cf. above. As this would be intolerable, existential-lowering must be ordered after neg-placement in the derivation from (14). Of course, this does not apply in the case of (13),

which will pass through (20a) on the way to (21). The key point is that (20a) is not an intermediate stage for (14).

At first sight the fact that existential-lowering follows neg-placement in cases where the neg is lower than the quantifier appears to be detrimental to Anderson's argument, since he wishes to derive all from a double neg-incorporation into some, i.e., it appears as if he wishes to adopt the following sequence:

(9.23) neg neg some > neg none > all

In order to accomplish this it would appear that (22a) is preferable to (22b), but (22a) involves the ordering which derives (20a) from (14), and therefore it must be rejected. This is not at all the case however; indeed we shall see in a moment that it gains strength from the forced ordering. Following Klima (1964:280) we can claim that the quantifier in (22b) is 'indefinite' and will be realised as any, but cf. §10.2. Klima shows that there must be a neg-incorporation rule, for otherwise the following pair will remain unexplained:

(9.24) a *Any snow didn't fall

b No snow fell

If we consider the stage immediately before (22b) we find:

(9.25) [_Sneg[_Ssome of the girls neg came]]

This is exactly the environment in which Klima's 'indefinite' and neg-incorporation rules apply, and thus the

next step is not (22b) at all, but rather:

(9.26) [_Sneg [_Snone of the girls came]]

The neg is lowered into the matrix sentence:

(9.27) [_Sneg none of the girls came]

There seems no reason why we should not apply at this point the variation of Klima's incorporation rule which concerns neg + 'indefinite' quantifier sequences. Only two modifications are necessary: the first is that the rule must apply obligatorily in such cases; the second is that the rule will convert no/none to all, rather than any to no/none.² We shall then have generated the desired surface structure for (10a). The non-occurrence of an overt existential sentence with all in the complement can be explained by the impossibility of Neg Transportation over two sentence boundaries, which is what would be necessary. The highest neg can only be lowered into the existential sentence if that remains, and thus cannot participate in the double incorporation which is necessary to generate all.

² There is a good case for claiming that neg-incorporation applies only after both neg's have been lowered and that it then applies from left to right; this makes it easier to derive (10b), for example. However, I shall attempt to abide by what I take to be Anderson's position, and if we have to reject it in favour of the proposal noted here, this will not affect the argument seriously.

The above account explains several of the puzzling features of all as well as giving a clear semantic description of the quantifier, but if it is taken no further then we shall still be unable to account for the occurrences of all in postnominal position. This objection is observed by Anderson (1973b), where he shows that such distributional facts are compatible with his proposal in Anderson (1973c). To see how this is the case we need a slightly more detailed version of (25):

(9.28) [_Sneg[_Ssome EXIST[_Ssome of the girls
neg came]]]

Let us now suppose the direction of neg-incorporation is reversible; then we can generate:

(9.29) [_Sneg[_Ssome EXIST[_Sthe girls none
came]]]

The EXIST is deleted and neg and some lowered, but in this instance they must be lowered into postnominal position, and thus the surface form (30) is derived:

(9.30) The girls all came

Such a hypothesis is in fact able to show that the distribution of all is determined by two factors.

Anderson (1973c) explains why all can appear in prenominal position; Anderson (1973b) shows, although we have not gone into this in detail, that all may also appear in every position where a verbal negator is grammatical. This explanation is also able to account for the unacceptability of (31) as opposed to (30), for in (31) the

position of all is neither a product of the usual position for a quantifier nor of the usual position for a negator:

(9.31) *The men kissed the girls all

One point which we have not yet discussed is the ability of all to be directly preceded by an overt negative. Of course this only occurs in subject position, but that is in any case explicable in terms of an extension of the thesis presented in §8.4.³ Leaving that point aside, we find:

(9.32) Not all the girls came

It is rather difficult to express Anderson's hypothesis (1973c) concerning this phenomenon in an 'NP VP' grammar, but as far as I can tell it would involve a third negator which would not be restricted in scope either to the existential sentence or to the matrix sentence, but might best be associated with a higher performative verb, cf. Ross (1970). It seems fair to object to Anderson's account here in two respects. Firstly, it is not at all certain that his hypothesis can be adapted to give a plausible structure in 'NP VP' terms, for there seems to be just too many negatives present. In order

³ Thus:

(i) *John bought not all the arrows
is worse than (8.92):

??John bought not many arrows

to constrain incorrect hypotheses it may be necessary to adopt ad hoc procedures which assign special scope to, and prevent deletion or absorption of, the third neg element. Secondly, if Anderson's hypothesis is accepted then it will not be possible to generalise the negation process found with compound existentials to the universal quantifiers. This point may well be more important than the first, for that may only be a product of our own inadequacy or the inadequacy of the 'NP VP' theory. But taken together these two factors cast some doubt upon Anderson's proposals.

Another difficult point in regard to Anderson's thesis is the non-occurrence of sentences such as:

(9.33) *The men many kissed the girl

The ungrammaticality of (33) is in fact noted by Anderson (1973b:26), and his explanation is that it is due to the lack of a lower negation in such structures. But it is quite simple to construct a structure with such negation:

(9.34) Not many of the men didn't kiss the
girl

Unless we place some ad hoc restriction on the convertibility of the underlying structure of (34) to a movable quantifier associated with many it is hardly possible to account for the ungrammaticality of (33). Note in particular that what we have to explain is the lack of a quantifier parallel to many in the same way as all is

parallel to some; an ad hoc restriction suggests that this lack is chance, whereas it is certainly (in as much as we can be certain of anything) syntactic. But our conclusion here may be too harsh, for as we were able to show in §§8.3 - 8.4, the negation on many is not associated with the higher existential sentence but rather with the quantity-referring coordinate partner. But how far this distinction can be exploited is open to reasonable doubt, see also our further remarks in §9.5.

Even in the face of these objections it is quite fair to state that Anderson's hypothesis has considerable attraction. There are two important justifications for this statement. First of all, his hypothesis enables us to relate the universal and existential quantifiers in a revealing manner, and the underlying structures for the two types of quantifier are by no means so distinct that they will have to be considered as belonging to two quite different grammatical categories. Secondly, if Anderson's claims are accepted then the fact that (some of) the universal quantifiers have a relatively free distribution (relative in comparison to that afforded to the existentials) will be explicable in terms of the dependence of the universals on underlying negatives and the distribution of negatives in surface structure. And that is obviously preferable to the type of solution offered by Carden (1968) or Dougherty (1970), where the mobility of the universal quantifiers is an unexplained

property of those quantifiers. Carden certainly offers no explanation; Dougherty's position is slightly preferable since his phrase structure rules, cf. §9.1, do assign different underlying properties to the universal quantifiers than are assigned to the existentials, but it is not so clear that the differences can be assigned on any principle other than that there are surface structure distribution differences, and therefore, of course, the argument tends to a vicious circle.

9.3 Generating generics

A curious omission in the discussion of all in Anderson (1973b, 1973c), which is shared by Carden (1968), is that almost no reference is made in any of these works to collocations of all with 'indefinite' NP's, i.e., constructions of the type exemplified by:

(9.35) All children like cream

One reason for this reluctance to discuss the behaviour of all in such contexts may well be that there is wide variation between speakers, but since that variation could give us some vital clues about the underlying structure of all, it is necessary to examine such collocations. In order to avoid confusion we shall chiefly be concerned with the type of English spoken by the present writer, in which all is ungrammatical in collocations with an 'indefinite' NP under certain circumstances which we shall specify later. Thus I find the

following sentence ungrammatical, or, at the very best, a stylistic variation approaching 'telegraphese' (in which, we should note, the is regularly dropped) which I would attempt to avoid:

(9.36) ?*All boys have kissed the girls
Even those speakers who have a greater degree of toleration for (36) will agree that (37) is much more acceptable:

(9.37) All the boys have kissed the girls
The difference in acceptability between the two sentences can only be accounted for in terms of absence versus presence of the.

The question we have to ask ourselves is why is it that collocations with all are affected, when this is not the case with other quantifiers such as some? Thus we find:

(9.38) Some boys have kissed the girls
What may be even more puzzling, but a point which we shall leave until §9.5 in the hope that it does not affect the validity of the immediate argument, is that substitution by every or each in (36) improves the acceptability:

(9.39) a Every boy has kissed the girls
 b Each boy has kissed the girls
We can thus observe that although the occurrence of unacceptable forms with all is only found in collocations with 'indefinite' as opposed to 'definite' NP's,

it must be the case that the unacceptability (such as it is) of examples such as (36) is due to the use of all in contrast to some other quantifier. But there is considerable evidence that this latter phrase is incorrectly stated. If we look at 'indefinite' (plural) NP's without any quantifier we find the following pattern:

- (9.40) a Boys kiss the girls
b Boys kissed the girls
c Boys are kissing the girls
d Boys have kissed the girls
e Boys have been kissing the girls

Although all the sentences in (40) are grammatical, the normal⁴ interpretation of the latter four is rather different from that for the first in two significant respects. Firstly, only (40a) is truly generic in the sense discussed in §8.1, that is, the sentence is timeless and at the moment of utterance there need not be any objects existing which satisfy the reference of boys. In contrast, in (40c), for example, there must be such objects existing at the moment of utterance, and the same is true of the other sentences, with suitable modification of the time reference. Secondly, the

⁴ It is important to stress this, since it is possible to place reverse interpretations on (40a) and (40b), although probably not in the other cases. See the discussion below on this point.

potential reference of boys in (40a) is to the full potential set of referents (which, of course, may be restricted in size by discourse conventions or even overt grammatical markers); on the other hand, in (40b) - (40e) reference is usually only to a partial set.

There is a simple test for at least the first of these points, for as is pointed out by Anderson (1973a: 481), the presence of an overt existential precludes a generic interpretation; apart from that, there is no reason to suppose that an overt existential sentence should alter the acceptability or interpretation of the sentences in (40), but cf. §7.4. Let us therefore insert an overt existential sentence in each of the examples of (40) and observe the results:

- (9.41) a There are boys (who) kiss the girls
b There were boys (who) kissed the girls
c There are boys (who are) kissing the girls
d There are boys (who) have kissed the girls
e There

are	}	boys	who have been
have been			

 kissing the girls

Examples (41b) - (41e) are clearly of the same status, and each can be related to its partner in (40), without any significant change in meaning. Thus we can indeed

assert that (40b) - (40e) do not have a generic interpretation, at least normally and in the sense of generic used above. Furthermore, the near-synonymy of, for example, (40d) and (41d), and their close semantic relation to a sentence such as:

(9.42) There are some boys have kissed the
girls

suggests that (40b) - (40e) and (41b) - (41e) should be derived from underlying structures very similar to (7.94). This would give the most probable explanation of why in these sentences the reference of boys is normally taken to be only to a partial set.

But (41a) provides a contrast to the above, for it has a reading clearly different from the normal reading of (40a): whereas (40a) is, as we have said, truly generic and the reference of boys in that sentence is to a full potential set of referents, (41a) is much closer in meaning and status to the other sentences of (41). To clarify this claim let us consider another pair of sentences analogous to (40a) and (41a):

- (9.43) a Elephants live only in Africa
b There are elephants (which) live
only in Africa

(43a) is true if and only if all elephants live in Africa and nowhere else; (43b) is true if there are some elephants which live in Africa and nowhere else. As the truth conditions for (43a) are different from those for

(43b), they undoubtedly have different meanings. Thus in the present time in the present world (43a) is false, (43b) is true. We may therefore conclude that (43a), and hence (40a), is not paraphrasable by an overt existential structure, and therefore it seems improbable that either (43a) or (40a) has an underlying existential source.

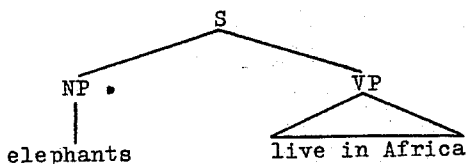
It is now possible to sketch out an explanation of the different interpretations of the sentences discussed above. An unquantified 'plural 'indefinite' NP must have two possible sources: the first source is one where there is an underlying higher existential sentence, the second is one where there is no such sentence; the first of these sources is connected to the normal interpretation of (40b) - (40e), (41) and (43b), the second to (40a) and (43a). These two sources are not equally available, rather they are restricted by the absence or presence of a tensed predicate. This is open to a quite natural explanation. If there is a higher existential sentence then the associated NP is specified, as can be verified by noting the possibility of immediately subsequent anaphoric reference, cf. Anderson (1973b:481), McIntosh (1968) and the discussion of 'individualising' and 'classifying' a in §§1.5 and 2.3. Now the function of a tense marker is to similarly specify the predicate, that is to say, a tense marker places the action denoted by the predicate at a specific point in time which is

(or was) existent. It is therefore to be expected that a combination of a specified subject with a specified predicate will be grammatical. But if the second source underlies the NP, there will be no specification of that NP and there will be a combination of an unspecified NP with a specified predicate, which, it seems plausible to argue, will be ungrammatical. On the other hand, if the predicate is not tensed then the action will be unspecified, and just as a specified predicate demands a specified subject, so an unspecified predicate will demand an unspecified subject.

As Anderson (1973a) points out, an untensed predicate has only two realisations in English, namely the 'simple' present and the 'simple' past; all predicates containing aspectual markers are tensed. From this it can be seen that boys in (40c) - (40e) can only be derived from an existential source. On the other hand, in (40a) and (40b) the verb may be either tensed or untensed, and therefore boys may be derived from either of the above sources and the sentences are properly ambiguous. In actual practice, however, cf. note 4, the present tense is normally taken to be untensed, the past to be tensed, but why this is so is not our present concern. The ambiguity of the two simple tenses explains why all the sentences of (41) are grammatical: if only an untensed source were available for them, then (41a) and (41b) would be ungrammatical.

The question which we must now consider is what kind of underlying structure is appropriate for a noun, such as elephants in (43a), which is unambiguously derived from a structure which does not contain a higher existential sentence. The first possibility is that the relevant part of the underlying structure contains only elephants:⁵

(9.44)



However if we accept that then we shall have lost the generalisation, already established, that NP's are derived from an underlying predicate nominal. Of course there is no a priori reason why the generalisation ought to be extended to what are undeniably exceptional NP's, and therefore this is not a crucial factor. But as Anderson (1973a:484) observes, the untensed occurrence of a past tense in a sentence such as:

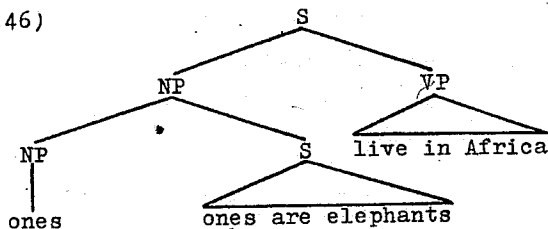
(9.45) The dodo was a bird

must be explained in terms of a 'pastness' feature attached to dodo. The most obvious move in terms of the

⁵ We shall ignore here the status of only, despite the fact that it is crucial to the existence of only one reading for (43a). Its relevance to the present discussion is purely clarificatory.

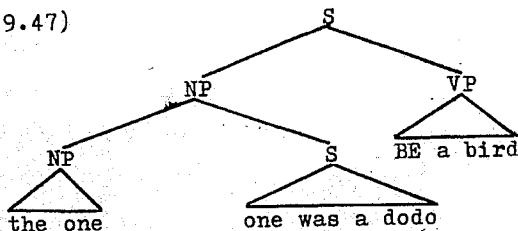
type of grammar which we are using is to derive elephants (or the dodo) from a predicate nominal, which will allow a [+past] marker to be placed on the copula of the lower S. And so, following Bach's (1968) formulation, discussed in §7.2, the underlying structure of (43a) ought to resemble:

(9.46)



In the case of (45) apparently only a slight modification of this is necessary:

(9.47)



Although (46) and (47) provide what are probably the most simple solutions to the facts as we have described them, which implies that they must be considered as serious candidates, further investigation reveals certain inadequacies which entail that they will have to be rejected. Firstly, contrary to the remarks of §7.4, where it was claimed that the complement of an underlying existential predicate had to contain an existential

quantifier-noun, it now seems to be the case that certain quantifier-less 'indefinite' NP's can occur in such a position. Thus we are now able to see that those earlier claims were to some extent an over-reaction to the more traditional claim that any 'indefinite' NP can occur as the complement of an existential sentence. But the result of this modification of our position is that there is now apparently no reason to suppose that a higher existential sentence could not have as its complement the NP of (46) containing ones. Yet this is in contradiction of the facts, and therefore it looks as if there will have to be an ad hoc blocking of such a structure.

The same problem does not, of course, arise in (47), for the potential complement of an existential sentence is then a 'definite' NP, and there is clearly no need to modify our original position in that regard. But the 'definiteness' of the one in (47) spotlights the second inadequacy, for in what sense is it correct, even allowing for the vagueness of the terms, to say that elephants in (43a) is 'indefinite' and that the dodo in (45) is 'definite'? Surely that description is only correct in terms of the surface absence/presence of the? Even without going too deeply into the analysis of instances of generic the, as exemplified in (45), it does appear to be correct to state that its function is semantically distinct from that of nongeneric the, most

especially in that it does not indicate reference to some object known to the speaker and presumed by him to be known to the hearer. Yet this is exactly the implication of (47), as can be confirmed by comparing that structure with the underlying structure for nongeneric 'definite' NP's proposed by Bach (1968), see again §7.2. Furthermore, the radical distinction between (46) and (47) which we are now able to pinpoint can be seen as intending to account for a rather trivial surface difference, for compare with (45):

(9.48) A dodo was a bird

(9.49) Dodos were birds

Whatever the differences between the three sentences, and we might suggest tentatively that the choice of type of NP is a reflection of a particular emphasis or point of view, it is quite certain that (48) and (49) are not to be distinguished from (45) by the semantic features associated with nongeneric instances of the.

A more adequate structure than so far proposed must, therefore, explain in a non-ad hoc manner the ungrammaticality of an underlying existential in (43a) and also the presence in (45) of a the which does not indicate what we may telegraphically describe as 'given reference'. There is yet a third point which it must clarify and which we have not fully discussed so far: this is that in the examples under discussion reference is properly to a class (of elephants, dodos, etc.).

This of course is precisely what a grammarian such as Krusinga (1932a:238, 315) implies when he refers to a 'classifying' use of a and the, see again §1.5. Obviously the structures proposed above give no hint that this is the type of reference present in the sentences they purport to analyse.

In attempting to construct an underlying representation which will fulfil these three conditions of adequacy and also retain the generalisation that nouns are derived from predicate nominals, we shall have to, for the sake of simplicity in argument, make certain assumptions, or at best ignore certain surface factors; this is because the necessary steps can only be justified after we have hypothesised a plausible structure and shown that this structure itself explains the factors that we have apparently ignored. The assumptions which we shall make are that (45) and (49), for example, are derived from virtually identical underlying structures and that (48) is derived from some rather different structure. The latter point especially will require some patience on the part of the reader, since we shall not discuss it further in this chapter; it is to be hoped that that patience will eventually be adequately rewarded in §11.4. One other point is also worth mentioning: in the discussion immediately below we shall suggest that there is an open choice between two transformations which are in any case optional. It is by no

means certain that the resultant nonequivalent surface structures (syntactically speaking) are semantically equivalent. This apparently implies that we accept the possibility that transformations change meaning and that underlying representations do not uniquely determine semantic relations, which is in contrast to the position held throughout the other parts of this work. But this is not necessarily so, for the suggestion of meaning-changing transformations is, at least here, better seen as the result of two inadequacies on the part of the author: (i) an inability to determine exactly the semantic relation between two surface structures; (ii) an inability to motivate slightly different underlying structures on the admittedly inadequately perceived semantic differences that do exist. If these two contingent - not necessary - facts were overcome, then no meaning-changing transformations would be necessary.

The simplest alternative to the type of structures exemplified by (46) and (47) will be one that replaces the rather suspicious occurrences of ones and the one by some explicit marker of the type of reference, i.e., class reference, with which we are concerned. The kind of structure for which we are searching for the class-referring quantified NP's is therefore likely to

approximate to one of the following two sketches:⁶

(9.50) [NP^A SET[S^A SET BE N]]

(9.51) [NP^{THE} SET[S^{THE} SET BE N]]

However even a very quick glance at the semantics of (50) will tell us that it is incorrect, for it would imply that the underlying structure of, say, (49) would be:

(9.52) [S[NP^A SET[S^A SET WAS dodos]][VP^{BE}
birds]]

and (52) ought to generate at least the following sentence:

(9.53) A set of dodos were birds

Whatever the meaning of (53) may be, it is quite different from that of (49), and therefore it ought to be rejected. On the other hand, (51) seems a much more plausible candidate, as can be seen by comparing (54) with (49):

(9.54) The set of dodos were birds

⁶ There is no conscious significance in the choice of the word set rather than class. It is very doubtful that our knowledge of semantics is sufficient to permit us to make such a fine choice at this stage. The only conscious criterion is that set has a well-known mathematical usage to which it may be convenient to refer. However this does not imply that we are using set in its proper mathematical sense.

The two sentences are not grossly dissimilar in meaning, despite the presence of the in (54). But observe that this the has an interesting source: it is used because the set referred to is unique by definition, not because the reference is 'given'. Thus an apt paraphrase of (54) might be:

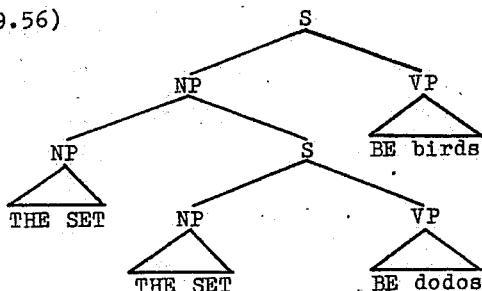
- (9.55) The set which contained all dodos and only dodos had the property of 'birdness'

It is indisputable that only one set can satisfy the description given in the relative clause. If we now compare (49) with (55) we can see that the latter is only making explicit a point which is implicitly contained in the former. How we ought to derive instances of this the-type is in detail mysterious, and all that can sensibly be said is that the the in (51), (54) and (55) is principally the marker of a uniquely defined set. If we may allow such a modest statement to suffice then it seems reasonable to continue with our argument, but see further, Chapter 12.

Since we have now, with certain reservations, been able to sketch out a possible candidate for the underlying structure of (45) and (49), we ought now to examine it in rather more detail in order to see if the correct surface structures can be derived from it. The underlying structure must be on the lines of (56) below, although certain features which are not wholly relevant,

such as tense marking, which occurs in the predicate nominal, cf. Anderson (1973a:485-88), are ignored:

(9.56)

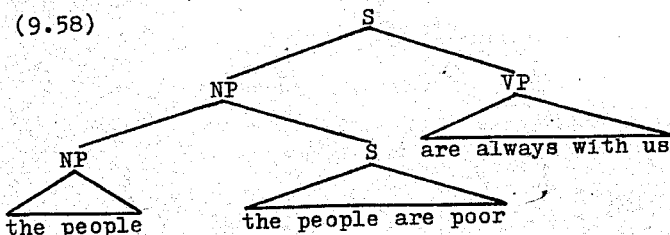


Obviously it is possible to derive (54) from such a source, but the question is whether or not it is possible to derive (45) and (49) from the same structure. What we must really decide, in other words, is whether or not it is possible to justify a transformational process from (54) to either of the other sentences under discussion. To obtain (49) it appears that we would have to delete THE SET; such deletion is always feasible, but we have to bear in mind the severe warnings uttered by Fiengo and Lasnik (1972). Nevertheless, two points suggest that the deletion is plausible. Firstly, we have already noted that THE SET is redundant in that it only makes explicit an otherwise implicit fact. Secondly, the structure resulting from deletion is controlled by another factor, namely the absence of a tensed predicate in the matrix of the highest sentence. Admittedly, it is true that many sentences are ambiguous, having both a generic and a nongeneric interpreta-

tion, but it is precisely the kind of deletion under discussion which is likely to create the ambiguity. Let us therefore accept that deletion of THE SET in the structure immediately underlying (54) - and below the surface insertion of of - is the correct source of (49). That deletion is of course optional.

Although there are some further remarks to be made on the relation between (49) and (54), and so on the transformation used to relate them, let us first turn our attention to the attempt to find a derivation for (45), so that the triple may be considered as a group. One possibility here would be to delete set and singularise dodos, but the only motivation for this seems to be the desire to generate the right surface structure. We must therefore look elsewhere. If we consider (56) once more, we are reminded that it assigns to the subject NP a structure similar to that for an NP containing a restrictive adjective. Thus at some stage of derivation the structure of:

(9.57) The poor people are always with us
corresponds to:



We then find adjectivalisation of the restrictive clause and after that it is possible, under ill-understood conditions, to obtain:

(9.59) The poor are always with us

Corresponding to this it seems possible that (56) could give an intermediate structure corresponding to:

(9.60) The dodo set was a bird

Then, as in (59), set is deleted, to give (45). The major point which we have omitted in this argument is the question of number agreement, and this has two facets. Firstly, and more simply, the number of the VP is determined completely by the number of the subject NP; this is why we find the was a bird/were birds variation. Secondly, why is it that dodo in (45) is singular? This is surely to be explained by its adjectival quality: the lower NP BE does is in fact an adjectival modifier of the singular noun set, and it is that noun which controls the concord, for it is still represented at the surface by the, cf. (49), and in any case its deletion must be very late indeed. Nevertheless it has to be conceded that this triple of sentences displays characteristics of number concord which are only poorly understood, cf. Morgan (1972) for a discussion of further problems raised by concord rules.

It seems highly unlikely that anyone, and this is especially true of the present writer, would wish to claim that the above derivations are wholly adequate;

indeed I would not claim that they do anymore than take a couple of paces nearer the kind of solution which would be fully acceptable. But they do work after a fashion and they also reflect certain semantic intuitions respecting the triple of sentences under discussion. Thus, it is certainly the case that (54) is the most explicit of the sentences (some speakers may find it over-explicit and thus clumsy); this accords with the fact that it preserves the maximum of underlying structure, including, as was shown in §§7.4 - 7.5, a marker of underlying subordination. In (49) there is no explicit statement that reference is in terms of a uniquely defined set, and because of this sentences like it are often interpreted as statements about a tendency, although naturally this does not happen in all-or-nothing cases like (49). The interpretation that the sentence is a remark about a set having a tendency to have a property is in accord with the deletion mechanism which we have proposed. Turning now to (45), it is more like (54) than (49), for it clearly is a statement about a certain set having a certain property - there is no question of it being a tendency which is being described. This too is explained by the derivation we have suggested, for that makes it clear that the focus of attention is kept on set, even although that noun is itself deleted. Furthermore, we can explain why sentences with a 'definite' plural NP, such as (61), are not generic, but

rather descriptive:⁷

(9.61) The dodos were birds

The reason is that if such a sentence were derived from the generic structure (56), then the highest NP would be the sets. But that NP is uniquely defined if it is generic, and therefore it cannot be other than singular, just as proper nouns cannot be other than singular. Thus, if our hypothesis is accepted, a truly generic interpretation of (61) is impossible, because of the contradictory demands on the underlying subject NP.

9.4 The emphasis on 'all'

However adequate the above discussion has been, it is only natural that we should now ask what relevance it has for the analysis of all. The answer to this can be found by comparing the sentences in (40) with those below:

- (9.62) a All boys kiss the girls
b ??All boys kissed the girls
c ?*All boys are kissing the girls
d-?*All boys have kissed the girls
e ?*All boys have been kissing the girls

Leaving aside for the moment the dialect variation which we mentioned briefly at the beginning of the previous section, we may observe that at least in some dialects

⁷ But see here §4.1.

the insertion of all is fully grammatical only when the normal interpretation of the sentence in question is generic; conversely, when a generic interpretation is impossible insertion of all is either very bad or impossible. The most obvious interpretation of these facts is that all may only collocate with an 'indefinite' NP if that NP is generic and that all has no influence on the grammaticality of a sentence other than as predicted by that condition.

Before we attempt to discover whether there is a possible analysis of all which might account for that condition, let us first examine an alternative hypothesis which appears to explain the facts exemplified by (62). Anderson (1973a:481) observes that an overt existential sentence precludes the possibility of a generic interpretation, and we saw in §9.3 that this is true even if the existential sentence is transformationally deleted. Anderson then extends this argument by suggesting that the generic interpretation of a sentence such as:

(9.63) All rhinoceroses eat small snakes
is possible just because the existence of a set (of rhinoceroses) which does not eat small snakes is denied. This analysis of (63) follows from the arguments presented in Anderson (1973c) which were discussed in §9.2, and the position is compatible with our own claim that generic interpretation is only possible if no specific

existence is predicated. Anderson (1973a) does not state whether his hypothesis will account for the badness of, for example, (62c), but clearly it is desirable that it should. Yet the explanation of the generic interpretation of (63) as due to the presence of a double negative appears to break down in such cases. Thus, although:

(9.64) No rhinoceros doesn't eat small snakes has at least a pseudo-generic interpretation, similar sequences where a generic interpretation is not possible are as grammatical as could be expected granted the presence of an overt double negative, which usually requires special intonation:

- (9.65) a No rhinoceros is not eating small snakes
b No rhinoceros has not eaten small snakes
c No rhinoceros has not been eating small snakes

Notice that the possibility of adding words or phrases such as yet and so far confirms the lack of a true generic interpretation. In the light of such examples it is difficult to see how Anderson's hypothesis can be accepted.

Let us therefore return to the first interpretation of the distribution exemplified in (62). If we ask ourselves what kind of item could be in specific structures

without altering the grammatical status of those structures, for in effect this is what happens in the case of all, the most likely answer is that such an item will be the product of emphasis. We have already had cause to remark, in §§3.4 and 4.4, that all is in some respects a marker of emphasis which is added to a generic plural NP, and therefore this answer fits in with the semantic facts that we have already discovered. It is probably the case, however, that the difference between a sentence with all and one without all, as, for example, between (63) and:

(9.66) Rhinoceroses eat small snakes

is slightly greater than would be predicted by mere emphasis, and therefore the structure we eventually propose must be able to explain this. And this is only one point where we shall have to tread carefully; there are many others. Perhaps the simplest, and yet the most important, is the fact that in both (63) and (66) emphatic stress can be placed on any item in the relevant surface NP: thus consider the following examples:

- (9.67) a Rhinoceroses eat small snakes
b All rhinoceroses eat small snakes
c All rhinoceroses eat small snakes

Then there are also other ways in which emphasis is marked, as for example by Clefting, and it seems probable that Pseudo-cleft and overt existential sentences mark emphasis by retaining a nearer-to-base form at the surface, cf. respectively Akmajian (1970) and §§7.4 -

7.5, above. In this context consider a pseudo-cleft sentence such as:

(9.68) What I spotted in the cellar was a rat
Despite the fact that there is a nonemphatic version:

(9.69) I spotted a rat in the cellar
there are emphatic variants of both the pseudo-cleft sentence and the derived form (69):

(9.70) a What I spotted in the cellar was
a rát

b I spotted a rát in the cellar

The four variations in (68) - (70) can only be accounted for if we assume that emphasis is introduced into English in at least two ways:⁸ (i) by heavily stressing the item on which emphasis is to be focussed; (ii) by preserving some deeper structure which is normally subject to transformational change but which if it remains will highlight the item on which emphasis is to be focussed. This being the case, there can be no objection in

⁸ Two further examples of marking emphasis are seen in clefted sentences, where a transformation induces a structure which highlights the item in question, and after which heavy stress can still be placed on that item. The fullest discussion of emphatic constructions such as these is in Jackendoff (1972b:229-78), but for another approach and further references see Lakoff (1971c:260-63).

principle to claiming that all is in one sense a marker of emphasis, despite the fact that in (67) all itself is subject to emphatic stress.

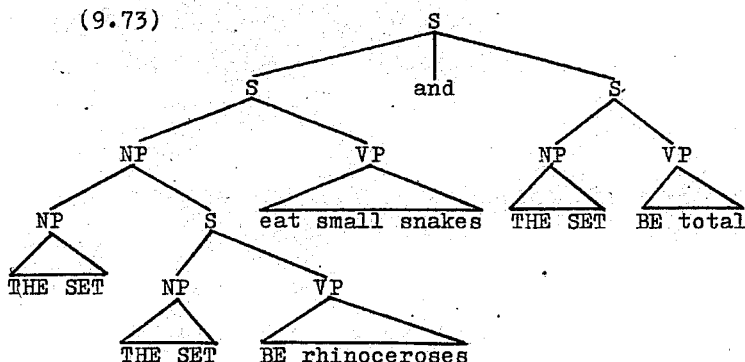
We demonstrated in the previous section that sentences like (66) are normally interpreted as describing a tendency on the part of the referents of the subject NP to display a given behaviour pattern, i.e., not all rhinoceroses need eat small snakes for (66) to be generally considered as valid. We have so far observed only two ways in which the speaker may commit himself to the assertion that every potential member of the set of referents has the property stated in the predicate: these are the constructions the + singular noun and the set of + plural noun. But the latter usage is commonly regarded as clumsy and the former has two disadvantages, for it is ambiguous - we might, for example, be talking of a specific rhinoceros - and it is often regarded as stylistically arch. We should now note that the emphatic stress variant (67) does not perform this task, for it only highlights the fact that it is rhinoceroses which are being talked about rather than some other group of carnivores. In this situation it is perhaps not unexpected that all should be called in to provide the necessary commitment. Further support for such a suggestion is found in the fact that all is ungrammatical in collocations with the emphatic variants so far discussed:

- (9.71) a *All the set of rhinoceroses eat(s)
small snakes
- b *All the rhinoceros eat(s) small
snakes

It is insufficient to explain the ungrammaticality of these sentences in terms of an unacceptable collocation with a singular noun, both in view of the derivations which we have proposed for the equivalent sentences without all and of examples such as:

- (9.72) All the cat family has/have claws

Although we have very little guidance about how all might satisfactorily be introduced into underlying structure, it does appear that a good way of doing so would be to introduce it as some adjectival modifier of set. One initial justification for this is the close relationship between, for example, all the world and the whole world. It seems probable that the modifier is to be considered as an addition, and thus perhaps as a non-restrictive clause. We may therefore propose the following tentative underlying structure for (63), in which the adjective is taken to be total (although there seem to be other equally good candidates, for instance complete):



That structure has one obvious advantage, for it will be possible to negate the coordinate partner and thus derive a negated form of all:

(9.74) Not all rhinoceroses eat small snakes
 The possibility of negation is confirmed by the fact that the structure of (73) is, except in one respect, identical to that proposed for compound existentials occurring in postdeterminer position, cf. (8.42) in §8.3, the important distinction being that the lexical items NUMBER and large (for many) are replaced by SET and total. There is one good consequence from this replacement, for total, unlike large, is not subject to comparison, because it is a maximiser, cf. Quirk et al (1972:446-48). This will explain why there are no comparative and superlative forms of all, in contrast to the compound existentials.

Despite these two advantages belonging to (73), we run into a rather serious problem in proposing that it

exemplifies the underlying structure of all. This is that if (73) is identical in node relationships to (8.42), as it is, why then does all not appear in post-determiner position? To make this clearer we need only consider the surface structures which result if lexicalisation to a surface quantifier does not take place. We shall find the parallel structures (8.41) - repeated here for convenience - from (8.42) and (75) from (73):

(8.41) The large number of boys kiss girls

(9.75) The total set of rhinoceroses eat
small snakes

Now the lexicalisation process for many in §8.3 related (8.41) to:

(8.40) The ~~many~~ boys kiss girls

Therefore should it not be the case that (75) is similarly related to:

(9.76) *The all rhinoceroses eat small snakes

One possible explanation for the ungrammaticality of (76) concerns the fact that the two transformations which have already been shown to raise rhinoceroses into a position analogous to that which it holds in (76) either delete the (along with set) or singularise rhinoceroses. If either one or other of these transformations is compulsory when raising takes place, then (76) will be ungrammatical because neither occurs there. We have already pointed out that a plural NP with the is not truly generic, cf. (59), and that would explain the compulsion. In (76) we would have a contradiction:

since all appears the sentence ought to be generic; since the ... rhinoceroses is a surface NP the sentence ought to be nongeneric. Thus the sentence is ungrammatical.

Then the generation of (63) might be achieved by deletion of the, but although that is just possible, it has very little appeal, since it has an ad hoc appearance. If this is the only way in which (63) and (75) can be related, ought we not to avoid relating them? That position may yet have to be adopted, but given the semantic relation which holds between the two sentences we must be reluctant to take that step. Since there is an alternative solution available let us consider it. It is to claim that the lexicalisation transformation to generate all involves a collapsing of the total set rather than total set without the. Indisputably the correct surface structures will be generated, but is there any justification for such a rule, which will otherwise be ad hoc, for the lexicalisation rules for the existential quantifiers never involve the? A first point is that it is essential to capture the fact that all is normally generic when in collocation with a plural NP without 'definite article'. So far we have not been able to do this, but if the is part of the underlying representation of all then we shall be able to do so. The reasoning behind this statement is that it is the sequence the set which marks out the generic

nature of the structures concerned. But in a sentence such as (63) the only sign of genericness is all, which is in contrast to, for example, (45). This would be explained if all has transformationally incorporated the set.

Secondly, it is clear that the the in generic contexts is unusual, even if the precise reason for this is uncertain. We have already noted some semantic reasons for this assertion, but there are also other facts which support it. Nongeneric the can be heavily stressed for emphasis, as in:

(9.77) The politician you can trust is
Gerald Ford

This is not the case with generic the:

(9.78) *The elephant which lives in Africa
has long ears

We have already observed, in §4.4, that it is quite possible that the ought to be derived from more than one source, and here we may have further evidence for this; whatever the precise solution may be, cf. Chapter 12, it does seem to be true that generic the is distinct from nongeneric the, although the degree of distinction may be in dispute. Thus the fact that nongeneric the does not form part of the source of existential quantifiers can hardly be claimed to be crucial evidence about the role of generic the in the derivation of all. The final point is very weak and therefore we can pass over it very

quickly. It is that we have noted that in some dialects and styles sentences such as (62d), which are nongeneric, are acceptable despite the presence of all. This may be accounted for by a partial falling together of all + N and all + the + N, but that suggestion, which is consistent with the facts of, for example, Dutch and German, is surely only reasonable if surface all + N is itself derived from a lexicalisation of a structure involving the. We might then be able to account for the introduction of such a phenomenon by means of analogical extension, but admittedly that is highly speculative.

It is therefore reasonable to conclude that there is some justification for supposing that all is the result of lexicalisation of the total set, and indeed we shall henceforth presume that this is the case; nevertheless two problems remain. One is that the above hypothesis is unable to explain the variability of position found with all; the other is that, although it permits negation of all, certain facts about that negation are unaccounted for. We shall examine the latter of these problems first, since it may provide a solution to the former. The interaction of negative elements with all is similar to the interaction between those elements and the existential quantifiers, cf. Carden (1968, 1970b, 1970c). The two cases are not identical, as was pointed out in §5.4, but they do resemble one another sufficiently for it to be desirable that our

hypothesis show that this is a consequence of similarity of underlying structure. This suggests that all ought to be derived from some kind of higher sentence. Another argument in favour of that suggestion is that it is without doubt that all displays most of the characteristics of a quantifier, cf. §9.1, and since all the other quantifiers we have discussed so far are derived from higher sentences, this is probably also the case with all. Even many is in all cases except where it is in postdeterminer position derived from a higher sentence.

If a higher sentence for all is the correct answer, then our arguments suggest that the most probable form of that sentence will be:

(9.79) The set BE total

But what kind of evidence is there that emphasis should ever be represented by a higher sentence? We can give indirect evidence at least, by means of the following argument. If Neg Transportation is agreed to be a (minor) rule of English grammar - which must be doubtful, cf. §5.4, then it will relate the pair of sentences below:

- (9.80) a Bill expects that not all the boys
will win a prize
- b Bill doesn't expect that all the
boys will win a prize

The explanation of this which is given by Carden (1968:

8-9) is very dubious, as we have commented previously, see too Jackendoff (1971b:287-96), but let us accept for the moment that Neg Transportation does apply here; if that is true all must be derived from a higher sentence. Now consider the following pairs:

- (9.81) a Bill expects that J^óhn won't win
a prize
b Bill doesn't expect that J^óhn will
win a prize
- (9.82) a Bill expects that John won't win
a prize
b Bill doesn't expect that John will
win a prize .

If the sentences in (80) are related by Neg Transportation, then the pairs immediately above can also be related by that same transformation, since they show the same semantic relation (which is not synonymy). But neither of the sentences in (81) has any reading identical to a reading of either of the sentences in (82), for in the former Bill does expect that someone will win a prize, in the latter he has no such expectation. This can be seen to be true by adding a further clause to, say, (82b):

- (9.83) Bill doesn't expect that John will
win a prize after all, because a
self-appointed committee has decided,
in the interests of democracy, that
all prizes will be abolished at once

Such an explanatory clause cannot be added to (81b). Now the possibility of Neg Transportation together with the meaning difference between (81) and (82) can both be explained by a theory that emphasis on John is a trace of an underlying higher sentence. Without worrying about the details of the structure, we can relate the pair in (81) to (84) and the pair in (82) to (85):

(9.84) Bill expects neg be John who will
win a prize

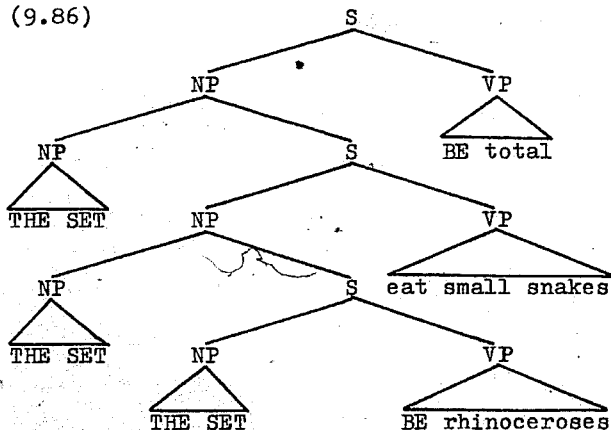
(9.85) Bill expects John neg win a prize

Even though there is absolutely no necessity to conclude from the above that Neg Transportation is a valid rule, we may yet permit ourselves to be convinced that surface emphasis is often the trace of an underlying higher sentence. Now it is interesting to note that the type of conclusion which we have reached concerning emphatic stress applies equally to all. Therefore it seems probable that all could profitably be derived from a higher sentence, given the consistency of the evidence we have already examined. Indeed, we shall see below that (81) provides even stronger support for this hypothesis, which in any case is previously supported by the parallels that do exist between all and the existential quantifiers. An alternative explanation of the contrast between (81) and (82) would rely on the theory of presuppositions. Thus Jackendoff (1972b) would explain the stress on John in (81) as due to the existence of a variable in the same position in the presupposition of,

say, (81a) as in the example itself. But since that analysis cannot be extended to the clearly similar case of all, it does not seem as useful as the suggestion above.

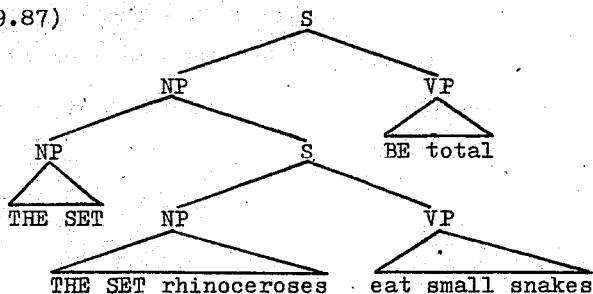
Since there is no reason why we should not accept (79) as the higher sentence source for all, the most likely underlying structure of (63) now appears to be:

(9.86)



Before quantifier-lowering (not existential-lowering since there is no EXIST in the higher sentence, and we may therefore accept different conditions on the lowering, as we shall see) and lexicalisation take place, but after all other relevant transformations, we shall find:

(9.87)

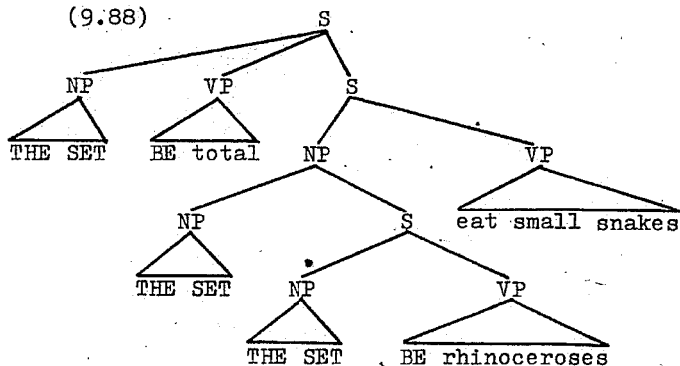


If there is no lexicalisation then (75) will be generated. Lexicalisation, however, converts the higher sentence into all and the lower occurrence of THE SET converts to all to permit quantifier-lowering, but see below, thus generating (63). We can thus claim to have provided a plausible underlying structure, in the shape of (86), for (63) and any other instance of all preceding an 'indefinite' NP. Furthermore, we have already noted that the higher sentence can be negated, and such will be the case in the underlying structure of, say, (74).

However it is not possible to claim that (86) is the correct underlying structure, for we still cannot explain why all has variable position; it is not sufficiently distinct from the underlying structure of, say, many, to do that. Let us therefore attempt a solution which will combine in some way the insights which (73) and (86) have attempted separately to explain, but unsuccessfully. We shall need to derive all from a higher sentence, as in the latter structure, but the NP

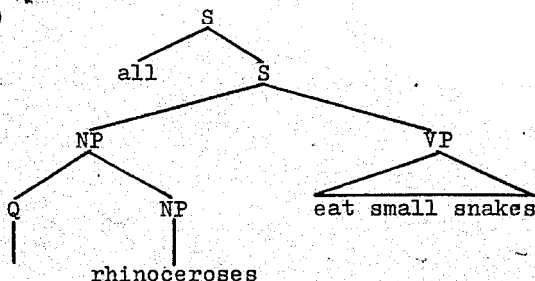
with which it collocates ought not to be embedded in all, following to some extent the former structure. To satisfy these requirements we need something like:

(9.88)



Let us now suppose that the relevant transformational rules are as follows and apply in this order: (i) raising of the predicate nominal; (ii) deletion of THE SET as described in §9.3 for the generation of (49); (iii) quantifier lexicalisation. In fact (ii) probably needs modified so that a quantifier node remains. This is to ensure that all is lowered into the correct position and it avoids problems of irrecoverability, briefly mentioned above. Thus immediately before lowering we find:

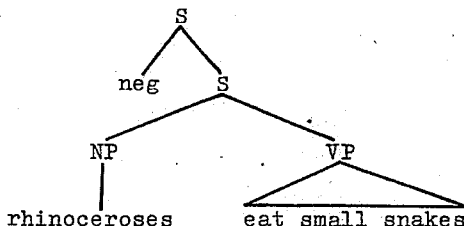
(9.89)



The interesting feature about (89) is that it clearly resembles the structure for (90) at the same stage:

(9.90) Rhinoceroses don't eat small snakes

(9.91)



The similarity of structure between all and neg which is thus hypothesised at this point suggests that it is far from unreasonable to presume that all might be lowered in two distinct ways. Firstly it might be lowered into prenominal position, thus onto the quantifier node created by the deletion of THE SET; secondly it might be lowered into the positions into which its structural twin neg may be lowered. If we distinguish between this transformation and neg-placement only by stating that the former does not require do-support, cf. Klima (1964: 256-57), whilst the latter does, we are then able to account for the variability of all-placement.

To justify the above claim we may note that in the pairs below all and not occur in exactly the same positions except if do-support has taken place; in those cases all occupies the position in which, it could be claimed, not would occur if there were no rule of do-support. In some of the pairs below all collocates with

a 'definite' NP; this is in order to give examples with aspectual markers present and it does not invalidate our claim:

- (9.92) a Boys all kiss girls
b Boys do not kiss girls
- (9.93) a The boys were all kissing the girls
b The boys were not kissing the girls
- (9.94) a The boys have all kissed the girls
b The boys have not kissed the girls
- (9.95) a The boys have all been kissing the girls
b The boys have not been kissing the girls

If all appears in a position other than one predicted above, then it is at best only marginally acceptable:

- (9.96) a ??The boys all were kissing the girls
b ??The boys all have kissed the girls
c *The boys have been all kissing the girls

The fact that (96c) is by far the worst of the above triple is explicable in the following way: in (96a) and (96b) all occupies the position held by not before it is correctly ordered in the VP, and therefore they are simply cases where all does not follow the tense-bearer, compare (92a); but in (96c) the position of all, were it grammatical, would have to be the result of a placement rule applicable only to it, and this is rather worse

than its failure to follow a rule only fully applicable to not.

The above proposal is also able to account for an apparent asymmetry in the distribution of all, namely that it does not appear postnominally when collocating with a surface NP which is not a subject; thus, assuming that all collocates with small snakes, (97) is ungrammatical:

(9.97) *Rhinoceroses eat small snakes all

In grammars where the position of all is handled by some independent quantifier-movement transformation, the ungrammaticality of (97) must be handled in an ad hoc fashion, cf. Carden (1968:21), Dougherty (1970:877-79), so that it does not apply to surface structure objects. But we are claiming that the surface position of all is in part a function of neg-placement rules. Now in §8.4 we were able to establish that neg-placement rules were subject to at least two constraints: firstly, that neg might not be moved leftwards over a verb; secondly, that neg might not be moved rightwards over a verb. Obviously these can be combined into a single constraint:

(9.98) Neg-placement rules may not move a neg over a main verb node.

Furthermore, there appears to be a generalisation from the distribution of not that neg-placement rules, i.e., movement rules other than lowering rules, may only move a neg into a VP. Given these restrictions on neg-

placement rules and our theory that all, after lowering, is moved by the same mechanisms as move neg, then it is quite simple to explain the ungrammaticality of (97). After all is lowered into the quantifier node we shall find a structure which is equivalent to the structure of:

(9.99) Rhinoceroses eat all small snakes

All cannot then be moved to the left since that would involve crossing a main verb node, nor can it be moved to the right since there is no VP to the right (and in the same sentence) into which it might be moved. Further, (99) cannot be synonymous with (63) since an all originally modifying rhinoceroses could not be moved into the position occupied in (99) because of the extension of constraint (98). The same fact excludes a derivation of (97) from (63). We can therefore explain the grammatical positions of all basically in terms of the neg-placement rules and the constraints upon such rules. The relation between all and its collocating NP in (92a) is not the same as the relation between all and its collocating NP in (97), despite the same surface relation of order which appears, and therefore it is not remarkable that the latter is ungrammatical while the former is grammatical. And of course we may finally add that we have been able to give an explanation here which is fully compatible with the hypothesis concerning the lowering of negated quantifiers into a VP and the lowering of non-negated quantifiers into a negated VP, as proposed in §8.4.

Although it would be possible to discuss the distribution of all more fully, and consequently refine the transformational process outlined above, our discussion of this quantifier has continued for some considerable time, and therefore it might be best if we were to look at only one more aspect of all before moving on to discuss the other universal quantifiers. This aspect is the interaction between all and negators. In fact the underlying structure proposed for all, namely that exemplified in (88), is sufficiently similar to that proposed for many to enable us to handle most interactions between all and a negator in the same way as we have handled the interactions between negators and compound existentials. The only cases we need discuss are those where the interaction between all and a negator proves to be different. Preeminent amongst these is the fact that for many speakers, cf. Carden (1970c), (100) has a reading - and it may be the only reading - where the neg must originally be higher than all:

- (9.100) All rhinoceroses don't eat small
snakes

In other words, (100) has a reading identical to the only reading of either (74) or (101), which are the predicted derivations from the underlying structures which we have sketched out, (74) being repeated here for convenience:

- (9.74) Not all rhinoceroses eat small snakes
(9.101) Rhinoceroses don't all eat small snakes

As we were able to observe in §8.4, similar ambiguity does not arise if we replace all by a compound existential. In Carden's (1970c) terms, (102) has only a neg-V reading:

(9.102) Many rhinoceroses don't eat small
snakes

The neg-Q reading of (100) is inexplicable in terms of quantifier-lowering alone, for we would expect the neg and all elements to reflect in their surface structure ordering the underlying command relation; this is achieved in both (74) and (101). (100) is surprising for two reasons: firstly, it splits up not and all, which might reasonably be expected to stay together; secondly, if surface precedence does not reflect underlying command relations then we might expect that another part of Lakoff's global constraint (1971c:244-46) could be used to predict that for the neg-Q reading to be obtained heavy stress ought to fall on not. But as was pointed out in §5.4, if all has heavy stress then only a neg-Q reading is possible for (100). Further, for some speakers this may be the only way to obtain such a reading; certainly it is the case that if not is heavily stressed a neg-V reading is obtained. But this is a reversal of Lakoff's constraint.

Luckily there is some rather good evidence to show that the puzzling characteristics of (100) only remain puzzling if we insist on regarding all as simply another

quantifier. If we approach the problem from another direction we shall encounter sentences such as (103), compare here the sentences in (81):

- (9.103) John didn't buy the new book on
Faroese phonology

This has exactly the same type of interpretation as does (100) when in that sentence there is heavy stress on all; the interpretation is that the neg was originally on the stressed item. This becomes quite clear if we apply Clefting to (103):

- (9.104) It wasn't John that bought the new
book on Faroese phonology

If (88) provides the basic underlying representation for (100), with the addition of a higher neg, then (105) ought to be generable too:

- (9.105) The set of rhinoceroses which eat
small snakes is not total

This is presumably related by Clefting to:

- (9.106) It is not the total set of rhino-
ceroses which eat small snakes

If quantifier lexicalisation has taken place, then (107) will be generated:

- (9.107) It is not all rhinoceroses which
eat small snakes

As far as our argument is concerned it matters very little whether or not (104) and (107) are any 'deeper' than (100) and (103); rather, their main interest is in

showing that the latter pair of structures can plausibly be expected to be derived from similar types of structure. This, of course, is not surprising, since we have claimed that each in its own way is the result of emphasis. Now, as the evidence of (81) showed, there is good reason to believe that in the underlying structure of (103) neg is in a higher sentence commanding John. But when lowering of neg takes place it has to be moved into the VP, since neg cannot occur within an NP. The surface position of not could now be the result of a wide choice of underlying structures, i.e., it could originally have negated, for example, John, buy, new or phonology; therefore heavily stressing not would do nothing to resolve this multiple ambiguity, unless it were constrained in some manner. In fact it does seem to be constrained in the most natural manner, namely not can only be heavily stressed if the negation was originally on the verb; in all other cases it is the item which was originally negated which is stressed. The only difference between instances involving items like John and those involving all is that in the latter case neg need not be moved into the VP; it can, as in (74), remain immediately before all. But if not is moved into the VP, the heavy stressing rule works exactly as if all were a marker of emphasis:

- (9.108) a All dogs don't chase cats
b Dogs don't chase all cats
c Dogs don't chase cats

d Dogs don't chase cats

In every case the interpretation is that the neg was originally on the stressed element. This explanation, if it is correct, can best be stated in terms of a global rule, cf. Lakoff (1970a), but for a competing solution see Jackendoff (1972b:352ff.). In view of the general tenor of our argument this is unfortunate, but it may be that global rules can be restricted to influence on stress patterns; thus consider the similar situation with regard to 'echo questions', cf. Katz and Postal (1964:111-12). Global rules would thus be more strictly constrained and so better defined than as in Lakoff (1970a).

We may conclude, however, that all ... not sequences can have for many speakers a neg-Q reading and that the likelihood of such a reading is increased when there is heavy stress on all is not to be explained, as both Carden (1970c) and Anderson (1973c:Appendix I) appear to believe, solely in terms of a higher quantifier analysis. The behaviour of all is, as we have seen, quite similar not only to that of neg but also to that of emphatic segments, and in those cases where all diverges from the 'normal' quantifier pattern, we can claim that this is because a pattern associated with either neg or emphasis is taking over from that associated with quantifiers. But of course this could only happen if the underlying structure of all were similar to that found for those

other items. Therefore the apparently idiosyncratic characteristics of all can be regarded as in fact the mingling of three very general patterns (of quantifiers, of negators and of emphasis), and this inexorably leads to an acceptance of a structure such as (88), which enables the mixture to be predicted, as (approximately) the correct underlying structure for all.

Since our hypothesis makes several claims about all which are perhaps rather novel, it is worth repeating them explicitly before we move away from all and towards the other universal quantifiers. Firstly, like the existential quantifiers, all is derived from a higher sentence. This permits us to account for the many points of grammar which the two types of quantifier have in common. Secondly, however, this sentence is more like an additional statement of emphasis 'on top of' a normal declarative. This explains why all does not greatly affect the acceptability of a sentence, as we discussed in §9.3. Thirdly, all, like many, includes an adjectival modifier of a quantifier-noun, and thus may be negated. But the fact that all is emphatic and thus must always come from a sentence higher than the matrix explains why all, in contrast to many, does not appear in postdeterminer position. Fourthly, the claim that the basic quantifier-noun underlying all is THE SET, rather than A NUMBER as with existential quantifiers, explains both the lack of existential sentence with all

and the strong (in some dialects, absolute) tendency for all to have a generic interpretation when collocating with 'indefinite' plural NP's. Fifthly, quantifier lexicalisation, a process common to all quantifiers, in this case produces a structure for all very similar to that for negatives, and this explains why all has variable position. Finally, although we have not discussed this point, it is clear that collocations of all with mass nouns, which are completely acceptable, present no special difficulty given the kind of derivation which we have suggested.

9.5 'Every' and 'each'

Without wishing to appear dogmatic, there does seem to be some grounds for claiming that our hypothesis about the underlying structure for all ought to be considered seriously. Therefore we may now permit ourselves to move on and examine the two other universal quantifiers which we must consider in this chapter: they are every and each. Of course there is a third quantifier which is indisputably a universal, and that is both, but it ought to be clear from the discussion in Chapter 4 that both should only be reconsidered once we have proposed a structure for partitive constructions, which will be one of the tasks of Chapter 10. Also in that same chapter we shall attempt to ascertain the status of quantifiers such as any, which has some claim

to be considered among the universals, but whose claim is far from indisputable. Thus every and each are the only remaining universals which are proper objects of enquiry in this chapter. There is no particular reason for looking at one before the other, so the decision to look first at every is quite arbitrary.

There appear to be three important differences between all and every for which we shall have to attempt to find an explanation and then relate that explanation to differences in the grammar. Perhaps the most obvious difference is that every collocates with singular nouns only. This is generally regarded as a purely surface fact which reflects the underlying distributive meaning of every, see, for example, our remarks in §3.4. The claim is that a semantic feature of distributivity is realised by a late transformation which changes the collocating noun from [+plural] to [-plural]. Although this is a simple, and thus intuitively appealing, explanation it is not clear that it can be used to explain the ungrammaticality of sentences such as those in (109) below, cf. Vendler (1967:72-76), from whom the first example is taken, and also Dougherty (1970:866-71), who, although he does not discuss every, leads the way to a number of interesting examples. We may also note that Carden (1968:13-14) argues for a number-changing rule:

(9.109) a ?*Every one of the blocks is similar

- b *Every one of the girls left the party together
- c *Every girl left the party simultaneously

The problem is that if the collocating NP is an underlying plural, then the ungrammaticality of (109) is only to be explained in one of two ways: either the requirement of be similar, together, etc. that the subject NP be plural is a surface requirement or some such feature as [+distributive] is attached to every and we suppose that the NP in question be required to be [-distributive]. However there is undeniable evidence that the first of these alternatives must be wrong, for compare with (109):

(9.110) *The scissors are similar
and the second alternative is clearly ad hoc. Thus the simple explanation may have to be rejected.

The second point of difference between every and all is that the former does not have the same freedom of movement as the latter; thus both the following sentences are ungrammatical, regardless of whether the collocating NP and subject - verb concord is singular or plural:

(9.111) a *Boy(s) every run(s) quickly

b *The boys have every won a prize

Since the sentences remain ungrammatical if one is inserted after every, which would parallel partitive

constructions with every, it has to be accepted that every only appears before the NP with which it collocates. We might note here that Carden (1968:18-19) claims that this is not true, for he suggests paradigms which give, for example:

- (9.112) a The boys every one of them run
b The boys hit the balls every one
of the balls

But these paradigms, which give a spurious air of generality to Carden's quantifier-movement rules, rely, it seems to me, on a quite invalid discounting of intonation pauses which show that in (112) we have examples of repetition, not quantifier-movement. Further, Carden's (1968:23-25) actual discussion of the importance of intonation pauses is totally inconclusive. The only possible type of counter-example is:

- (9.113) The soldiers every one advance

But here Carden (1968:16) does not make it clear whether or not there are pauses before and after every one. If there are, it is not a counter-example, and in any event it appears to be the case that most speakers reject (113) if there are no such pauses. Assuming that Carden's examples prove nothing, we can now observe that, since it also does not appear in postdeterminer position, every has the same distribution as some as far as surface ordering is concerned and leaving aside the question of negation. The restrictions on the distribution of every, could, of course, be for two reasons: it might

be because of number disagreement, which, as can be seen from the bracketing in (112a), is difficult to resolve. However, whatever the validity of (113) it does suggest that the problem is not impossible. More likely, therefore, is the hypothesis that every's distribution is more like that of some than that of all because its underlying structure resembles the underlying structure of the former more than it does that of the latter.

In contrast to the second point, that in one aspect every has a more restricted distribution than all, our third point is that in another aspect every has a much less restricted distribution than all. More specifically, whereas all can usually only collocate with the subject NP of a generic sentence, if that subject is 'indefinite', every is fully grammatical in nongeneric sentences; thus compare the sentences below with those in (62):

- (9.114) a ?Every boy kisses the girls
b Every boy kissed the girls
c Every boy is kissing the girls
d Every boy has kissed the girls
e Every boy has been kissing the girls

Whereas all was most likely to be unacceptable in sentences where no generic interpretation was possible, the only instance where every is at all dubious is (114a), where a generic interpretation is most probable. In

§3.4 we suggested that in the light of such complementary distribution it would be possible to regard every as a suppletive form of all, but tempting though that proposal may be it will have to be discarded in the light of the evidence obtained from examples such as those in (109). The semantic difference between the two quantifiers is such that it must be the product of more than a suppletion process. The answer appears to be that every behaves as if it were an existential quantifier rather than a universal quantifier like all. Thus, like the existential quantifiers, cf. §9.3 and Anderson (1973a), every demands a nongeneric interpretation. The appearance of suppletion is due to the complementary semantic characteristics of the two types of quantifier. Given the evidence of the second and third points it seems reasonable to adopt the following position: all is a 'true' universal quantifier, having the underlying structure approximately outlined in (88), and it therefore has variable surface structure position, etc.; every, on the other hand, is the result of some modification of the structure underlying existential quantifiers.

The phrase "some modification" is, of course, very vague, and therefore it may appear as if there were a great deal of work to be done before we can reach the correct hypothesis. But that is not so, for we have already discussed a proposal, which, at least in outline,

is consistent with the demands which the semantic and syntactic facts entail. This is the proposal made by Anderson (1973c), discussed in §9.2, that all is derived from a construction including the negation of a higher existential sentence and a lower negation of the matrix sentence. Without, at least for the moment, repeating either the arguments of that section or the transformational processes involved in the generation of the correct surface structures, it should be clear that if we claim that the surface quantifier which is generated is not all but every, then the principal objections made against Anderson's proposal will be met. One modification of his work is, however, necessary: as we observed, Anderson (1973b) suggested that a reversal of neg-incorporation, followed by a lowering of the higher quantifier onto the none which then appears in the VP, would account for the postnominal position of all. But every does not occur postnominally. To account for this we need only claim that this procedure does not in fact take place. This claim is intuitively satisfying since it dispenses with the only transformation in Anderson's hypothesis which is not clearly independently motivated and which thus may be suspected of being ad hoc. But as Anderson (1973c) has been able to show, the remainder of the transformational process is natural and justifiable. We may therefore claim that in modifying his hypothesis to generate every rather than all, we have both simplified and strengthened it.

But we still have to explain the first point discussed above, which, we were able to observe, was inadequately explained by the postulation of a late number-changing rule. Here again a modification of a proposal made by Anderson seems to give the correct results. In Anderson (1973b:28-29) it is proposed that a sentence such as:

(9.115) Each of the men has kissed the girl
has an underlying structure which we may abbreviate as:

(9.116) [neg one of the men[one of the men
has kissed the girl]]

Let us claim that that kind of structure is inappropriate for each; the reason for this is the same as that which makes analogous structures inappropriate for all, see below for some justification of our claim. On the other hand, it is much more appropriate for every. If we assume that one, like many, is a compound existential and its acceptability in postdeterminer position together with the possibility that it may be negated suggests that this is so, then only one problem remains: is the highest neg associated with the quantifier on the existential sentence or on the quantity-referring partner? But since a structure of the form:

(9.117) A number_i does not exist and
a number_i is one

is quite meaningless we can presume that the negation must be on the quantity-referring sentence. We may admit that if every had variable position then we would

now be unable to explain the lack of variable position for many in the manner suggested towards the end of §9.3, for the negation is at the same point for every as it is for many. But since every does not have variable position the problem does not arise.

We can now claim that the underlying structure of, for example, (114b) will be:

(9.118) [_S[_SA NUMBER EXIST[_Sneg[_SA NUMBER
kissed the girls[_SA NUMBER BE boys]]]]
and[_Sneg[_SA NUMBER BE one]]]

Following the proposal in note 3, all lowering rules apply before neg-incorporation, and we then obtain:

(9.119) neg one boy neg kissed the girls

Then neg-incorporation applies twice to generate the quantifier every and we have thus generated (114b). We can see that the sequence not one is preferable to not any, because in the latter case we could generate a plural NP; furthermore we saw in §3.2 that not one is always interpreted as less than one, i.e., zero, cf. Smith (1972). The above solution accounts for the semantic and syntactic characteristics of every in an adequate manner. The semantic point that the reference of every is the same as that of all, except for the generic/nongeneric contrast, is explained by the sequence not one ... not; the syntactic point that every behaves like an existential quantifier except that it cannot

appear as the complement of an overt existential sentence is explained by the constraints on the derivation from (118) to (114b) which Anderson (1973c) has demonstrated to be correct; and the further syntactic point that every always collocates with a singular countable noun is explained by deriving every from a structure including one, for one has that precise restriction. Thus the unacceptability of the sentences in (109) is paralleled by the unacceptability of those in (120), and it is to be explained in exactly the same manner:

- (9.120) a ?*One of the blocks is similar
b *One of the girls left the party
together
c *One girl left the party simultaneously

Let us now move on to a consideration of each, which can be distinguished from all in four important respects. Firstly, and most strikingly, each cannot be negated and it does not even seem to be very acceptable in a negated declarative sentence, although in interrogative sentences it is more acceptable:

- (9.121) a *Not each boy won a prize
b ??Each boy didn't win a prize
c ?Didn't each boy win a prize?

The gradience of acceptability here can be correlated to the availability of a neg-V reading: the more usual the neg-V reading the more acceptable the sentence. This

suggests that the only constraint is on direct negation of each. The second distinction is that each, like every, demands a singular NP:

- (9.122) a Each boy won a prize
b *Each boys won a prize

But in postnominal position the reverse is true, for the collocating NP is then plural:

- (9.123) a *The boy each won a prize
b The boys each won a prize

The third distinction is that each, although apparently associated with the subject NP, can appear after the object NP:

- (9.124) a The boys won a prize each
b *The boys won a prize all

It is relevant to note, however, that there seems to be a restriction on the object NP in this case, to the effect that it include an existential quantifier:

- (9.125) *The tasters sampled the wines each

Compare with (125):

- (9.126) a Each of the tasters sampled the
wines
b Each taster sampled the wines

We appear to have stumbled upon another problem here, for compare with (126):

- (9.127) *Each boy ate up the apples

It seems to be the case that if the object NP does not include an existential quantifier then there are two restrictions on each: (i) it cannot appear in sentence-

final position; (ii) it can only appear elsewhere if the verb does not denote an exhaustive action - thus sampled against *ate up. How such facts can be captured within the grammar is quite mysterious to the present writer.⁹

The fourth point of difference between each and all is that each regularly has a nongeneric interpretation. One example will suffice to demonstrate this:

(9.128) Each boy has won a prize

Since there is perfective aspect the sentence must be nongeneric, yet each here is acceptable, unlike all, compare:

(9.129) *All boys have won a prize

Strangely, however, the equivalent sentence with each in postnominal position is ungrammatical:

(9.130) *Boys have each won a prize

The contrast between (128) and (130) explains why we have silently introduced the in several of the examples above. Remarkable as the contrast may be, it is reminiscent of a contrast found with another quantifier. Thus we find the following parallel pair:

⁹ And we still have to explain:

(i) Each boy has eaten up his lunch

Note that here also each does not occur sentence-finally:

(ii) *The boys have eaten up their lunches each

The presence of anaphoric pronouns clearly complicates matters.

(9.131) a Both boys have won a prize

b *Boys have both won a prize

Similar examples were discussed in Chapter 4, where the following explanation was given: the surface quantifier both is derived by a lexicalisation rule (called Dual Copy) from a structure including a the originally dependent upon the collocating NP; but if the quantifier was moved into postnominal position, then the was left behind, there being a constraint that it could not cross its own NP. Obviously the same explanation potentially accounts for the ungrammaticality of (130). Therefore, just as both is derived from all the two, so must each be derived from all the, with some additional factor we have not yet discovered. However each differs from both in one very important respect: whereas both (in pre-nominal position) obligatorily has the-incorporation, each need not have incorporated a the. If it does, then it will be nongeneric, if it does not and yet there is no the present, it will be generic. Thus (132) is ambiguous because it may or may not be the result of the-incorporation:

(9.132) Each worm has five legs

But exactly as with both, if each is postnominal any incorporated the must remain behind in postnominal position, and this has the effect of resolving the

ambiguity.¹⁰ Therefore the (a) sentences below are generic, the (b) sentences are nongeneric:

- (9.133) a Worms each have five legs
b The worms each have five legs
(9.134) a Worms have five legs each
b The worms have five legs each

In this respect Garden's claim (1968:15) that each is 'definite' except in sentence-final position is seen to be quite incorrect. The great advantage of our explanation is that it enables us to account for the common nongeneric interpretation of sentences containing each without having to posit an underlying structure for each which is quite different from that for all, as the lack of genericness in (128) is explained by its collocation with a plural 'definite' NP, even though there is no surface the.

But even if we can now explain the fourth difference between each and all, the first three still remain unresolved. In this respect the observation made by Vendler (1967:78) that each "directs one's attention to the individuals as they appear" is extremely helpful. The neatest way to express this syntactically is to introduce an adverb like individually (singly seems

¹⁰ It is very probable that in those dialects where all may incorporate the, cf. §9.4, exactly the same rule will apply.

equally adequate for our purposes). Thus the generic interpretation of (132) may provisionally be considered as a variation of:

(9.135) Individually all worms have five
legs

The nongeneric interpretation, of course, would be provided for by the insertion of the after all. Since the adverb in (135) includes the quantifier within its scope we may now go on to propose that the underlying structure of each is exactly that of all, except that there is a yet higher sentence containing the adverb represented by individually. Each will be the product of the inclusion of the adverb into the incorporation process which we have described in the previous section for the generation of all. The above claim is bare and unjustified as it stands, but there are a number of facts which show that it has considerable plausibility.

Firstly we may note that most adverbs appear in the surface structure positions where each may occur, including, crucially, sentence-final position. If each is derived by incorporation of an adverb, then we can explain its grammaticality in that position, which otherwise would be an idiosyncratic characteristic of that quantifier. The parallelism with neg which is found with all, and which thus may be deduced to occur with each also, is insufficient as an explanation of this fact. Secondly we can now explain the impossibility

of negating each. If (315) is intelligible and represents in some fashion an intermediate stage in the derivation of each, then there are two possibilities for the underlying negation of each: the negation may either be upon all or be upon individually:

(9.136) a Individually not all worms have
five legs

b *Not individually all worms have
five legs

The interesting fact is that (136b) is unacceptable, although it is not completely certain how such structures are to be blocked. Whatever the exact explanation is, we can see that only (136a) will provide a potential source for the negation of each. Now there is in fact a realisation of (136a) involving each, namely:

(9.137) Not all worms have five legs each

Such a sentence, we may observe, can only be generated if we have assigned a partly adverbial status to each, since two quantifiers cannot cooccur (modify the same noun) unless they are of different types. Our proposal will hypothesise only one underlying quantifier-noun for (137), since each will be an optional lexicalisation of individually. However, the ungrammaticality of (138) - and thus (121a) - is still to be explained:

(9.138) *Not each worm has five legs

It seems most probable that such sentences are ungrammatical because lexicalisation of individually all only occurs if there is no intervening item, which is not the

case in (136a). In such cases the adverb is, as we have said, optionally converted to each. These instances of each must be regarded as pseudo-quantifier, for their adverbial status is clear and undeniable. It can be observed that (138) would also result from (136b) if that sentence were grammatical and it may be that, to avoid confusion between the two types of structure, one grammatical, the other not, the fact that in underlying structure the negation is grammatically possible only on the quantifier and never on the adverb must be overtly reflected at the surface. We now only have one distinction between all and each to explain, namely the number-changing rule which each induces in prenominal position. It looks as if this ought to be related to individually, but unfortunately no formalisation of the rule is obvious.

If our suggestions concerning the underlying structure of each afford at least a temporary solution to the problems surrounding that quantifier, and we can certainly claim that we have been able to account for a wide range of facts without having to introduce ad hoc or item-individual transformations such as the quantifier-movement rules of Carden (1968) and Dougherty (1970), then we have returned to the theme presented at the beginning of this chapter, namely that universal quantifiers are related to adverbs. But we are now looking at that theme in a new and more satisfactory

way, at least in terms of descriptive power, for we have been able to observe that universal quantifiers must be related to several other grammatical categories. The difficulties facing the grammarian when he attempts to analyse universal quantifiers are primarily the result of a quite understandable desire to view them as a unitary whole. Yet the relevant syntactic and semantic facts deny the possibility of the kind of unitary description which we were able to propose for existential quantifiers. This scarcely seems an inviting conclusion when we consider that existing hypotheses have claimed a common underlying structure for all (or most) quantifiers. But if we reexamine, for example, Carden (1968: 15-36), it is indisputable that all his movement transformations, which, unlike our transformations performing the same task, are quantifier-specific (that is, affect no other category) and furthermore they have to be restricted in application to various arbitrary subsets of quantifiers. Thus, although Carden's hypothesis looks homogeneous when we consider only the underlying structures, the derivational processes are quite different for each quantifier and so the hypothesis is in fact heterogeneous.

Were it not for one factor our proposals would be no better, for they too apparently display a heterogeneous character. This factor is that all our proposals are variations upon independently-justified

structures and transformations. Thus, in the case of all the underlying structure is related to two other structures: (i) the structure for existential quantifiers, in that a higher sentence containing a quantifier-noun is still necessary; (ii) the structure for emphatic sentences. The transformational process has similar relations to other parts of the grammar of English: (i) a quantifier-lowering transformation is necessary, which is similar to existential-lowering; (ii) this transformation induces a structural confusion with neg and thus all has features in common with not. Parallel facts hold for each, and the relation of every to the existential quantifiers is even clearer. Since both the idiosyncratic syntax of the universal quantifiers and their semantic roles support such proposals, there can be little doubt that we have at least pointed the way to an adequate hypothesis. The complexity of the grammar of the universal quantifiers is such, however, that considerably more work will have to be done before we can even start to hope that we might be approaching a definitive solution.

Chapter 10

Beyond the paradigm

10.1 The lumber room

In the first three chapters of Part III, we have concentrated our attention upon those quantifiers and those quantifier constructions which follow the patterns suggested by the first heuristic procedure of §7.1. As we observed at the time, however, not every quantifier shows a friendly attitude towards that device, and there is at least one quantifier construction, namely the partitive construction which has a quantifier as its head followed by a 'definite' NP, which appears to be quite separate in its grammar from any of the other relevant constructions. Therefore before we leave the field of undisputed quantifiers in order to examine, in Part IV, the 'articles', we must first discover whether these as yet unanalysed items and constructions are consistent with our general hypothesis or if they demand that some modification of our hypothesis, or perhaps even a quite different hypothesis, must be adopted.

Inevitably we are faced in this chapter with rather a rag-bag of items - this is the lumber room of the grammar to which Kruisinga refers - for the objects of study are precisely those which do not obviously fit into the established paradigms. Nevertheless, efforts

to impose some sort of order can be established in two directions: we can limit the number of items and constructions to be discussed and we can impose some sort of sequence upon our discussion. Despite the fact that the former of these is a blatant encouragement to avoid difficult problems and that the latter is not totally possible, we shall attempt to follow both courses; but the former especially requires some justification. If we continue to ignore, as we have done so far, potential candidates for discussion such as stressed some, a certain, several and enough, then we can hardly make a claim of comprehensiveness for our survey. But although comprehensiveness is a desirable aim there is no reason to suppose that it is a viable one. Although quantifiers form a 'closed-class' category, the range of behaviour within that category is extremely wide and as such it is not wholly amenable to grammatical analysis given both the present level of adequacy of grammatical theory and this writer's capabilities. We can use a metaphor from phonology here. At present the most we can hope to ascertain are the phonemes of quantifiers, and the allophones of those phonemes are as yet not fully determinable. Although the value of taxonomic phonemics is to be disputed in a sophisticated phonological theory, its usefulness at a more primitive level is certain. And we should not delude ourselves into believing that the study of syntax and semantics is beyond that more primitive level.

The equivalents of phonemes in this present study are the paradigmatic quantifiers and our concern must be, as it has been, to relate other quantifiers (allophones) to the paradigmatic models. Knowing so little about the paradigms it would be a luxury to examine quantifiers which, although interesting in themselves, have no obvious relation, even of a negative character, to the putative paradigms. Therefore we must rely on the certain grammatical facts and our less certain intuitions to tell us which quantifiers are not to be related directly to the paradigms which we have constructed, yet are most likely to show productive results if investigated in detail. The most probable are the pair any and no, but in restricting our further analysis to these two quantifiers we do not deny the necessity of investigating the other quantifiers mentioned above. After we have considered any and no we shall move on to examine the structure of quantifier partitive constructions, since all quantifiers (some with slight morphological changes) appear in such contexts as well as in the prenominal position upon which we have concentrated our attention. Finally we shall discuss a quantifier which, although it does not depart from the paradigms in a very radical manner, cannot be discussed properly without the evidence from partitive constructions being at hand.

10.2 Some reasons why there must be some 'some-any'
rule

The many-splendoured nature of the meaning of any is captured most concisely by Jespersen (1933:181) when he says that "Any indicates one or more, no matter which". This indeterminateness, so precisely expressed, suggests that it would be foolish to hope that the basic underlying structure of any might be determined on semantic grounds; rather, it would be most profitable to consider its syntactic characteristics firstly and so attempt to determine a plausible underlying structure. Fortunately, the syntactic behaviour of any has been quite extensively studied and indeed we have been able to make a few relevant observations at earlier points in this work, see especially §§2.3, 3.3 and 3.4. Therefore many of our remarks below need only be very brief.

It is well-known that any has only a restricted range of grammaticality, for it is unable to occur in a simple declarative sentence:

(10.1) a *Any boy is sitting on his desk

b *At the party I saw any boy

Although the above statement needs a certain amount of elaboration, the only point which we have to note immediately is that collocations with a plural or mass NP are equally ungrammatical. Whenever the sentence is not declarative, however, any is usually grammatical. The best-known examples involve negation:

(10.2) At the party I didn't see any boy
But in this case we may note that the status of the collocating NP has a marginal effect, in that (3a) and (3b) are both more acceptable than (2). The effect is definitely marginal, however, and (2) is grammatical:

- (10.3) a At the party I didn't see any boys
b At the party I didn't drink any milk

An element of confusion with regard to the status of any in negative contexts is introduced by the ungrammaticality of:

(10.4) *Any boy isn't sitting on his desk
However, this is explained by Klima (1964:280) as due to the failure of an obligatory rule of neg-incorporation into the preceding 'indefinite' quantifier. The operation of Klima's rule gives:

(10.5) No boy is sitting on his desk
We shall return to the problems posed by (4) below, but for the moment we need only note that the distinction between the types of sentence exemplified by (1) on the one hand and (2), (3) and (5) on the other is that in the former the existence of boys is asserted, in the latter such existence is not asserted. We might, of course, state that in the latter cases the existence is denied, but we shall see that this would be to miss an important generalisation, and, more crucially, such denial is not logically equivalent to an assertion not being made.

A second type of sentence in which any is grammatical is one where there is a certain type of modal; most usually a modal expressing possibility:¹

(10.6) a He can read any book

b Any boys can come to the party

Once again we may note that the existence of referents for the collocating noun is not asserted; it is not stated as a fact that, in the limited world to which the discourse is referring, there are books or boys. This must be associated with possibility in some way, but it is not simply a matter of possibility, for compare with (6):

(10.7) a He can read some books

b Some boys can come to the party

We shall see eventually that there is a relatively simple explanation of this, even if we ignore the possibility that the instances of can in (7) are not identical semantically with those in (6), but let us firstly consider other examples of grammatical occurrences of any. To some extent generic sentences are similar to 'possibility' modals, in that a generic sentence, cf. §9.3, is a statement about the potential value of some events

¹ For an illuminating discussion of modal verbs see Anderson (1971c), and for a further extension of the contexts in which any is grammatical see the remarks on modal operators in Jackendoff (1972b:279-300).

being performed by some potential object. Therefore it can hardly surprise us that any occurs in generic contexts, for there too no assertion of existence is made:

(10.8) Any acid will dissolve that rust

Now in §3.3 we noted with reference to remarks made by Bolinger (1960) that sentences of that type are at least closely related to conditionals:

(10.9) If something is an acid it will
dissolve that rust .

In both generics and conditionals there seems to be a preference for singular over plural count nouns; thus both (10a) and (10b) are less acceptable than either (8) or (9):

(10.10) a ??Any cars pollute the atmosphere

b ??If some objects are cars they
pollute the atmosphere

The explanation of this is obscure to this writer, but at least they serve to underline the relatedness of generic and conditional sentences.

The existence of conditional sentences containing if enables us to explain an otherwise puzzling instance of any. We observed above that any cannot occur in a simple declarative sentence, but this is not so if there is dependent upon the collocating NP a restrictive clause or adjective, or even, apparently, if there is an 'understood' relative clause, i.e., there is no relative clause present but the stress pattern suggests that the

speaker has some such adjunct in mind. In all three cases any is grammatical:

- (10.11) a The headmaster thrashed any boy
who had teased the French mistress
- b Any lazy student failed the exams
- c The police booked any motorist

(11a) can be paraphrased by:

(10.12) If any boy had teased the French mistress the headmaster thrashed him and similar paraphrases are available for (11b) and (11c), although in the latter case the 'understood' clause has to be made explicit. That may provide counter-evidence to any hypothesis wherein (11a) would be derived from an underlying structure more closely resembling (12) than itself, and also such a hypothesis would apparently pose severe problems with regard to pronominalisation. This is unfortunate, for it is a most appropriate hypothesis, and therefore we shall make some attempt to ameliorate the situation below. In any event, with regard to (11c) we ought to remember the case of stressed all which we discussed in §9.4; it may be that here too some rather different explanation will be needed for stressed as opposed to unstressed any. But at present we can only claim that the grammaticality of any in (11a) is related in some unknown way to conditional structures.

Finally there are two other contexts in which any is grammatical, two contexts which are often considered together by historical accident, both having been discussed extensively in Katz and Postal (1964). The first of these is the imperative:

(10.13) Answer any question (!)

Strangely enough, however, this seems to be related to the type exemplified by (11c), for any may receive heavy stress; but the relation is unstable. It is most probable that (13) is ambiguous, having both a true imperative reading: "Answer any question that may be put to you!", and a pseudo-imperative reading: "Answer any question that you like". The second reading is of the type discussed by Vendler (1967:79-82) but the first reading is a counter-example to Vendler's position (and hence that of Jackendoff, 1972b:339), since there is no choice available to the addressee. Intuitively both readings are related to the examples in (11), but it is not certain how the relation is to be formalised. The other context to be noted is, of course, the interrogative:

(10.14) Did you sell any bananas?

Given the fact that all the other instances of grammatical any which we have mentioned are in contexts where the existence of the referent(s) of the collocating NP is not asserted, it is fully predictable that (14) would be acceptable, for in such a sentence it is the existence of bananas (such that they have been sold) which is

being questioned. All the above examples taken together point the way, therefore, to the tentative position that every instance of any is in one and the same context: the context where there is no assertion (but not necessarily any denial) that referents of the collocating NP exist in the world referred to and limited by the discourse.

Within the theory of transformational grammar there have been two principal paths along which the grammarian has trod in search of an answer to the problems posed by the facts above. The first leads to a 'some-any' rule, that is, a rule which converts some to any in certain contexts. This rule was first proposed in Klima (1964), and Klima suggests that the rule operates when a quantifier is within the scope of an 'affective', e.g., a negative, a question morpheme or a conditional, in other words basically the environments discussed above. The second path leads to the postulation of a group of operators, e.g., Hypothetical or Unrealised, together with a statement that quantifiers such as any are grammatical only when within the scope of such an operator. The most important proposals to this effect are to be found in Seuren (1969:104-63) and Jackendoff (1972b:279-320), although otherwise these two works adopt very different theoretical positions. To a greater or lesser extent the positions adopted in all three works are incompatible with the basic tenets of this study. This

is clearest in the case of Klima, for his 'some-any' rule is meaning-changing under certain conditions, and we have held that there should be no meaning-changing rules. Since we shall wish to discuss the relation between some and any below, and since the meaning-changing status of Klima's rule (but not of every possible 'some-any' rule) is not disputed, we shall leave that point for the moment. However the incompatibility between the introduction of operators and our own theory will probably be much less clear, and so it may be useful to discuss it more extensively now.

When we first discussed both, in Chapter 4, we toyed for a little while with the addition of features to a quantifier, i.e., the creation of a complex symbol, cf. Chomsky (1965), in order to distinguish between both and all. However we were able to show that such features were undesirable and that a more adequate solution did not need to use them. Later, in §§7.2 - 7.3 we discussed various proposals to introduce operators to account for the grammar of some (and the other quantifiers), in particular those by McCawley (1971) and Bach (1968). We were able to conclude that those proposals introduced an unnecessary complexity into the grammar and thus a more adequate grammar would not use such concepts either. Thus we can observe that the use of either features or operators leads, at least in some cases, to an inadequate grammar. This empirical conclusion is reinforced

by a theoretical point. Both features and operators have in common the fact that their existence is limited to underlying structures, that is, they must be modified into some other form before the surface structure is reached. On the other hand, the alternative structures which we have proposed contain only items which may appear in surface structure. In other words, apart from category symbols, the repertoire of our analysis of English contains only members of English. This is obviously a stronger hypothesis than one which permits arbitrary features or operators, and therefore it is theoretically preferable.² Although the operators introduced by Seuren and Jackendoff are different from those introduced by, say, McCawley, they are in principle the same and are thus incompatible with the 'best grammar'.

As we have ruled out the possibility of using either of the more common methods of accounting for the grammar of any, we shall have to search for an alternative method of our own. In order to do this with some hope of success we shall claim that there are three

² Some features are still necessary, however, for example [±singular]. It is difficult to see how their use can be avoided, but at least they have the merit that they are uniquely related to given linguistic events, see further Chapter 11.

subgroups of contexts which permit any, namely negatives, questions and conditionals. The first two categories speak for themselves; the third category covers all other cases of any which we have discussed, that is, modal-governed any, generic any, imperative any, and restrictive relative any. It will be recalled, however, that this latter type, exemplified by (11), is at first sight rather different from the other three (with perhaps some doubt about the imperative). Obviously the three groups mentioned here are not necessarily justifiable, but it is to be hoped that they will be justified by the kinds of analysis of any which we shall attempt to demonstrate are valid and necessary. A further assumption which we shall make is that some and any are synonymous, although this is perhaps incorrect; nevertheless it has an element of truth about it since it seems improbable that the underlying structures for the two quantifiers will be radically different from one another. We shall return to any putative meaning differences between some and any below.

Let us consider firstly the structures which we have called conditionals, as exemplified by (6), (8) and (13). The most obvious point is that none of them can be paraphrased by an existential structure but that all of them can be at least approximately paraphrased by a conditional. Thus compare the following existential paraphrases of (6a) and (7a) respectively:

(10.15) a *There is any book he can read

b There are some books he can read

Now compare the attempted conditional paraphrases of the same sentences:

(10.16) a If there is a book he can read it

b If there are some books he can
read them

Although (16b) is grammatical, it is not a paraphrase of (7a). The evidence of such attempted paraphrases together with our semantic intuition that with any there is no assertion of existence - which, clearly, can be the only explanation of the generic status of (8) - strongly supports our claim that there is an underlying conditional in some sentences containing any. However there is one problem which must be resolved before we postulate an underlying structure and discuss consequent derivational histories. The problem is that there appears to be two possible positions in structure for the conditional: again taking (6a) as our example, we could propose a structure corresponding to either (17a) or (17b):

(10.17) a If there is an object which is a
book he can read it

b There is an object which, if it
is a book, he can read

In the (a) case the conditional is on the existential sentence and in the (b) case it is on the predicate nominal. Some evidence that both might be possible but

that the (b) case is marked comes from the fact that the (b) interpretation of (6a) is preferred only if book is heavily stressed. With such stress the interpretation is that there is a given set of objects but it is only if one of these objects is a book that he can read it. Without the heavy stress there is no assertion that there are any objects at all (for the purposes of the discourse). However the following sentence shows that the problem is not easily resolved.³

(10.18) You can visit any ship

(18) is ambiguous: the invitation may be to visit only one ship, no matter which, or it may be to visit as many ships as is desired. The first interpretation is paraphrasable as (19a), the second as (19b):

(10.19) a If there are objects and one of
 them is a ship you can visit it

 b If there is an object which is a
 ship you can visit it

Therefore it appears as if conditional structures for both must be generated.

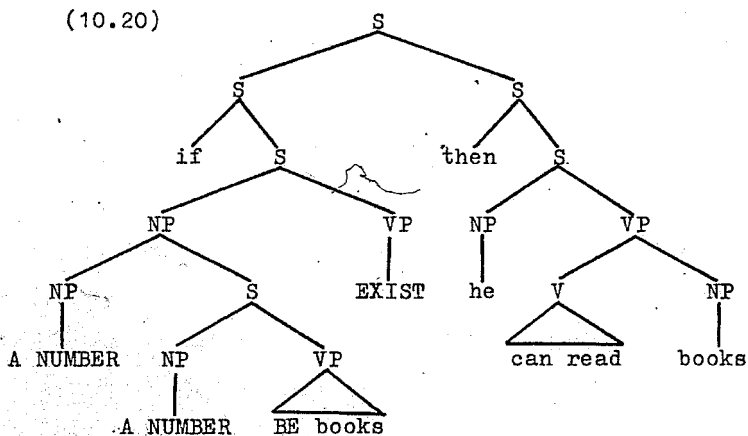
One general objection to the approach taken here concerns our claim that in a sentence such as (6a) there is no assertion of the existence of books. Taken

³ Notice that (18) is very similar in meaning to: "Visit any ship". This helps to support the claim that imperatives containing any are basically conditionals.

together with this there may be some objection to our use of the term 'conditional'. Thus it may be objected that (6a) would be nonsense if it were not presupposed that there were some books. Even if this objection is correct it is not wholly so, for it misses the point that the statement of possibility is not restricted by the presuppositions of the speaker: it is not only the books whose existence is presupposed that may be read, all other 'book-events' may be read too. This is what Vendler (1967:80) means when he writes that with any "I grant you the unrestricted liberty of individual choice". The choice cannot be restricted in any way, not even by the presuppositions of the speaker. And it is this implication which is intended by the use of the term 'conditional'. A sentence under the scope of a conditional is simply one where the speaker does not vouch for its truth or falsity. If any is used the speaker does not determine the referents of the collocating NP and therefore he cannot vouch for the truth or falsity of the sentence. Even in cases such as (11a) where it may be assumed that the speaker is willing to claim that at least some boys were thrashed by the headmaster, he is not making a claim as to how many there were, rather he is stating the conditions under which they were thrashed.

Therefore there seems good reason to suppose that the structure of any in the sentences which we have been

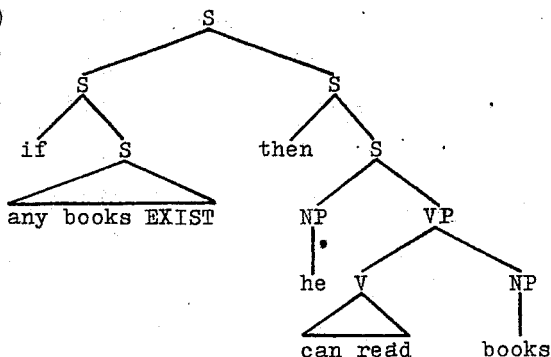
discussing is intimately bound up with conditionals. The main problem so far is whether (17a) or (17b) is the better surface approximation to the correct underlying form, As we have seen, there is little evidence upon which to make a decision, but that little does suggest that the former is to be preferred. So we can postulate the following underlying structure for (6a), in which we ignore the structure of modals and assume that some and any are synonymous:⁴



⁴ Obviously there must be some dispute about the underlying structure of conditional sentences; however, (20) seems to be the most plausible type of structure and so it has been adopted without further argument. A further point ignored here is the singular/plural distinction, whose import is insufficiently clear to this writer to make its discussion here fruitful.

Assuming that the development of any is identical to that of some, lexicalisation will give the following structure:

(10.21)



Notice now, however, that existential-lowering cannot apply, since there is no lower S into which any books could be lowered as EXIST is deleted. A possible solution to this would be to assign a lower-S status to the then-clause; but there seems to be no non-ad hoc motivation for that, and therefore we shall have to see what can be done with (21) as it stands. The only move available appears to be an any-placement transformation, which will shift any books into the coreferential NP in the then-clause. This can be formulated as:

(10.22) If_Sany N₁ EXIST]then[_SX N₁ Y]

1	2	3	4	5	6	7	8	
1	2	3	4	5	6	7	8	----->
1	0	0	0	5	6	2+7	8	

We still have to account for the deletion of if and

then, but this is to be explained as due to the lack of a dependent clause for if following (22) and the correlative nature of conditional constructions: we must delete if because there is no dependent S and then is deleted because there is no if remaining. Thus (22) would give an intermediate structure of:

(10.23) *If then he can read any books

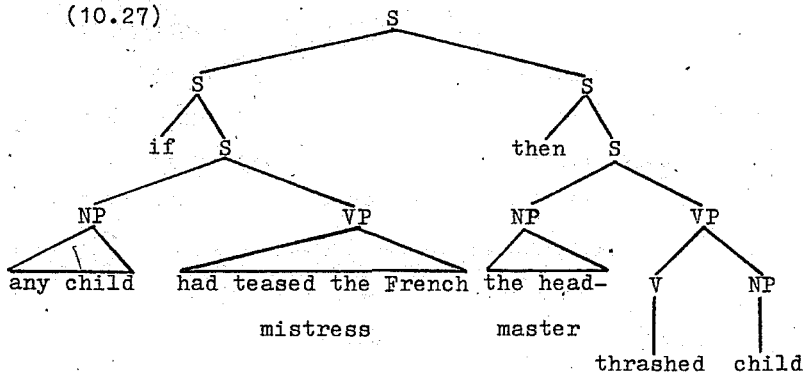
and we delete if and then by a rule of the form:

(10.24) If then S ----->• S

We shall see below, however, that the postulation of this any-placement rule to generate, say, (6a) is incorrect. And that is desirable because the rules are complex and otherwise unmotivated; however the evidence of (6a) does not in itself support immediately any other hypothesis.

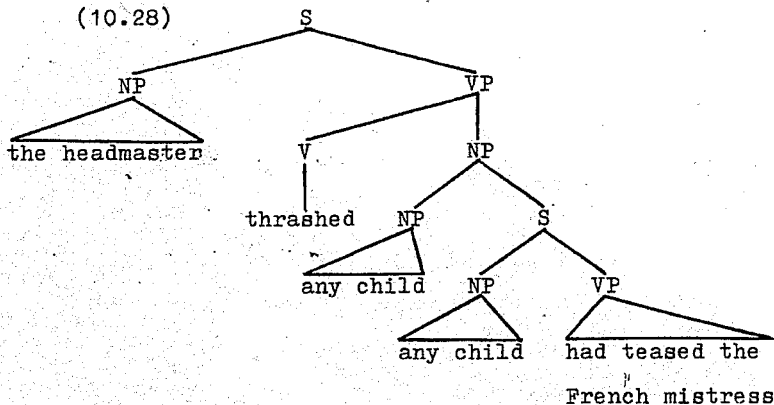
As we have already stated, although (11a), etc. can also be considered as underlying conditionals, their structure will prove to be rather different from (20), which we may take (for the present) as representative of the other types in this subgroup. This can be deduced from the fact that (12) is a paraphrase of (11a), and it is reflected at the surface by an overt conditional in (12). If we assume that (12) most closely reflects the underlying structure of (11a), as seems probable, then we may propose (25) as that structure:

(10.27)



It now looks as if our earlier any-placement rule is indeed wrong, for the preferred transformation to reach (11a) will be one which moves the S dominated by if to below the NP dominating child in the then-clause. If-then deletion will still take place, however, and the result will be:

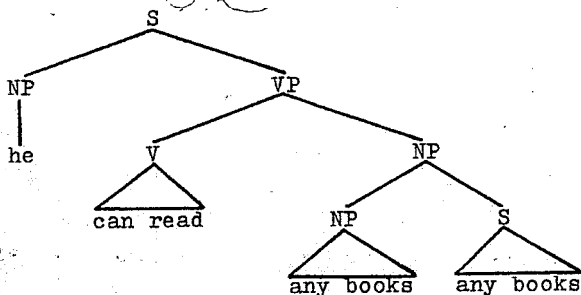
(10.28)



Then relative formation will change the lower any child to who and the surface structure of (11a) results.

The derivation from (25) through (27) to (28) and thence to the surface structure representation of (11a) has two distinct advantages. Firstly, the attachment of the if-clause to the coreferential NP in the then-clause bears at least a family resemblance to existential-lowering, since it creates a new highest S which is the matrix sentence and the quantifier is moved to a relatively lower position in the tree. Secondly, instead of having to derive (11a) in one way, and (6a) in another, as it was originally thought we would have to do, it now becomes clear that the latter sentence can also be derived by the same transformational process as immediately above. Thus from (21) we would derive:

(10.29)



The higher verb EXIST would be deleted since this is a function of all lowering transformations on an existential and it is now possible to identity-delete the second occurrence of any books, resulting in (6a).

There therefore appears to be some probability that our proposed hypothesis has some value; and from this hypothesis there follows another which may help to solve

some of the mysteries surrounding any. We have claimed that there are three contextual subgroups in which any occurs: negatives, questions and conditionals. The first two of these presumably involve higher neg and qu elements. We have now suggested that in the third case there is an if node above the existential sentence which is the basic source of any. From this we may conclude, at least tentatively, that one of these three elements, which correspond to Klima's (1964) category of affectives, must always command any in underlying structure. The interesting corollary to this hypothesis, for which we have not yet encountered any counter-evidence, is that some must not be commanded by an affective in underlying structure. Whether or not this is true we shall see below.

In order to test the validity of this proposed constraint we ought to consider the status of (7a), since apparently the only difference between it and (6a) is the some - any contrast. But we have already observed, cf. examples (15) and (16), that there is evidence that there is no conditional in the underlying structure of (7a), and therefore our predictions regarding the distributions of some and any still hold. More difficult, however, is the pair of sentences suggested by the discussion on some and any in a paper by Robin Lakoff (1969a:609-10):

(10.30) If you eat some candy, I'll give
you ten dollars

(10.31) If you eat any candy, I'll whip you

As R. Lakoff says, the if ... some combination is usually taken as a promise, whereas the if ... any combination is taken as a threat. Thus the above sentences are often more appropriate than the pair of sentences in which the above then-clauses have been transposed.

R. Lakoff continues by saying (1969a:612):

"It is difficult to see how ... both the meaning and the syntactic properties could be accounted for together, and the generalisations achieved without performative abstract verbs."

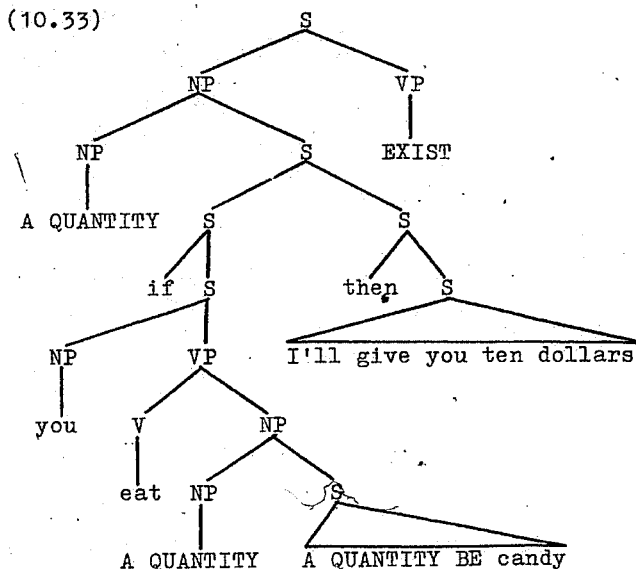
So far in this study we have attempted to eschew the use of performative verbs, and so it would clearly be preferable if we could do so here, but the problem, of course, is that there is no obvious difference in structure between the two sentences above which might be related to the differences in meaning whose only product seems to be the some - any contrast. However, not only is there an a priori objection to R. Lakoff's suggestion that some be linked to a 'positive' performative verb of the promise-type and any to a 'negative' or 'neutral' performative verb of the warn-type, there is also an empirical objection. Consider the sentence:

(10.32) If you eat any candy, I'll give you
ten dollars

Such a sentence need not have a negative or neutral implication in order to be fully acceptable, although if R. Lakoff is right then that is most probable; it may also be construed as a plea, in which case the performative, if it exists, must be of the promise-type. The plea situation is most likely when the action described in the if-clause is thought to be unpalatable to the addressee, and if we hold to Lakoff's approach, then it would seem that the abstract performative will be dependent upon presuppositions, which hardly seems to be an economical or even fully comprehensible situation.

Given that the consequences of the performative analysis are unappealing, it seems desirable that we consider whether or not our putative constraint will do the job; otherwise we are going to be faced with the system in which there is an optional and meaning-changing 'some-any' rule. From our discussion above it follows that the underlying structure of (31) must be similar to that given in (25). There will, of course, be no intermediate structure parallel to (28), for the transformation which generates that structure, which we may call Conditional Relative Formation, crucially depends upon the presence of an identical NP in the then-clause. Since any will be commanded by if in underlying structure, the surface (31) is grammatical. But in the underlying structure of (30) if must not command some if our constraint is correct. The only

structure for (30) which is at all similar to (25) and which will satisfy such a condition is:



As discussed above, especially with reference to (19), there is some ambiguity about the relation of the predicate nominal to the conditional. (33) simply represents an attempt to be consistent, even if one is being consistently wrong.

The only plausible interpretation of (33) is along the following lines:

- (10.34) There is a quantity of candy and if
 you eat that quantity of candy I'll
 give you ten dollars

If we consider the remarks of Jackendoff (1972b:340) in

this context then we can see that that is exactly the interpretation of (30):⁵

"Some ... implies that there is a minimum expected amount below which the speaker will not accept some X as appropriate. For example look again at (30) - (31)... In (30), you will probably get 10 dollars only if you eat at least a piece or two of candy, but not if you just nibble the corner of one piece. In (31), however, you are liable to be whipped even if you take the merest nibble."

Our analysis is simpler than R. Lakoff's performative analysis since it introduces no elements which are not otherwise needed, and, in any case, we have seen that the performative analysis breaks down in certain contexts. Jackendoff (1972b:341) also relies on semantic conditions (for this is what I take performatives to be) and is thus also more complex. On the other hand, our hypothesis only relies on the command relation between if and the quantifier. Further we have seen that everything which is within the scope of if is not asserted as true by the speaker, and this precisely and obviously relates to the remarks quoted from Jackendoff above.

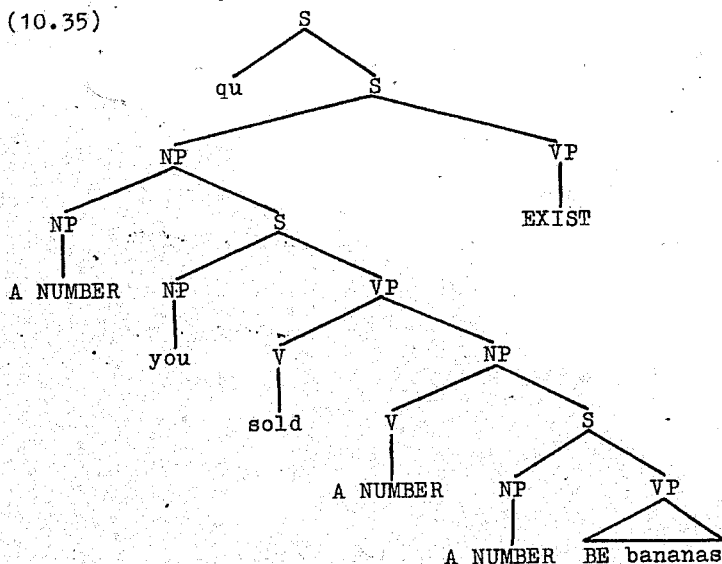
⁵ The numbering in the quotation below is due to the present study and not to Jackendoff (1972b).

When some is used the speaker asserts the existence of a certain quantity (of candy); when any is used no commitment to any discrete quantity is made.

Therefore there does seem to be some justification for claiming that our proposed structure for conditional sentences is adequate and is able to explain a wider range of constructions than other hypotheses. But when we come to consider questions with any we run into the same problem as do earlier studies.* Using the structure of conditionals as our guide, we may suppose that the underlying structure of (14) - repeated here for convenience - will be of the approximate form of (35):

(10.14) Did you sell any bananas?

(10.35)



The problem does not lie in the derivation from (35) to (14), which clearly is perfectly standard; rather it is that it appears as if (35) must also be the underlying structure of:

(10.36) Did you sell some bananas?

Any structure in which the qu morpheme is commanded by the underlying quantifier-noun is strikingly implausible as a source for (36). Given that, it looks as if we shall have to modify our position in two ways: firstly we shall have to permit some to occur within the scope of an affective if that affective is qu; secondly we shall have to drop the assumption that there is a 'some-any' rule which operates obligatorily in certain contexts. This second point obviously follows from the first.

But surely this first modification is highly implausible: the condition on some is suspect because it applies to one item only; in such cases it seems more probable that it is our analysis of the item which is incorrect. Now if we recall languages such as Latin and Gothic it will be remembered that apart from a 'neutral' method of questioning, questions may be introduced by the forms nonne and num in Latin and niu and ibai in Gothic.⁶ These alternative forms suggest that it may

⁶ For Latin see any elementary grammar; for Gothic see Wright (1954:168, 329, 338).

indeed be the qu morpheme which is ambiguous, not the some-any quantifier. Now a very plausible method of describing the ambiguity is by using the feature [+affective]. This corresponds approximately to the English sequences surely and surely ... not. It may well be that the use of the feature [+affective] is ad hoc, but at least it affords a temporary solution to the problem in Latin and Gothic. Now given the situation in those languages there is reason to suppose that something similar happens in English. Indeed, there is every reason to believe that this is the case, not only from an a priori desire to find universals, but also because it permits the most simple explanation of the ambiguity of (35). Instead of having to modify a constraint and reject a suppletion rule both of which have been observed to have some descriptive adequacy elsewhere in the grammar in an unmodified form, we can now explain the ambiguity of (35) as due to the ambiguity of the qu morpheme. If that morpheme is [+affective] then (14) will be generated, if it is [-affective] (35) will underlie (36). Furthermore we ought to observe the contrast in grammaticality between the (a) and (b) sentences below:

(10.37) a *Surely you sold any bananas?

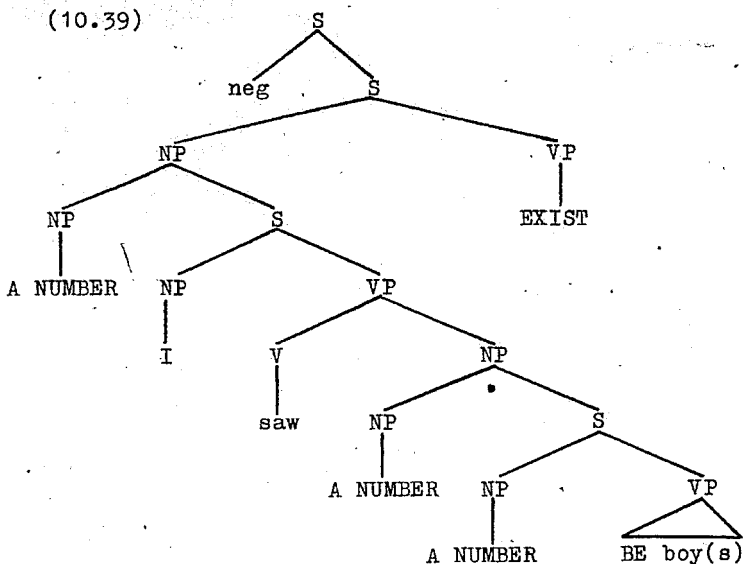
b Surely you didn't sell any bananas?

Clearly this is to be explained in terms of the presence of a negative in (37b), but nevertheless there must be some hope that a parallel with the simpler interrogative

forms can be found. Even if there must be some considerable hesitation it seems fair to say that we can now claim that the structure underlying any in conditional contexts and that underlying any in interrogative contexts are basically similar and that the transformations and constraints which generate the correct surface structures are the same for both types. Let us now, therefore, move on to a consideration of any in negative contexts in order to see whether our hypothesis holds true for the third subgroup.

The retention of the theory that for any to be grammatical it must be commanded by an affective element in underlying structure poses no problems for the derivation of any in negative sentences. Thus, allowing for number variation, (39) is probably the underlying structure of (38):

(10.38) I didn't see any boy(s)



If a negated quantifier lowers as described in 88.4, then (38) will be generated, unless the higher neg is lowered into the existential sentence before existential-lowering takes place, which is optionally possible. In that case we shall obtain, after lowering:

(10.40) I saw not any boy(s)

Then Klima's (1964:280) neg-incorporation rule transforms not + any into no:

(10.41) I saw no boy(s)

Notice that if the neg-incorporation rule is obligatory, and negated quantifiers are lowered as has been described, then the ungrammaticality of a sentence such as (4) - repeated here:

(10.4) *Any boy isn't sitting on his desk

is easily explained. After existential-lowering a structure corresponding to (42) will result:

(10.42) [_Sneg[_Sany boy is sitting on his desk]]

Neg is then lowered to give:

(10.43) Not any boy is sitting on his desk

Then on that structure neg-incorporation will obligatorily operate and the grammatical sequence no boy will result, cf. (5). If any is grammatical only when commanded by an underlying affective there can be no other source for the surface negative.

We can see, therefore, that the analysis of any in negative sentences is not only compatible with, but also supports, our hypothesis about occurrences of any. But since some can also occur in negative sentences we shall have to examine those instances before making any serious claims. We need only examine two typical cases, one with some in subject position, the other with some in object position, since any other examples will fall into one or other of these patterns:

(10.44) Some boys didn't come to the party

(10.45) I didn't see some boys

The first example does not in fact present any difficulties, since it is indisputable that the quantifier will be higher than the negative in underlying structure, i.e., there is only a neg-V reading, and therefore it cannot ever be a counter-example to our theory. The

real question concerns the relative heights of the quantifier and the negative in the second sentence. If Lakoff's (1971c:244-46) claims about surface structure precedence were correct, then it would appear that in some dialects the quantifier would have to be lower than the negative in (45). But there do appear to be other dialects in which there is no reason to suppose that the quantifier could not have been higher than the negative in underlying structure. Further, it certainly seems to be the case that for some speakers (45) is at best dubiously grammatical, and these speakers seem to be those who would prefer a neg-Q reading for (45). This is exactly predicted by the constraint which we formulated in §8.4, (8.88), by which it is not permitted to lower a quantifier into a negated VP. But some cannot originate from a position lower than a neg. Thus for speakers who obey constraint (8.88) our example (45) ought to be ungrammatical. The present writer has some doubts about whether such a constraint exists in an absolute form for any speaker, but that is not crucial since it undoubtedly does exist in a milder variant which could easily introduce at least the degree of unacceptability which is regularly found with (45), see further our remarks on (8.90) in §8.4.

Since the relation of negative elements to some and any completely supports our hypothesis we may now reasonably claim that that hypothesis is correct. Thus the

position we hold is composed of the following elements. Firstly, there are three items which, if they command a simple existential quantifier-noun in underlying structure, permit grammatical occurrences of any. These three elements are if, qu and neg. Of these three only the second has an air of dubiety about it, and it must be conceded that our use of the feature [affective] to distinguish between instances of qu permitting any and those not permitting any is ad hoc. But this only implies that a better analysis of the morpheme concerned is necessary. Secondly, any does not occur grammatically if it is not so commanded. Thirdly, some is grammatical only if it is not so commanded in underlying structure. Taken together these three points lead to the fourth, which is that some and any have identical underlying structures and there is an obligatory 'some-any' rule which converts some to any in the presence of an affective. Since the rule is obligatory it cannot change meaning. Finally, the rule postulated by Klima to convert not + any sequences to no is obligatory and operates whenever such a sequence is found. The economy of this position does not stem only from the fact that some and any are given an identical underlying structure; there is also the theoretical point that we need only use meaning-preserving rules and there is no need for the additional apparatus inherent in global rules or semantic interpretation rules being introduced. This

not only permits us to constrain the formal powers of the grammar, but it also serves to underline the autonomy of underlying semantic representations.

10.3 Partitive constructions

Despite the wealth of literature, if that is the correct phrase, written on the topic of quantifiers, remarkably little attention has been paid to the status of partitive constructions involving quantifiers, that is, constructions of the form:

(10.46) Some of the boys

Thus in Gärden (1968) - and in his later papers - there is absolutely no explicit discussion of differences between partitive and nonpartitive constructions. Jackendoff (1968) is little better in that all that he proposes is that of is obligatorily retained if the following NP is [+definite], unless the quantifier is all, both or half (if the latter is indeed a quantifier), in which case of-dropping is optional. The reason for this lack of attention is probably that the major problems concerning quantifiers, which we have discussed in the earlier parts of Part III, are found in the same form in partitive and nonpartitive constructions, but cf. Johansson (1974:26-27) and our own unsatisfactory remarks on his argument in §8.4. Therefore the distinction between the two types is not crucial for most purposes. Nevertheless the partitive constructions

that the first of these points is misleading in that the relation between possessives and partitives is not as simple as Lee assumes, and as would be necessary for his analysis to be consequently inviting, and it is also shown that Lee's analysis does not in fact meet the conditions of entailment he sets down. Since Lee's reply to this paper concedes the principle of both these points we need not discuss them further, rather the reader is referred to Lee (1971, 1972) and Hogg (1972) themselves. There are also a number of other points on which Lee's analysis could be criticised, especially in the light of the preceding discussion of quantifiers in nonpartitive constructions, but many of these ought to become apparent in the discussion below. In any case Lee (1972) is such a revision of his earlier paper, for example he adopts to some extent the Lakoff-Carden theory, that further remarks on Lee (1971) might be superfluous.

Anderson (forthcoming), and, in a similar fashion for Russian where the facts are unfortunately more complex, Miller (1972), propose that the partitive construction is the surface realisation of an underlying 'ablative' relation between the quantifier and the 'definite' NP. Regrettably, the 'translation' from a localist framework into the one used here presents more complex problems than were raised in our earlier discussion of universal quantifiers, and indeed I cannot

clearly see how such a 'translation' is to be effected. Rather than examine Anderson's proposals at considerable length, therefore, I wish to consider one point only. The structures given in Anderson (forthcoming) permit a quantifier partitive construction to be the complement of an existential sentence (the same seems to be true of Lee (1971), but there is no discussion of the point). Therefore they predict that (49) is grammatical:

(10.49) There were some of the boys came to
the party

But most native speakers whom I have consulted find, at least, that the grammaticality of (49) is not fully apparent, and it is certainly the case that (50) is rejected:

(10.50) *There were some of the boys who
came to the party

(e) It would be extremely difficult to explain why (49) was grammatical in contrast to (50) and so we shall take the position that the former is a substandard form which we should not attempt to generate. This position is justified by the majority opinion of the grammaticality of (49), but it has to be recognised that it may be a defect of the analysis we present below that it cannot

account for (49).⁷

In one sense, however, this stance does not appear to improve the chances of reaching a satisfactory analysis, since it presents us with a paradox. We have been able to show that the presence of an existential quantifier and the possibility of an existential sentence are mutually dependent, yet we now see that a partitive construction with an existential quantifier as its surface structure head cannot be the complement of an existential sentence. One way round this paradox would be to claim that the existential sentence is blocked by the presence of a 'definite' NP in the partitive structure. But this is unsatisfactory for two reasons. Firstly, the 'definite' NP is not the surface head of the phrase and a selectional restriction which makes no mention of a phrasal head but only of a subordinate constituent of the phrase is extremely improbable. Secondly, up to this point we have analysed the grammaticality of an existential sentence as a matter for the base; either an existential sentence is generated by the base rules or it is not, and that is the only factor at issue. Here we seem to be wanting to introduce another

⁷ Our position is supported in a negative way by the absence of the type of existential structure exemplified in (49) and (50) in the discussion of existential sentences in Quirk *et al* (1972:956-62).

factor which will determine the grammaticality of an existential sentence. This is obviously a complication of the grammar which ought to be avoided if possible.

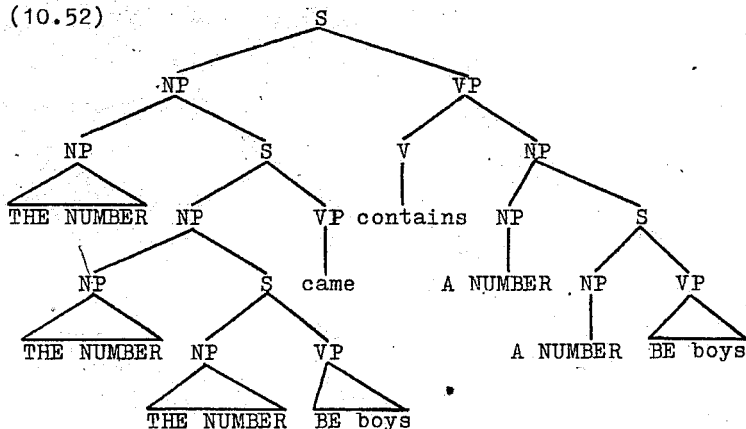
Clearly our aim must be to justify an underlying structure in which there is no existential sentence. But the only type of underlying structure containing an existential quantifier yet not containing an existential sentence which we have so far suggested as plausible is that underlying phrases with postdeterminer quantifiers, cf. §8.3. In such cases, however, there can be no some, since the structure relies crucially on an adjectival modifier to fill the postdeterminer position. But some appears freely in partitive constructions. Also, in postdeterminer constructions the quantifier does not originate from a higher sentence, which is not the case with the present type of quantifier which displays exactly the same characteristics as does a quantifier in a nonpartitive construction. Thus we are apparently faced with two conflicting demands, that the highest sentence must contain both an existential quantifier-noun and an NP - presumably 'definite' - which would account for the lack of existential sentence.

If the existential quantifier is in the highest sentence and yet is not the complement of an existential sentence it is no more than a truism to say that it must have some other function. And if a 'definite' NP is in the highest sentence, it must be related to the

quantifier-noun in some way since the two NP's are by definition non-identical. There is absolutely no evidence that any other NP is involved, and therefore the two NP's in question must be related by some verb. At this point we may take up once more Lee's (1971) suggestion, suitably modified for our theory, that the quantifier-noun is the object of the verb HAVE which has as its subject a 'definite' NP. We need not accept Lee's use of HAVE and his remarks about possessives to agree that he is correct in his viewpoint that the quantifier-partitive construction reflects a relation of set inclusion, cf. Lee (1971:9-11). This being so, it seems most probable that the highest sentence in the structure underlying a quantifier-partitive construction will be one that expresses the fact that the set indicated by the quantifier-noun is included in a given set. To express this notion we shall state that the given set contains another set; this will be the highest sentence and the matrix and predicate nominal sentences should follow naturally. Thus a first approximation of the structure underlying (51) will be (52):

(10.51) Some of the boys came

(10.52)



However (52), which follows the same construction principles as does the structure for postdeterminer quantifiers in §8.3, example (8.42), must be wrong, for the very simple fact that it gets the semantic facts absurdly wrong. The only possible reading of (52) seems to be:

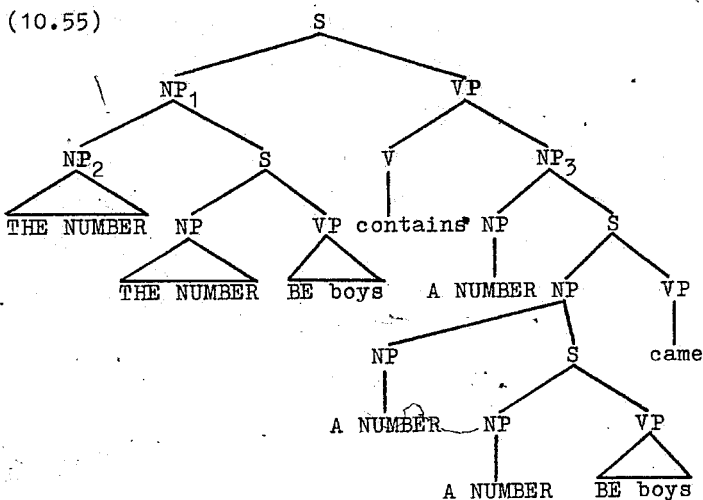
(10.53) The boys who came contain some boys

In other words, the subject of came is analysed as the 'definite' NP and not the quantified NP, whereas the reverse is the situation, as can be seen from an example like:

(10.54) One of the boys likes cheese

where the verb displays singular concord. (52) would predict plural concord. Precisely linked to this semantic error is the inaccessibility of (52) to quantifier-lowering, which must play a role in (51) equal to that played by existential-lowering in the parallel nonpartitive construction.

Instead of (52), therefore, we must have a structure in which the matrix sentence is below the quantifier-noun rather than below the 'definite' noun. That structure will look like:

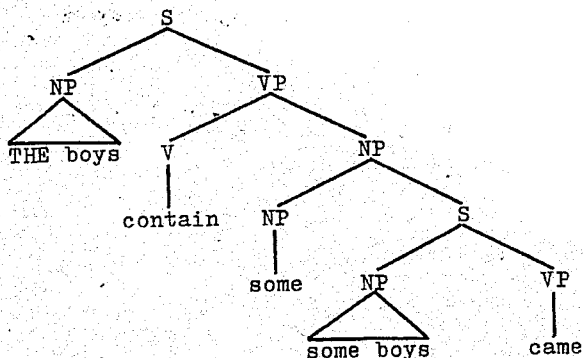


The most interesting feature of (55) is that the structure below NP₃ is identical to the structure dominated by the equivalent NP in a nonpartitive construction. The only difference between the two NP's is that the latter is the underlying subject of EXIST whereas NP₃ is the underlying object of contains. This has the important consequence that the development of the two types of quantifier construction will be identical except in one respect: the partitive quantifier will never become the complement of an existential sentence at the surface, instead its surface realisation will reflect its underlying object position in relation to contains. In every

other respect the partitive quantifier will behave exactly as does a nonpartitive quantifier. Thus compound existentials will be possible; the relation of negatives and, indeed, other affective elements, will be exactly the same towards a partitive quantifier as towards a nonpartitive quantifier; the universal quantifiers all and each will also be generated, since it is only the structure below NP₃ which determines their grammaticality. In connection with this last point we should note that the possibility of a generic interpretation for all in a partitive construction will be excluded by the presence of the 'definite' referring NP. This will explain why in all dialects partitive all is grammatical in sentences where no generic interpretation is possible.

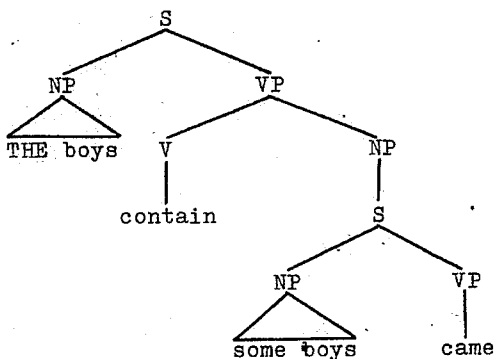
The principal objection to (55) must be the mechanism of quantifier-lowering. Before this transformation takes place we shall find, schematically:

(10.56)



We cannot simply lower some for then we would obtain:

(10.57)



Rather, it seems desirable that quantifier-lowering affect every node above the matrix sentence simultaneously, which means that partitive formation will occur at the same time. This makes the transformation unfortunately complex but there seems to be no alternative. And there is some justification for claiming that (56) is the structure immediately before quantifier-lowering in that we saw in §7.5 that the structure immediately preceding existential-lowering of nonpartitive some permitted generation of:

(10.58) There were some boys who came

The same moves in connection with (56) will give:

(10.59) The boys contained some (boys) who
came

This is desirable since although (59) is scarcely acceptable there are cases involving collectives which do have that structure:

(10.60) The group contained some boys who
came

Presumably (60) is derived by deletion from:

(10.61) The group of boys contained some
boys who came

We have already observed in §7.3 the great similarity between such nouns and quantifiers and thus this gives substantial support to our theory and the above objection cannot be considered of sufficient weight to reject a solution which in every other respect is compatible with our previously justified theories. Most importantly, this solution explains the syntax and semantics of partitive quantifiers in a more revealing manner than has been possible with earlier proposals.

10.4 Another look at 'both'

In Chapter 4 we examined the structure of both in terms of the Lakoff-Carden theory that quantifiers are in underlying structure higher verbs. We came to the conclusion, which we shall not re-justify here, that both was a highly complex quantifier containing a universal quantifier (all), a compound existential quantifier (two) and a deictic element (the). Not surprisingly, there were a number of points on which the Lakoff-Carden theory was found to be not wholly adequate and it was impossible in that theory's terms to provide full analyses for every occurrence of both. It is

therefore desirable to attempt at this stage to see if the hypothesis which has been offered in the preceding pages is able to overcome the defects of the Lakoff-Garden theory. In a very real sense this is a crucial test, for both is clearly one of the most complex quantifiers in English, and therefore if we are successful in the attempt we shall have some justification for claiming that our hypothesis is a useful one.

It will be recalled that there are two basic structures for both, one a surface nonpartitive construction, the other a surface partitive:

(10.62) Both boys came

(10.63) Both (of) the boys came

These two sentences, it was claimed, were related by the Dual Copy transformation to (64) and (65) respectively:

(10.64) *All of the two boys came

(10.65) *All two of the boys came

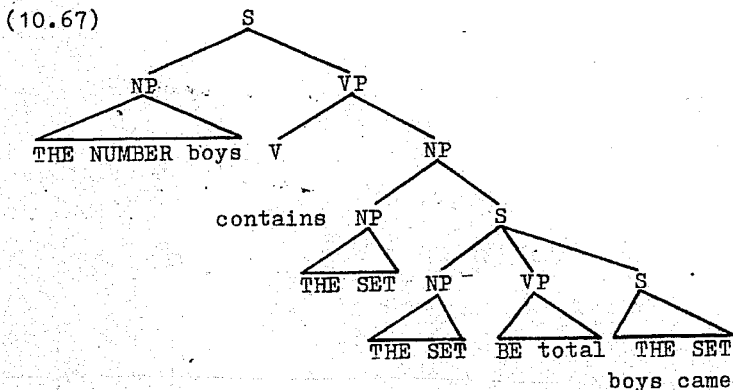
Clearly the point at issue is whether or not (64) and (65) are generable, and the question of how Dual Copy operates is unimportant (here), for it is quite independent of the specific theory about the origin of quantifiers (although it does assume that underlying structures are semantic representations). Therefore we need only consider the problems involved in generating this latter pair of sentences. Given the complexity of the situation it is certainly preferable to approach it in easy stages, as it were, and so we shall consider (64)

and (65) quite separately and, furthermore, in each case we shall approach the sentence by looking firstly at its constituents.

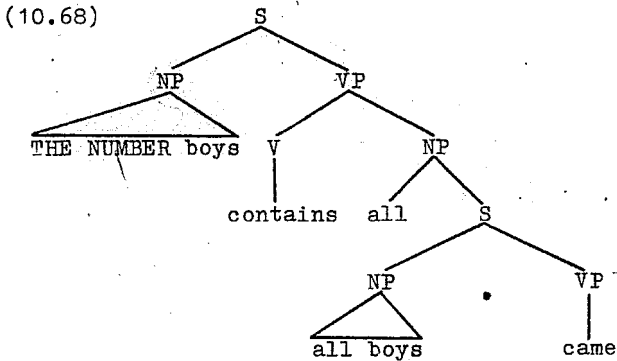
Since (64) appears to be the simpler of the pair, let us first look at it. There is no difficulty as far as the part containing the postdeterminer quantifier is concerned, for it will have the structure outlined for postdeterminer quantifiers in §8.3. But we have not yet given in detail the structure for:

(10.66) All of the boys came

However if partitive constructions are derived from the type of underlying structure proposed in §10.3 there will be no serious difficulty here either, and the underlying structure of (66) will approximate to (67), in which, for convenience, the predicate nominal structures have been raised:

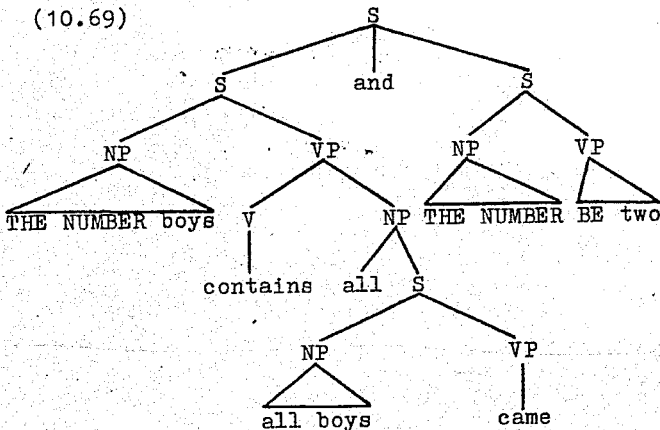


Immediately before quantifier-lowering and partitive formation the following structure will be found:



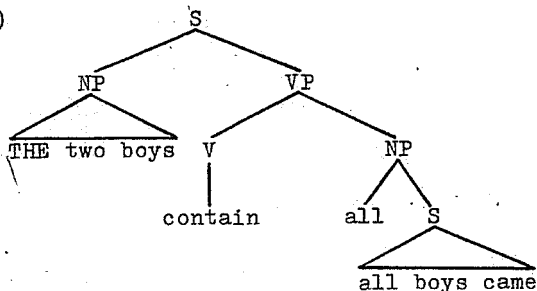
Thence the structure of (66) is derived.

If we can combine (67) with the underlying structure for postdeterminer two, then we shall have the underlying structure for (64). There is in fact nothing to prevent us doing so, and the result is, in an abbreviated form:



After two is moved into postdeterminer position but before partitive formation we shall find:

(10.70)



It is clear that we can then derive the surface structure for (64) without departing from the standard generation of quantifier partitive constructions. Therefore our hypothesis runs into no difficulties with respect to the first type of both occurrence, and we may immediately move on to consider the second type, as exemplified by (65).

Before we can attempt to provide an underlying structure for this latter sentence we must first determine what its exact meaning is. There is no doubt that part of its meaning is:

(10.71) The total set of boys came

This, of course, is predicted by the claim that all is a lexicalisation of the total set, and our first step towards the underlying structure follows quite straightforwardly from this claim: the underlying structure of (65) must partially resemble (67). But what is the significance of the appearance of two? Because it

appears either at or adjacent to the head of a partitive construction there is a strong temptation to suggest that there is a coordination reduction of:

(10.72) All of the boys came and two of the
 boys came

But this runs into the objection that we shall then have to impose an ad hoc constraint on coordination of quantifiers, in order to block sequences such as:⁸

(10.73) a *Two all
 b *All many
 c *Many two

The only combination we find is all + numeral and the underlying structure must attempt to explain this in a non-ad hoc fashion.

A more appropriate paraphrase attempt seems to be:

(10.74) The total set of the boys came and
 the total set was two (in number)

It is certain that if (74) is at least fairly close to the underlying representation then we can explain why the first element can only be all. In §8.3 we discussed examples such as:

⁸ Some in some two, etc. is hardly likely to be a quantifier, among other reasons because of the ungrammaticality of the examples in (73).

- (10.75) a The number of boys is large
b The boys that kiss girls are many
(in number)

The result of this discussion was that we were able to claim that such constructions were only possible if the underlying subject was THE NUMBER; if the subject was A NUMBER the resultant sentence was ungrammatical. Let us now suppose that this analysis can be extended to SET. Then we find that the structure of (74) is only possible if the partitive quantifier is all, for in every other case the underlying subject of was two will be A NUMBER, and that is known to be ungrammatical, cf.:

- (10.76) *A large number of boys came and a
large number was twenty (in number)

(76) would paraphrase:

- (10.77) *Many twenty boys came

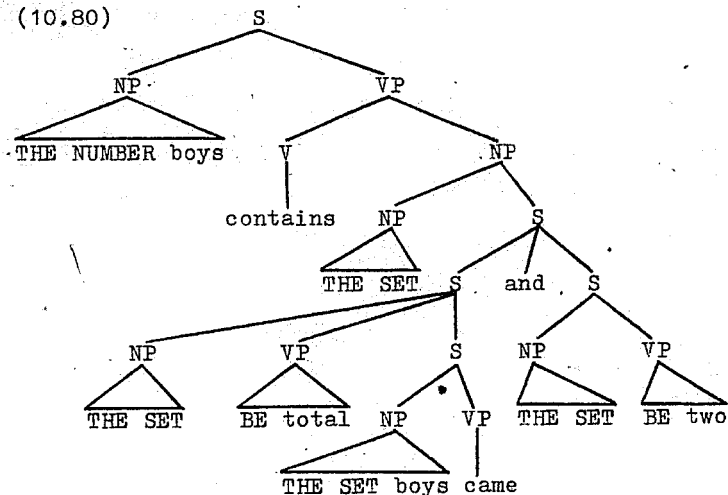
Although this explains why only all may be the first element of the pair under consideration, it does not explain why only a numeral may be the second element, since, as (75) shows, that part of (74) which contains the elements for the formation of two may also contain the elements for the formation of many, etc. In this connection, therefore, consider the following:

- (10.78) a The boys numbered two
b The boys numbered twenty
(10.79) a *The boys numbered large
b *The boys numbered many

These examples suggest an interesting hypothesis, namely that the compound existentials, with the exclusion of the numerals, must either retain the elements of underlying structure, or, if they are lexicalised (to many, etc.), then all the elements of underlying structure must take part in the lexicalisation. Thus (79b) is ungrammatical because part, but not all, of the relevant structure has undergone verbalisation. The numerals, on the other hand, must be marked to show that neither of these requirements apply to them. If we can show that the derivation from the underlying structure of (65) cannot fulfil the above requirements for the development of compound existentials other than numerals, then we shall have been able to explain why the second element must be a numeral.

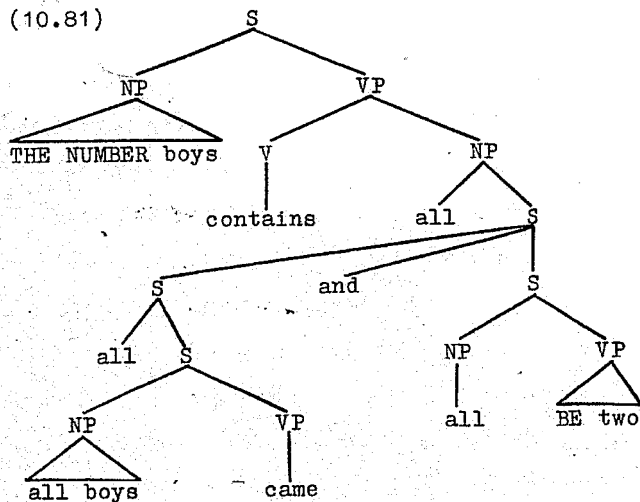
If (74) is indeed a reasonably appropriate paraphrase of (65), then the underlying structure of that latter sentence will be a combination of (67) and the structure of (75), given previously as (8.33). This combination will result in (80), where again we ignore predicate nominals:

(10.80)



Now observe the structure following quantifier lexicalisation to all:

(10.81)



It can be observed that (81) does indeed fail to meet the requirements which many imposes, for lexicalisation to all has 'robbed' (80) of any NUMBER/SET node which could combine with large. Therefore our proposed derivation does explain why the second element must be a numeral. Furthermore, since (80) - and thus (81) too - is a combination of an already existing stock of base rules and transformations, no problems arise in deriving the surface structure of (65) by means of nonrestrictive adjective formation (to give all two boys) and quantifier-lowering and partitive formation.

As we have been able to demonstrate that our hypothesis is able to handle derivations of (64) and (65) then we can also claim that it will be possible to generate (62) and (63), for all they necessarily require is the further application of Dual Copy. However it might be interesting to ask whether or not that rule should apply earlier, for this might help to generalise the possible lowering of all into different surface positions, cf. §9.4, to both. (68) supplies the necessary structure to enable the variable position of partitive all to be explained, and thus it would be disappointing if it could not be extended to both. Nevertheless we shall not pursue the question, since there are a number of minor difficulties to be cleared up, cf. Chapter 4, and we must content ourselves with the fact that our theories provide a more adequate account of both than hitherto possible.

10.5 Summary

Without prejudice to any suggestions which we might eventually have to make concerning the status of a and the in Part IV of this study, we can now claim that with the discussion of both concluded we have also concluded our study of the group of English words commonly called quantifiers. As was admitted in the Introduction and §10.1, not every quantifier has been discussed and not every quantifier construction has been examined in full detail, but as in every other matter a line must be drawn; it can only be hoped that the line has been drawn through the right points. Therefore our study has completed an important stage and it would be reasonable and perhaps useful to review briefly the main arguments and conclusions about quantifiers before we move on elsewhere.

After having first established by means of quite simple surface-biased procedures an elementary classification of quantifiers, we examined in §§7.2 and 7.3 the basic requirements which any analysis of quantifiers would have to fulfil. We were able to ascertain that base structures which closely corresponded to the eventual surface structures were quite inadequate, and thence determined that the underlying structure of some, which was taken as the most basic quantifier, has to be consistent with at least two important points: firstly,

some could not be in the same NP as its collocating NP until some point near surface structure; secondly, some ought to be analysed as an underlying noun. This position was essentially a combination of two earlier theories, one of which was the Lakoff-Carden theory that "quantifiers are derived from a higher sentence, the other of which was Jackendoff's proposal that quantifiers be at least partially analysed as nouns. On the way to this position we noted that there was no need to introduce nonlinguistic elements, such as logical operators and variables, in order to establish an adequate grammar of quantifiers. After considering and rejecting a number of possible analyses in §7.4, we concluded in §7.5 that some ought to be derived from the subject noun of a higher existential sentence, that noun denoting quantity.

In §8.1 we were able to provide further semantic support for this analysis and so in §8.2 we moved on to discuss quantifiers patterning like many. We showed that the claim by Carden (1970c) that many ought to be analysed as an underlying predicate was incorrect and that instead it had a more complex structure, being derived from an adjectival modification of the quantifier-noun underlying some. This enabled us to provide an underlying structure for the simplest occurrences of many, in §8.3, and thence we proceeded to an analysis of more complex examples involving many, including its

occurrence in postdeterminer contexts. In the same section we were able to explain the occurrence of the quite large number of quantifiers which behave quite similarly to many. Finally in §8.4 we examined the interaction of negation with this type of quantifier. Not only were we able to show why there was no similar interaction with some, but we were also able to show that the interaction which did occur could be explained by using meaning-preserving transformations alone, and thus that no further accretions to the grammatical theory were necessary.

We then turned our attention to all and noted in §9.1 that it could not be related to some in as simple a manner as was many. Nevertheless an attempt by Anderson (1973c) to show that all was indeed a more complex variant of some was discussed in §9.2, but this attempt was not found to be convincing. A quite lengthy excursus on generics in the next section was justified in §9.4 when we returned more directly to the study of all. Although we discovered that the grammar of all was to some extent confused by its structural resemblances to apparently quite different grammatical items, for example, negatives, the basic point remained that all was to be derived from an underlying quantifier-noun. The parallels with other items which are not quantifiers were seen to account for otherwise idiosyncratic and inexplicable characteristics of all. In §9.5 we demon-

strated that only a slight modification of the basic structure for all was needed to generate each. Since these two quantifiers had an underlying structural pattern which was different from that associated with some (and hence many), they had, it was agreed, some right to be considered members of a system other than the existential quantifier system. But this was not the case with every, which, it was shown, should be related to some in the manner which was discussed with reference to all in §9.2. Thus, paradoxically in view of its semantics, every was analysed syntactically as an existential rather than a universal quantifier (but we need not accept here the usual implications of that latter term).

As was stated in §10.1, this present chapter has been in the nature of a tidying-up operation, for the items under consideration have been those which did not obviously fit into established patterns. However, we were able to determine in §10.2 that any was the result of an obligatory transformation upon some in certain syntactic environments, namely those where an affective element commanded the quantifier. Furthermore, we established that the rule converting some to any was never meaning-changing nor did it require triggering by some abstract element, contrary to earlier hypotheses. Klima's (1964) analysis of no as a conversion of not + any was confirmed. We then turned our attention in

§10.3 to partitive constructions and concluded that the distinction between these constructions and nonpartitive constructions could not be handled merely by the insertion of the. Instead it was necessary to postulate a higher sentence which made explicit the relation of set inclusion. Nevertheless the basic quantifier structure was preserved and so the earlier generalisations about quantifiers still held. Finally in §10.4 we tested our hypothesis by attempting derivations for the complex quantifier both, a test which the hypothesis passed with a modicum of success.

More generally, therefore, we may conclude that quantifiers display a striking homogeneity of behaviour. Once we accept a basic division between a some group and an all group i.e., 'existential' and 'universal' quantifiers, we can relate every other quantifier to one or other of these groups quite simply. And both groups have in common the structure of a higher quantifier-noun which is, except in the case of the most basic quantifier some, adjectivally-modified. This means that what appear to be rather puzzling surface differences between various quantifiers can be related to one another in a concrete and simple manner. And together with simplifying the grammar of quantifiers it has been possible to note that the analysis proposed suggests that some valuable constraints on the power of the grammatical theory may be possible. Two points are especially

important. Firstly, we have observed that only meaning-preserving transformations upon a semantic base are necessary to generate the correct surface structure, and that more powerful or complex mechanisms, such as global constraints or rules of semantic interpretation, are to be rejected. Secondly, the stock of nonlinguistic items needed in the grammar is highly limited: we have used only tree-branching structures (which perhaps ought to be replaced by a dependency system, cf. Anderson, 1971a) and the nodes S, NP, VP and V, together with a very occasional use of binary features. It is to be hoped that even these latter features may be eventually excluded, and since we have paid very little attention to the verb phrase, it may be that V will also eventually be shown to be unnecessary. Of course at surface structure a greater variety of nodes may be needed, but that is neither surprising nor undesirable in view of the breakdown of general categories which occurs there, cf. Ross (1972, 1973). In this Part we have been able to eliminate the need for a special Quantifier node, in Part IV we shall consider how valid is the notion of an 'Article' node, which has already been the object of some sceptical remarks.

Part IV

English 'Article' Systems

Chapter 11

The 'indefinite article'

11.1 The status of the 'articles'

In this final part of our study we shall be examining, and to some extent suggesting analyses for, two members of the determiner and quantifier systems which have not been the objects of our attention in Parts II and III; these two items are those which have been commonly called the 'indefinite' and 'definite' 'articles', namely a (or an) and the. This is not to claim that a and the are not determiners, in the broadest sense of that word as defined in the Introduction; rather we are merely stating a point which has been made previously by many grammarians, that point being that a and the are not indisputably members of any of the quantificational systems discussed in Parts II and III. Indeed we were able to establish in Part I, especially §§1.2 - 1.4, that this very fact, the recognition that a and the each had idiosyncratic characteristics unshared with any quantifier, was a major factor in the establishment in vernacular grammars of 'article' as a separate part of speech in English. The syntax of a and the was such that it was almost impossible to claim either as a member of some preexisting grammatical category.

Nevertheless, the idiosyncracies of a and the were not the only factors leading to the eventual creation of an 'article' class, and we were able to note in Part I that historical accident was of at least equal importance to supposed grammatical necessity, for there were the special dogmatic demands of 'parts of speech' theory. Therefore it was not surprising to note that this segregation (and, what was worse, alliance) of a and the led to unsatisfactory analyses of these words. Rather than repeat the arguments of Part I here, it seems reasonable to accept that their validity has been demonstrated, at least as far as some underlying level of structure is concerned. This leaves the way open for a claim that there is some justification for postulating an 'article' node at surface structure, and that, of course, would not contradict the arguments of those 'parts of speech' theorists who are basically 'surface-ist' in approach. However we shall not pursue at length the correctness of such an argument, since its status is quite marginal to the aims of this present study. The purpose of the following discussion will therefore be two-fold. Firstly we shall attempt to discover how closely related to the grammar of quantifiers are the grammatical systems exemplified by a and the. Secondly, in so far as a and the demand analyses separate from that for quantifiers, as it seems, without prejudging the issues, must be the case, we shall attempt to outline

what these analyses might be. We shall not reexamine at length the notion of an 'article' system operating at some semantically significant level.

Many of the points which have been made in the preceding paragraphs are also made in a most important and enlightening study of the 'articles' by Perlmutter (1970). Thus, if we consider part of Perlmutter's conclusion we find that he says (1970:246):

"... the relation between the definite article and the indefinite article in English is quite different from what has generally been supposed. Grammarians have worked on the assumption that NP's may bear either a definite or an indefinite article, and that the two constitute some sort of opposition. If the analysis given here is correct, however, the indefinite article is simply a numeral like all other numerals, and the occurrence or non-occurrence of the definite article is a completely independent phenomenon."

Since Perlmutter's conclusion is, at least in broad outline, so similar to the points which we have attempted to establish already, it would be both foolish and churlish if we did not examine the arguments for his particular analysis of a and the in some detail. Therefore the basic approach of Part IV, and especially

Chapter 11, will be very much influenced by Perlmutter's approach, and before we consider other possible analyses we shall examine the validity of his. The most immediate consequence of this decision is that we should look at a before the, since Perlmutter has claimed a numeral-like status for that word, and this implies that its analysis is more likely to relate to the analysis of quantifiers than is any analysis of the.

11.2 Artculus numeralis

It may be recalled that in Part I we mentioned that as early a grammarian as John Wallis (1653) called a the "articulus Numeralis", stating further that it had always the same meaning as one except that it was less emphatic. Basically the same position is held by Perlmutter, as can be confirmed by the following remarks (Perlmutter, 1970:239):

"There is a variety of evidence, then, that the so-called 'indefinite article' is simply the result of a phonological rule which obligatorily converts unstressed proclitic one to an."

Before discussing what may prove to be more controversial aspects of Perlmutter's hypothesis, it is necessary to note that it seems to be undoubtedly correct that a (or an, which we take to be a phonological variant of a, occurring under easily statable conditions) is an

unstressed proclitic in the vast majority of its occurrences. The only conclusion which Perlmutter apparently wishes to draw from this is that if the is also an unstressed proclitic, then the English 'articles' will share this feature with their correlates in many other languages, cf. Perlmutter (1970:247). It may also be possible to account for the ungrammaticality of strings such as:

(11.1) *The a man who came to see me wore a
red hat

in terms of a constraint on proclitic sequences, but see Perlmutter (1970:240-45) and below, §11.3. Since neither of these points is immediately crucial to our argument, we may tentatively accept for the moment Perlmutter's claim.

In order to support his thesis that a is obligatorily derived from unstressed one, Perlmutter draws upon two basic arguments. One of these is that a has the same distribution as any numeral, for example, one, two, three, etc., except in a few cases where the deviation of /a/ is precisely paralleled by a deviation of one from that same paradigm. The other argument is that in environments where only a stressless numeral is grammatical we do not find one but rather we find a. Obviously the two arguments are closely interrelated since the second is somewhat meaningless without the first (although not vice versa); nevertheless it would be

profitable to discuss them separately as they are of a somewhat different order, the first relying purely on syntactic information, the second being primarily phonological. It might also be fair to add that perhaps the latter is slightly less certain, more impressionistic, than the former, but that should not weigh too heavily in our considerations.

It would appear most appropriate from the above facts that we first turn our attention to the syntactic arguments concerning the distribution of a. On this point Perlmutter's initial argument (1970:234) is that just as the phrases in (2) are ungrammatical:

- (11.2) a *one blood
- b *two bloods
- c *three bloods

so too is (3) ungrammatical:

- (11.3) *a blood

Now this fact can be explained in the following manner: let us accept, as we have previously claimed, that numerals are in underlying structure compound existential quantifiers. Now each quantifier has a selectional restriction or, as we claimed in §7.5, has an underlying structure of the form such that it may collocate only with a countable noun or only with a mass noun, or, in a very few cases, such as some, with either. The underlying structure of numerals, which must include a prelexical NUMBER, determines that numerals may only

collocate with countable nouns, in other words, the first group above, and hence the sentences in (2) are ungrammatical. Now if a is derived from one it too will only collocate with countable nouns, and thus (3) will correctly be predicted to be ungrammatical without there being any necessity to introduce a special (ad hoc) rule to cope with structures such as (3). So we may observe that treating a as a quantifier-cum-numeral enables a generalisation to be captured.

But then Perlmutter (1970:235) goes on to show that this argument, which is not precisely his, although it may be a step in the right direction, cannot be completely correct. Consider the following:

- (11.4) a one seventh
- b three sevenths

- (11.5) a *all sevenths
- b *few sevenths

From this we may deduce that in certain environments true quantifiers and numeral quantifiers differ crucially. And whatever the exact nature of the divide between quantifiers and numerals, it is indisputable that a is to be found on the numeral side of it; thus (6) is grammatical:

- (11.6) a seventh

So far we have amassed considerable evidence to show that the distribution of a is numeral-like, and if we add two further bits of evidence then we shall see that

where one has a distribution different from that of other numerals, then a follows one. The first point is that a, like one, occurs only with singular countable nouns:

- (11.7) a one man
b \a man
c *one men
d *a men

The second point is that in certain (near-) idiomatic expressions we find that a and one may be the only possibilities. Thus Perlmutter cites (1970:235):

- (11.8) a It was one hell of a mess
b It was a hell of a mess

Compare with these a substitution by some other numeral:

- (11.9) *They were two hells of a mess

The above is a summary of the principal points in Perlmutter's first argument for deriving a from one. It is undoubtedly a strong argument, although there seems to be good reason for not believing it to be as strong as Perlmutter would claim. But before elaborating on that point let us see what Perlmutter's second argument is and what its foundations are, for, as we have already noted, the two arguments are not independent of one another. The simplest way to state this second argument

is to quote directly from Perlmutter (1970:233-34):¹

"English noun phrases with numerals have different possibilities of occurrence, depending on whether the numeral or the noun is stressed:

- (11.10) a There are only two boys in the room, not five
b There are only two boys in the room, not any girls

But the numeral one does not occur unstressed before a noun; instead we find the indefinite article a:

- (11.11) a There is only one boy in the room, not five
b *There is only one boy in the room, not any girls
c There is only a boy in the room, not any girls

This suggests that English has a rule which obligatorily converts unstressed proclitic one to an, with the final n later dropping before a consonant."

Other cases which Perlmutter cites in favour of his position are those such as (8a) and (8b) where stress

¹ The numbering of the examples in the quotation below is amended to follow the sequence of this chapter.

placement on the numeral is, he claims, optional, and thus both variations are possible. However let us leave aside for the moment such examples and concentrate our attention on the quotation above.

One major difficulty in assessing the validity of the argument is that judgments of acceptability concerning the examples given by Perlmutter are variable. Thus many speakers, including myself, would reject not only (11b), but also (11c). And the explanation for this is of some consequence. Consider the following:

- (11.12) a There is only one bláck cow in
the field, but there are five
brówn ones in it
- b *There is only a bláck cow in the
field but there are five brówn
ones in it
- c There is only one bláck cow in
the field, and there aren't any
brówn ones in it
- d There is only a bláck cow in the
field and there aren't any brówn
ones in it

This quadruple poses a number of difficulties for Perlmutter. Firstly we may note that in all four sentences one and a occupy relatively unstressed positions. Now if unstressed one is obligatorily converted to a, then only (12b) and (12d) should be grammatical, which is

false. However one could argue that one receives enough stress to protect it from conversion to a; this is possible since Perlmutter does not clearly define what he means by "unstressed". But in that case only (12a) and (12c) should be grammatical. As both predictions are incorrect, the only solution would appear to be that Perlmutter's rule be made optional rather than obligatory, at least if we still wish to keep such a rule at all. Now let us compare (12c) and (12d) with (11b) and (11c). The major difference between the former and the latter is that in the latter the existential of the second coordinate sentence remains, whereas in the former it is deleted. Now note that the deletion is peculiar, for the deleted existentials (~~there are~~) are not identical to their left-hand partners (there is). This would appear not only to violate deletion conditions but the resultant sentences potentially break concord rules, cf.:

(11.13) *There is not any girls in the room

It seems plausible to suggest that the explanation for the rejection of (11c) by some speakers is merely a matter of concord. It is obscure why for Perlmutter (11b) is ungrammatical and (11c) grammatical, but we may observe that concord violations are more acceptable in informal speech; thus (14) is better than (13):²

² (14) is acceptable only in the most informal styles of speech.

(11.14) ??There isn't any girls in the room
It may be that (11c) is regarded as less formal than
(11b).

Our position at this point is approximately as follows: the evidence of (11b) and (11c) is insufficiently clear-cut to provide convincing proof of the validity of Perlmutter's derivation of a from unstressed one, but even if we were to accept it, the data in (12) shows that Perlmutter's rule cannot in any case be obligatory, at best it is often optional. And here the so far unexplained ungrammaticality of (12b) is crucial. The semantics of (12b) are reasonably clear: it states that the only cow in the field is a black cow but that there are also five brown cows there; thus it is a contradiction and ungrammatical. On the other hand, (12a) is not a contradiction and not ungrammatical. The probable reason for this is that only contains some kind of operator and that in (12a) the scope of that operator extends over one but not over the existential as a whole - this is quite possible given the structure proposed for numerals in §8.3 - whereas in (12b) the scope of the operator extends over the higher existential wholly, or perhaps only over that part of the coordinate existential which does not quantity-refer, again in terms of our previously proposed structures for numerals. It does not yet seem fully determinable which of these alternatives is correct, although we shall be

able to discover below that there is a quite simple solution.

Yet the details above scarcely matter, for the important point stands out clearly: (12a) is grammatical and non-contradictory, (12b) is ungrammatical and contradictory. This being the case, (12a) and (12b) must differ in meaning. But the only surface difference between the two is that where the former has one the latter has a. If we are to preserve the hypothesis that a is derived from unstressed one then we shall have to adopt one of the following two solutions. Either we shall have to claim that the phonological conversion rule will have to be sensitive to some kind of global constraint pertaining to differences in underlying structure of the type described in the previous paragraph, or we shall have to accept that this conversion rule, which we have already observed to be optional, is also meaning-changing. We have already agreed with, and shown reasons for doing so, the claim first made by Katz and Postal (1964:32) that transformations never change meaning, and in §8.4 we cast a certain amount of suspicion on global rules; furthermore, the type of global rules which might be necessary here seems very implausible indeed. We may therefore claim with some confidence that the evidence of (12a) and (12b), together with the other evidence presented above, is such that Perlmutter's conversion rule is most unlikely to be correct.

Before we leave the question of the role of stress there is one minor point which is worth considering.

Let us continue to accept that a is created by the type of phonological rule suggested by Perlmutter. If that is the case, then we must have the following rule sequence:

- (11.15) i) stress assignment rules
ii) one [wɔn] > an [æn] (> a [æ])
where stress is less than some value n.

This sequence poses no difficulties as such, but it will be recalled that not only are there unstressed but there are also stressed variants of the 'indefinite article', namely [ej] and [æn], cf. Quirk et al (1972:136) (and it should be noted that the derived forms in (15 ii) above must also undergo vowel reduction at some stage). These stressed forms occur under conditions of emphasis. Thus (15 i) and (15 ii), if more fully formulated, must be followed by two further rules:

- (11.15) iii) emphatic stress assignment rules
iv) [ə] > [ej] (a)
[ən] > [æn] (an)
where stress is greater than some value m.

But it is clear from Perlmutter's examples, cf. (10) and (11), that he believes emphatic stress to be assigned before reduction of one to a. Thus even if it were possible to have 'standard' stress assignment rules - for example, the Main Stress Rule cited in Chomsky and

Halle (1968:72) - ordered at one point in the grammar and 'emphatic' stress rules ordered at a later stage (which, in this case, would have to be after vowel reduction rules), which is far from being established, this would be of no consequence, since Perlmutter's position demands that emphatic stress assignment occur both before and after reduction of one. At first sight this might appear to be an example of an ordering paradox, as described by Newton (1971), but this is not so. Newton is describing cases where some rule must apply both before and after some other rule in order to generate correct outputs, and this is to a large extent diachronically justified; Perlmutter is hypothesising a derivation in which in the first instance emphatic stress is deliberately incorrectly assigned in order to derive [ən] from underlying /wʌn/, after which the stress is reassigned correctly, in order to generate [ən]. This is a theoretically quite illegitimate practice, which has to be rejected. The conclusion to be drawn is that, quite apart from the serious syntactic objections we have raised, there is a grave objection within phonological theory to Perlmutter's hypothesis. Consequently it can scarcely be considered to be at all viable.

11.3 The inflexional status of 'a'

Since we have now been able to show that we cannot derive a from a vowel reduction rule operating on one, and thus that a cannot be analysed as the unstressed variant of one, let us now once more turn our attention to the distribution of a in order, in the first instance, to ascertain whether there are in fact any unexplained distributional differences between one and a, and thence to see if there is some non-phonological explanation for the rather close distributional parallels which undoubtedly do pertain. The great majority of facts concerning the distributions of one and a are not controversial, and therefore it would be best if we were to restate these briefly before turning our gaze upon the less settled areas. As the evidence of examples (2) - (7) shows, a has many of the characteristics of a singular quantifier-numeral, being ungrammatical in collocation with, for example, a mass noun or a plural count noun. This elementary point, it is important to note, is not in dispute, and thus any possible analysis must recognise it.

The problems arise when we consider cases where only one of the items one and a is grammatical; we have seen two examples of this so far. The first of these is in (1), where the sentence would be grammatical if one replaced a. Here, however, we were able to suggest that

this might be due to some surface constraint. Whether or not such a constraint is necessary we shall discuss below, but it will serve as a working hypothesis which keeps the area of dispute to a minimum. The second example concerns the ungrammaticality of (11b) and the alleged grammaticality of (11c). If this contrast exists it does not appear to be explicable even in Perlmutter's terms, and we have attempted to demonstrate that whatever differences there may be between the two sentences, they are probably due to factors largely irrelevant to the question at issue. Henceforth we shall therefore largely ignore both these examples and concentrate on the problems posed by further data.

One caution which it is absolutely necessary to utter before we proceed is that it must be recalled that we have already seen that it is not possible to derive a from unstressed one, as Perlmutter would wish us to do. Thus his claim (Perlmutter, 1970:238-39) that:

(11.16) He's a doctor

is derived from:

(11.17) *He's one doctor

and that (17) is only ungrammatical because in such contexts "the stress cannot fall on a numeral", fails because of the implausibility of the stress rule rather than because of any failure of plausibility in the supporting examples, given below as (18), which Perlmutter cites:

(11.18) a They are six doctors

b *They are six doctors

Yet we cannot ignore the examples immediately above. Note firstly that even if in such constructions the numeral must be unstressed, a may be stressed:

(11.19) He's á doctor (but there are many others, better qualified)

Therefore if, notwithstanding our previous arguments, a is analysed as a numeral, (19) will have to be treated as an ad hoc exception. Secondly, Perlmutter's arguments all tend to suggest that if in a given language the numeral one and the 'indefinite article' are in the phonological relation of stressed and unstressed variants of a single underlying form, then no rules other than phonological should be needed to account for the distribution of the equivalent of a in that language. We might even go further, although this is speculative, and state that if a given language has an equivalent of a, then its form will be that of an unstressed variant of the numeral one in that language. This, it seems to me, is one of the strengths of Perlmutter's hypothesis. It is of some interest, therefore, that quite unexotic languages such as French, German and Dutch bear out this very prediction. But precisely in the case of (16), where English employs a, French does not employ un, nor German ein, nor Dutch een:

- (11.20) a He is a soldier
b Il est soldat
c Er ist Soldat
d Hij is soldaat

Unless there is some kind of unmotivated a-deletion rule in the latter three languages, which would operate only in the restricted context of structures like (20), Perlmutter's hypothesis will predict incorrect surface structures. There may be other objections to the specific analysis of (16) given by Perlmutter,³ but these points above would appear to have already ruled his theory out of court.

We have above some evidence that a ought not to be analysed as a numeral; perhaps more convincing evidence is to be seen in examples such as:

- (11.21) a Many a thesis lies unread, its
pages uncut
b I have seen many a production of
Othello, but none so misguided as
that last night

³ One possible objection is that (16) and (18a), whether or not doctors is strongly stressed, are not syntactically nor semantically equivalent. My intuitions rather fumblingly tell me that this is the case, but I have found no certain evidence to confirm or disconfirm this point.

The sentences in (21) have been chosen to suggest that many a N constructions are somewhat archaic (or simply arch), but nonetheless they are still grammatical, and ought to be analysable. Yet if a is a numeral this is virtually impossible, for we have been able to show, in §10.4, that the only permissible quantifier + quantifier sequences are of the structure all + numeral.⁴ This is confirmed by the slightly misleading statement of Quirk et al (1972:143) that "cardinal numbers and quantifiers are mutually exclusive". If we are both to uphold an otherwise valid generalisation and to generate structures like (21) without recourse to ad hoc exception statements, then we have no option but to regard a as something other than a numeral (or, even, a quantifier). It is of no relevance that the ungrammaticality of:

(11.22) *Many one thesis

is, at first sight, explicable in terms of stress rules, parallel to (17), for the simplest explanation of the contrast between (21) and (22) is that in the former there is not a quantifier + quantifier sequence, whereas in the latter there is.

Another apparently good counter-example to Perlmutter might be thought to be:

⁴ We exclude from consideration possible numeral + numeral sequences such as one/a hundred and twenty, which would appear to be better analysed as single items.

(11.23) A few boys came to the party
The grammaticality of (23) as opposed to the ungrammaticality of (24):

(11.24) *One few boys came to the party
could be attributed to the lack of quantifier + quantifier-sequence in the former. But (23) is probably not a genuine counter-example. If we also consider:

(11.25) A little whisky will get Bill drunk
we can observe that the collocating noun must be either plural - as in (23) - or mass - as in (25). In neither case should a be grammatical. The only reasonable explanation seems to be that a few, a little ought to be regarded as single lexical items, in which a is not to be identified with the item a which occurs elsewhere. In other words, we have a semi-idiomatic expression. I would wish further to suggest that the occurrences of a in examples (8a) and (8b) are wholly idiomatic. One reason for this is that the phrases are unalterable, cf. (9). Of course it is of some interest that both (8a) and (8b) are acceptable, and it is certainly true that Perlmutter's hypothesis, if it were valid, would be able to account for the existence of the variants in a much simpler and more general way than might be the case with some other theory; however it seems unreasonable to

place too much emphasis on an idiomatic construction.⁵

We are now, therefore, in a position to reiterate that Perlmutter's hypothesis that a is derived by some phonological rule from one is not only insufficiently justified but also impossible. Thus the sequence of rules in (15), demanded by his theory, is theoretically incorrect, and grammatical sequences of quantifier + a would be predicted to be ungrammatical because quantifier + numeral sequences are in general ungrammatical. Thus we must search for some other analysis of a. To help us in our search let us once more review the contexts in which a is acceptable. Firstly, if the sequence Quant + X + Nplural is acceptable, then so too is a + X + Nsingular (it is possible to phrase this more precisely, but the consequent statement is lengthy and not wholly necessary). Secondly, if the sequence Numeral + X + N is grammatical, then a + X + Nsingular is grammatical. We have seen that the observations made by Perlmutter are basically the same as this, but that nevertheless he is not able to account for all cases of non-idiomatic a within his framework. The outstanding cases can be covered by the statement that if the sequence X + Nsingular is not internally ungrammatical and if mass

⁵ Similarly we shall not discuss obviously idiomatic usages of a as in of a truth, twice a day, cf. Christophersen (1939:126, 135-36).

nouns are not regarded as [+singular], then any sequence a + X + Nsingular is grammatical. This third statement, of course, is powerful enough to cover all the cases already dealt with by the first two statements. And since it is so powerful it should be easily disconfirmable if it is incorrect. We shall see below that some minor modifications are necessary, but first it is necessary to discuss potentially serious counter-examples.

One such example which ought to be examined is mentioned by Perlmutter (1970:238):

(11.26) *A boy is tall

He suggests that (26) is unacceptable because if the subject NP of a stative predicate (such as is tall) contains a numeral then the stress must go on the numeral. If Perlmutter's arguments concerning stress had been correct, but we have already seen that they are not, then the unacceptability of (26) would follow. But in any case there are examples rather similar to (26) which are acceptable, such as:

(11.27) A beaver is furry

The distinction between (26) and (27) does not seem to be wholly explicable in purely linguistic terms. All that we appear to be able to say is that (27) is interpretable in generic terms since we know (for some reason or another) that beavers are habitually furry, whereas (26) is not so interpretable since boys are not

habitually tall. From this rather vague statement we can deduce that nongeneric unquantified NP's cannot freely appear in subject position if the predicate is stative. Why might this be? If we consider two further sentences with nonstative predicates, we find that the situation is not quite as described above:

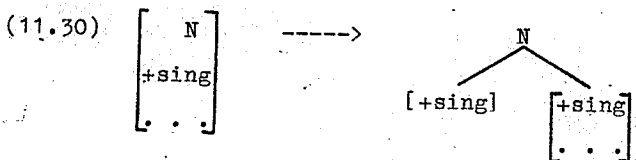
(11.28) *A boy flies to the moon.

(11.29) A beaver builds dams

On comparing (26) and (28) against (27) and (29), the difference now appears to be simply that the former pair are, considered as generic sentences,⁶ empirically false, whereas the latter are empirically true. The former pair are therefore reinterpreted as nongeneric sentences. However, it would appear that because neither (26) nor (28) is grammatically incorrect, but merely empirically false, such reinterpretation is not permissible. It seems doubtful that the status of (26) and (28) should be predicted by grammatical rule, and therefore we make no attempt to account formally for such examples. Furthermore, it has been pointed out to me by Geoff Pullum (personal communication) that Perlmutter's claims about the ungrammaticality of (26) and (28) is disputed by many speakers. This only weakens Perlmutter's position.

⁶ We ignore the possibility of an interpretation of (28) - at least - as an historic present.

Another possible type of counter-example is raised by the fact, already observed, that the a sequences are unacceptable; similarly one a sequences are rejected. In the former case Perlmutter relies on some constraint on proclitic sequences, and in the second case he has to have a rule which he himself admits (1970:236, fn. 6) is quite possibly unique. But there is in fact an extremely simple way to generate a only in those contexts where a singular countable noun occurs and yet obviate the necessity for such deletion rules as are here needed by Perlmutter. Let us accept that at or near surface structure nouns contain a feature complex which contains the information necessary for morphological rules such as concord. We have argued previously, for example in §10.2, that the use of features such as [\pm affective] is at best infelicitous, but it seems not only unreasonable but indeed impossible to argue against the use of low-level morphological features. Given the presence of such a feature complex, it is clear that one feature it must contain will be [\pm singular] (in the case of count nouns). Let us propose, therefore, a segmentalisation rule, of the type first proposed by Postal (1966:184-86), which segments the feature [\pm singular] by copying that feature onto a left-sister of the relevant N in the following manner:



But (30) only gives the principle of the necessary rule, and we need a more precise formulation, which will show clearly that a is segmentalised to a position to the left of any 'adjective' (including, vacuously, post-determiner quantifiers derived from an adjectival source) and to the right of elements derived from some other source, such as a higher sentence. Now it is interesting to note that the only elements which will appear to the left of a will be either quantifiers or deictics; further these items will be marked for the feature [+singular].⁷ It will be recalled that the purpose of the segmentalisation rule is to create a segment specifically marked [+singular]. But if there is a node already present and which is explicitly marked as [+singular], for example the node which dominates one, then there is no need to have another rule to create an explicit marker of [+singular]. Thus our segmentalisation rule ought to be of the form:

⁷ The proposed underlying structure for quantifiers implies a feature [+singular], and deictics need a similar feature, if only to account for concord relations and morphological change in the demonstratives.

$$(11.31) \quad [{}_{NP}^X + Y + \begin{bmatrix} N \\ +sing \end{bmatrix} + Z] \text{ -----} \rightarrow \\ [{}_{NP}^X + [+sing] + Y + \begin{bmatrix} N \\ +sing \end{bmatrix} + Z]$$

Conditions:

- i) X, Y and Z may be zero
- ii) If X is present it may not dominate [+sing]
- iii) If Y is present it dominates [+adjective]⁸

One obvious consequence of rule (31) is that the a and one a sequences will not be generated by virtue of the duplication of the feature [+singular], which would otherwise result. This seems intuitively satisfactory. But another more interesting fact emerges, concerning examples of the type shown in (21a) and (21b). In our discussion of these sentences we noted that the type of explanation given by Perlmutter could not possibly be applied to many a sequences, for reasons quite unconnected with his basic proposition regarding stress. The difficulty was that these sequences involved an apparent quantifier + quantifier sequence which is not permitted. Now, however, no such problem arises. Since many is derived from underlying A LARGE NUMBER it must be

⁸ Alternatively, condition (iii) could be handled by a global rule, if that were desirable.

[-singular]. But the collocating noun is [+singular]. Therefore, as with other [+singular] nouns, we expect segmentalisation to take place, and since many is not itself [+singular] there is nothing to block the operation of the rule, and thus such sequences are freely generated, and need not be handled as exceptions. Admittedly, we have not explained the collocation of many with a singular noun, nor shall we do so. Clearly the collocation is connected with the distributive force of many, a fact already recognised by Wallis (1653:72), but a formal explanation is at best problematical.

Finally, there is theoretical point of some interest. It is well known, see Fries (1940), that a major characteristic of the Middle English period is the rise of 'analytic' forms to replace the 'synthetic' structures of Old English. Primary amongst these analytic forms are periphrastic expressions of surface 'case' in the nominal phrase and of tense and mood in the predicate. Although no statement of the cause of this change can fail to be controversial, from one point of view we can see the periphrastic forms as fulfilling the same function as a no longer sufficiently unambiguous morphological system once did. One example of this change is the rise of a periphrastic genitive which replaces the older inflectional genitive. Fries (1940:206) gives figures which show that in 1200 only 6.3 % of genitives were periphrastically formed (i.e., with of +

noun), by 1250 the proportion had risen to 31.4 %, and by 1300 to 84.5 %. It is notable that the rise of similar forms in other contexts occurred at similar dates, cf. Fries (1940), Mustanoja (1960:74-76; 95-97; 602-5). In the light of such evidence it is not implausible to suggest that the rise of a is a similar move towards periphrasis, on this occasion to express number, for this is the type of analysis which (31) implies. It is, therefore, of considerable interest to note that Christophersen (1939:103-7), in his historical survey of a, places its rise at exactly the same point chronologically as Fries places the rise of the periphrastic genitive. Such chronological coincidence can never, of course, be a definitive proof, but it is no disadvantage that our synchronic analysis of a could well be of some assistance in a diachronic explanation of the roles of a.

11.4 Two residual problems and one conclusion

The first problem which I wish to discuss here has already been mentioned in the previous sections, where, however, no adequate solution was offered. The problem centres on the fact that (12a) is grammatical whereas (12b) is not (the examples are repeated here for convenience):

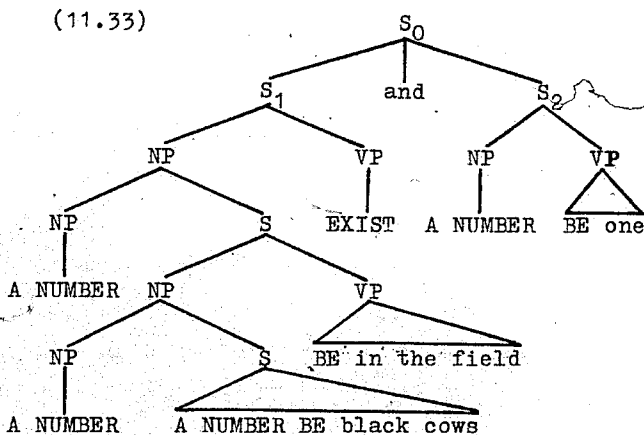
- (11.12) a There is only one black cow in
the field, but there are five
brown ones in it

- b *There is only a black cow in the field, but there are five brown ones in it

If we attempt to paraphrase (12a) we obtain something like:

(11.32) The number of cows in the field which are black is not more than one, but the number of cows in the field which are brown is five .

Taking only the first part of (12a) and omitting only, we would find the following underlying structure:



The second part of (12a) will only differ from the first in the substitution of five for one and brown for black. The only point that now remains is the status of only. If we follow Lakoff (1970d:393) in his analysis of only, then we may maintain that the paraphrase in (32) is correct in so far as it shows that only modifies the

the quantifier-numeral one. It is unclear what the precise underlying structure of only is, but it is clear whereabouts in a structure such as (33) it ought to be found, namely either immediately dominating or immediately dominated by S_2 . Given that, there will be no contradiction between the conjoined sentences of (12a) in underlying structure, which is the desired position.

Now let us consider (12b). If we attempt a paraphrase of that sentence something rather like the following is obtained:

(11.34) *There is no cow in the field which
is not black and there are five
cows in the field which are brown

How can we account for this paraphrase and the fact that (12b) is contradictory? From our discussion in §11.3 we must deduce that in the first coordinate sentence of (12b) there is no underlying quantifier, since a is a purely morphological creation. In §9.3 we saw that it was necessary to modify the original structures proposed in §7.5 in order to account for nongeneric sentences without an overt quantifier, such as:

(11.35) Boys kissed the girls

The underlying structure of the nongeneric interpretation of (35), it was suggested, would still involve a higher existential, but that existential would not have a quantifier-noun as its subject; rather the subject would simply be ones, as in (36) below. It should be observed

Whether or not only is analysed into several parts, for example, along the lines of:

(11.42) There are no cows which are not ...
it is indisputable that it will act as an operator over the higher existential. But in the second coordinate sentence there is no only which would so perform, and thus we obtain the contradictory:

(11.43) *There are no cows ... and there are
cows

It is this contradiction which lies at the root of the ungrammaticality of (12b), and it is explicable only in terms of an analysis which denies a the status of a compound existential such as is accorded to one. Because one is a compound existential, only in (12a) does not contain within its scope - does not command - the sentence containing EXIST, and hence no contradiction arises; rather, only commands the 'adjectival' element in one. But in (12b) only must command EXIST. We may note that a similar explanation holds in the case of the simple existential some, for in:

(11.44) *There are only some black cows in
the field, but there are five brown
ones in it

we encounter the same contradiction as with a.

The second point which it seems useful to consider before concluding our study of a is the behaviour of so-called generic a. In a lengthy footnote, Perlmutter

(1970:239-42) presents evidence which, he claims, shows that generic a does not have the distribution which would be predicted if it were derived from the same source as nongeneric a, i.e, for Perlmutter, from one, for us, from a morphological segmentation rule. Perlmutter suggests that it might be more correct to derive generic a from some any one sequence. The validity of Perlmutter's suggestion depends crucially upon the grammatical distribution of generic a, which we examine below, and its necessity stems from the fact that whereas a sentence such as (46) may be generic, (47) has only a nongeneric interpretation:

(11.46) A beaver builds dams

(11.47) One beaver builds dams

Naturally this forces a different underlying structure for generic a only upon the grammarian who agrees with Perlmutter that nongeneric a is derived from one. We shall see below that the analysis which we have presented of a produces no such difficulties.

Let us firstly consider, however, the distribution of generic a. Perlmutter's claim is that the (underlying) subject of a generic sentence belongs to one of four types, exemplified below:

(11.48) a The horse has four legs

b Horses have four legs

c A horse has four legs

d Any horse has four legs

He then claims that types (c) and (d) are restricted in their distribution in generic sentences, and that the restrictions are identical for the two types. Therefore it is most economical to derive generic a from a source containing any. As far as we are concerned we ought to note that if types (a) and (b) have a distribution different from that of type (d), this is no problem, since we have already proposed quite different underlying structures for such examples, cf. §9.3 and §10.2 respectively. As a result we need only consider those examples where Perlmutter alleges that the (c) and (d) types are both ungrammatical, although the (a) and (b) types are both grammatical. Unfortunately, of the five environments which Perlmutter discusses, it seems to me (and to most informants whom I have questioned) that only one unambiguously supports Perlmutter in showing an identical distribution for a and any.⁹

The first of these five involves conjoined generic sentences, as in:

- (11.49) a A beaver and an otter build dams
b *Any beaver and any otter build dams

⁹ In order to avoid prejudging the issues, examples (49a), (54a) and (56a) have not been asterisked, but this does not necessarily imply that they are grammatical. The reader is referred to the subsequent comments for judgments of grammaticality.

Perlmutter claims that (49a) is, like (49b), unacceptable, but this is not the response of most informants, and any hesitation in reply is ruled out if both is inserted:

(11.50) Both a beaver and an otter build dams
Or if there is no conjunction reduction and no other transformation (of relevance), then generic a is fully acceptable:

(11.51) A beaver builds dams and a sparrow
builds nests

On the other hand, generic any is at best very dubious in such cases:

(11.52) *Both any beaver and any otter build
dams

(11.53) ??Any beaver builds dams and any
sparrow builds nests

Thus it would appear that Perlmutter's facts are simply incorrect on this point. This also seems to be the case with his second group of examples, where passivisation has taken place:

(11.54) a Dams are built by a beaver
b /*Dams are built by any beaver

Again there seems to be no justification for asterisking (54a), as Perlmutter does; certainly there are many similar examples which are fully acceptable, e.g.:

(11.55) a Pigs are cared for by a swineherd

- b Shoes are repaired by a cobbler,
hats by a milliner and dresses by
a seamstress

The third case presented by Perlmutter is seen in the following examples:

(11.56) a A beaver\built dams in prehistoric
times

- b *Any beaver built dams in prehis-
toric times

Here it does indeed seem to be the case that, as Perlmutter says, (56a) is ungrammatical, but what is most interesting is that if we prepose the adverbial, then both sentences are grammatical, but only (57a) has a generic interpretation, the other sentence being purely descriptive:¹⁰

(11.57) a In prehistoric times a beaver
built dams

- b In prehistoric times any beaver
built dams

A fully explicit analysis of the contrasting grammaticality of (56a) and (57a) does not appear possible, but

¹⁰ It is possible that not only is (57b) grammatical but that (56b) is grammatical also. If this is the case, then our argument is strengthened, but the question is not pursued here, where we confine ourselves to discussion of generic a.

an investigation using the notion of perceptual strategy, cf. Bever and Langendoen (1972), might be useful. As Perlmutter (1970:241) says, verbs in the past tense do not ordinarily yield generic sentences. Now it is clear that in a sentence such as:

(11.58) A beaver built the dam
that a is nongeneric. But if we revert to dams, this seems to be grammatical only in a generic interpretation, and even then it is distinctly unhappy:

(11.59) ??A beaver built dams

I would suggest that this is because the generic variant is in some sense marked, and that some overt clue - a perceptual device - is strongly preferred in order to point out the genericness. This clue is provided by an adverbial such as in prehistoric times, but in (56a) it comes too late in the sentence; only when it is preposed is the 'signal' sufficiently strong, and early enough, to enable the generic interpretation to be picked up. Obviously this is a very tentative and informal suggestion, but it does appear plausible and it does provide an explanation for the singular grammaticality of (57a), which Perlmutter entirely fails to explain. But it is in the fourth case that we may find most support for Perlmutter's position. He claims that generic a and generic any are ungrammatical in of-phrases, as instantiated by:

(11.60) a I said of a beaver that it builds
dams

b *I said of any beaver that it
builds dams

Perlmutter seems to be correct in claiming that (60a) is unacceptable, but this does not always appear to be the case. Thus (61):

(11.61) ??I said of a beaver that it once
built dams but that it no longer
does so

seems to be slightly preferable, although substitution by any is impossible. However the situation here is too unclear to permit a satisfactory explanation.

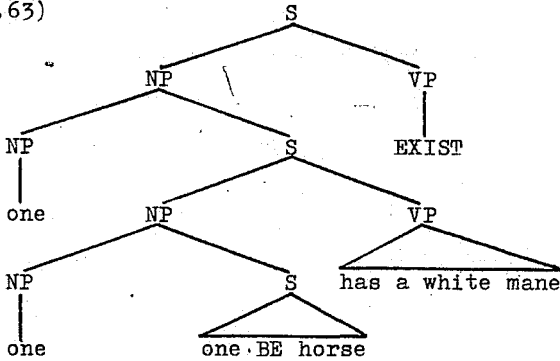
Despite the uncertainties surrounding the fourth type of environment we have been able to establish that the distributions of generic a and generic any are not identical. Obviously this poses severe problems for Perlmutter's hypothesis, but rather than attempt to find out whether it can be satisfactorily modified, let us consider whether there is an alternative solution consonant with our proposals in §11.3. Once we have done that we shall examine the fifth case given by Perlmutter. It may be recalled that in §9.3 it was claimed that a common property of generic sentences was the absence of a higher existential sentence. Now the derivation of

a nongeneric sentence such as:¹¹

(11.62) A horse has a white mane

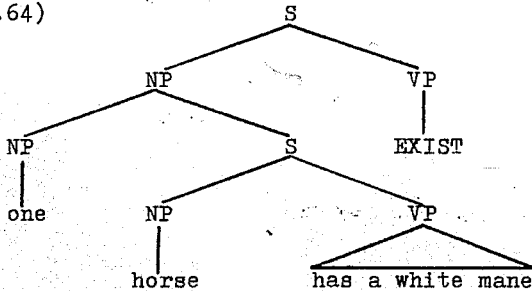
is approximately as follows: the underlying representation is:

(11.63)



Then raising of the predicate nominal occurs, giving:

(11.64)



¹¹ It might be doubted that (62) is grammatical in a nongeneric interpretation, but this is not so; however horse must normally be strongly stressed. Compare the discussion of examples (26) - (29), above.

At this stage the higher existential may be preserved, just as in the parallel derivation of quantifiers, in which case (65) results:

(11.65) There is a horse (which) has a white mane

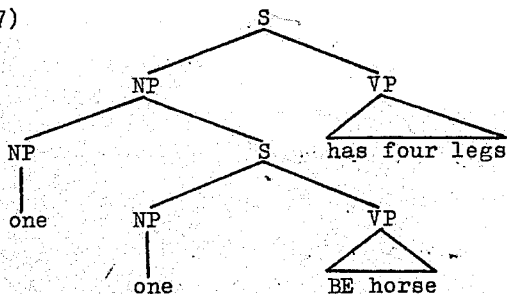
To derive the surface structure of (62) the existential is lowered and the morphological production of a follows, as indeed it does in the case of (65).

Now if we wish to derive generic instances of a we need only assume that as in other cases of generic sentences there is no higher existential, which would imply that the underlying structure of a generic sentence parallel to (62), say:

(11.66) A horse has four legs

would be:

(11.67)



Such a structure has the advantages of paralleling the lack of an existential in other generic sentences and yet retaining a single type of source for a. It is also distinctive in structure from the types of generic

sentence exemplified by (48a) and (48b) and therefore different grammatical distributions are to be expected and will be analysable. Also, the structure of any, as we have observed in §10.2, is quite different again, and this too is desirable in the light of the evidence. A further advantage is that the underlying structure of (67), as opposed to that of (63), explains why (68), like (65), cannot have a generic interpretation:

(11.68) There is a horse which has four legs

This analysis of generic a looks, at least at first sight, to be preferable to Perlmutter's, since it makes use of quite independent generalisations and retains the single morphological rule for the creation of a. But before making that claim more definite we ought to consider the fifth case which Perlmutter claims supports his evidence. The type of paradigm is as follows:

- (11.69) a The beaver is increasing in numbers
b Beavers are increasing in numbers
c *A beaver is increasing in numbers
d *Any beaver is increasing in numbers

Perlmutter states (1970:240):¹²

"A fourth piece of evidence for deriving generic a from any comes from the inability

¹² Although this is the fifth piece of evidence we have discussed, the original ordering was slightly different, which accounts for Perlmutter's use of "fourth".

of both to occur with predicates which require non-conjoined plural subjects and which predicate something of the entire group or class rather than of any individual in it. Not only the plural generic NP ..., but also the definite singular generic NP ... can occur with predicates of this kind."

Now it so happens that the underlying structures we have proposed for sentences of the type exemplified by (69a), (69b) and (69c) correlate exactly with Perlmutter's observation. The underlying structure of (69c) involves only a [+singular] NP with no reference to a class or set (the fact that a horse is interpreted generically, hence giving the implication of set reference, is purely a matter of the absence of a higher existential), and therefore the sentence ought to be ungrammatical since the predicate demands reference to more than one object or to a set of objects. On the other hand, it will be remembered, the underlying representation of (69a) and (69b), as developed in §9.3, includes an underlying prelexical SET, which was designed to make explicit the fact of set reference. Therefore this structure contains the element demanded of the subjects of predicates such as increasing in numbers, and the analysis predicts the grammaticality of the sentences under discussion without any modification. This fifth case of generic a, therefore, poses absolutely no problems for our analysis.

We are therefore justified in claiming that the underlying source of a is identical to that for the plural inflexion -s, save that the structure from which a derives contains the feature [+singular], as opposed to [-singular] for -s. In both cases we need a morphological rule which will segment out of the collocating noun that contrastive feature, in the case of a leftwards, in the case of the plural rightwards. Thus, in order to derive a, and this is a point of some importance, no rule is needed which is not independently paralleled elsewhere in the grammar. As we were able to observe, this was not the case with the only other proposal, that of Perlmutter (1970), which is able to dispense with the notion of an underlying 'Article' node. That Perlmutter's analysis, like ours, needs no such node in deep structure is of course greatly to its advantage, since it means that we are able further to constrain the types of structure generated by phrase structure rules or their equivalents. The crucial difference between Perlmutter's analysis and ours is that we have claimed that a is not a member of the English quantifier-numeral system, despite some apparent affinities. The difficulties, however, are such that to posit similar underlying structures for a and, say, one would require so many ad hoc constraints that the generalisation which would have been achieved would have to be regarded as patently false.

In this regard it is interesting to recall that it was rare within 'parts of speech' theory, cf. §1.4, to classify a alongside the quantifiers (which latter group, of course, was a source of considerable difficulty). In view of the evidence which we have considered, this decision seems to have been correct. On the other hand, we observed that it was common - indeed, almost exceptionless - to consider a and the as a complementary pair. In our earlier discussions we criticised this at some length. Now, by our implicit contrast between a and the plural morpheme, we have virtually excluded any possibility of a and the being in such direct contrast. It therefore behoves us, before we conclude this study, to attempt at least a preliminary analysis of the which should be sufficient to establish its status in some tentative fashion, although it cannot be hoped that we shall provide a definitive solution to such a recalcitrant object of study.

Chapter 12

The 'definite article'

12.1 The status of 'the'

Notwithstanding the considerable puzzlement to which the behaviour of the gives rise, there can be little doubt as to its basic grammatical status. In other words, the difficulties in analysing the are not due to the fact that it stands alone, having no associations with any other linguistic item, rather they are due to the fact that the larger class of which the is a member is as a whole difficult to analyse formally. But even if a primary classification of the is relatively simple, it is necessary to discuss it at least briefly, because if we are fairly sure of such a classification then there will be rather more evidence that might help in solving the larger problems still to be faced. In the light both of our earlier discussions and statements by previous grammarians about the, it seems reasonable to suggest three possible classifications of the: it might either be a quantifier, an 'article' or a deictic. Let us now examine the attractiveness of the competing claims.

Since the greatest part of this study has been concerned with the grammar of quantifiers, it is perhaps most convenient to start by considering whether the

might be regarded as a quantifier. But this need not detain us for long, for the evidence against any such position is extremely strong, indeed so strong that a discussion of three quite simple matters will show that the behaviour of quantifiers and the behaviour of the contrast so sharply that a common analysis would not only be fruitless, but also misleading. The first of these points concerns the fact that in §10.4 we were able to show that the underlying structure of quantifier + quantifier sequence was universal quantifier + numeral. In Chapter 11 we were further able to point out that some apparent counter-examples to this hypothesis were false. Now if the were a quantifier the number of grammatical quantifier + quantifier sequences would be greatly increased, for structures of the type in (1) are fully acceptable:

(12.1) The many boys came to the party

We have already suggested an analysis for such structures, in §8.3, but there, although the remained relatively unanalysed, it did not have a quantifier-like status. Obviously it would be possible to amend that analysis, but then some other explanation for the grammaticality of (1) would have to be found. It is surely simpler to preserve the already suggested structure for postdeterminer quantifiers, together with the generalisations it encapsulated, than reanalyse the and then be

forced to find another, possibly ad hoc, explanation for the + quantifier sequences in which the was taken to be a quantifier itself. The explanation for their grammaticality would be quite different from that for, for example, all six, since the collocational range is quite different.

The second distinction between the and quantifiers which we shall discuss concerns the presence or absence of some trace of a higher existential. We have already seen that all quantifiers except those which we have called "universals", e.g., all, may function as the complements of an existential predicate when they are interpreted nongenerically; but this is never the case with the:

- (12.2) a There were many boys came to the
party
b *There were the boys came to the
party

This could, of course, imply that the is a universal quantifier, but it has none of the freedom of surface position which is such a dominant characteristic of the universals:

- (12.3) a Boys all like cheese
b *Boys the like cheese

And so it hardly seems possible that the is a universal quantifier. Since the behaviour of the in these circumstances is comparable neither with an existential nor

with a universal quantifier - the only two types we have so far been able to discover - it is all the more improbable that the is a quantifier. And the third point for discussion confirms this, for it is that the semantic status of the is quite different from that of any quantifier. All quantifiers in one way or another convey information about the number or quantity of objects referred to by the collocating noun. Thus the question:

(12.4) What number of boys came to the party?

however inelegantly it may be phrased, can be answered by a quantifier collocating with boys:¹

(12.5) a Many boys

b Six boys

c All the boys

In contrast, it would be utter nonsense to reply:

(12.6) The boys

This can only be because the semantic information requested in (4) is not provided in (6); and the reason for

¹ But that is not to say that every quantifier can be used to answer the question. Thus some, deriving as it does from A NUMBER, will be inappropriate, for it conveys no new information. And we can also observe, as in the case of (5c), that some quantifiers demand slight variation from the norm. Neither of these points, however, can be considered as significant.

this must be that the is quite different semantically from any quantifier. Since the is therefore seen to be both semantically and syntactically completely distinct from any quantifier, we must reject any analysis which might in any way imply the opposite.

As the cannot possibly be a quantifier, might it not be an 'article'? Now the problems raised by this question are of an order quite different from that above. In traditional English grammar, cf. §1.4, and indeed up to and including Chomsky (1957), if not beyond, the class of 'articles' has generally been understood to include two items only, namely the and a. But in Chap-11 we were able to demonstrate that a was a morphological item whose primary contrast was with the plural inflexion. Whatever the contrast between it and the, and it seems highly unlikely that there could be one, it must be only very subsidiary. That being the case, it can only be misleading to claim that there exists the primary relation implied by the labelling of these two items as 'articles'. From this it follows that if we remain true to the traditional classification then the must be the only member of the 'article' class. But this is only unenlightening, for we are in search of relations which exist between the and other words in the language, we are not attempting to establish the lack of such relations. Of course, there may be no important connections, but we should not give up the search

because of an unfortunate stipulation.

Yet it is still possible to maintain fruitfully that the is an 'article'. The most important representative of this approach today is Paul Postal, who writes (1966:179):

"... my basic claim... is that the so-called pronouns I, our, they, etc. are really articles, in fact types of definite article."

To a certain extent, and we have already pointed this out, cf. §1.2, this is reminiscent of the original Aristotelian approach, in which 'articles' and 'pronouns', in present-day terminology, were then classed together as arthra, i.e., articles. We shall not discuss the merits of Postal's specific analysis here, for it has been convincingly refuted by Delorme and Dougherty (1972) and Sommerstein (1972), although from quite different theoretical bases.² As far as we are concerned Sommerstein's arguments are the more interesting, because he suggests that the correct relation between the and pronouns is a mirror image of the one presented by Postal; in other words, it is not the case that

² See too Kjellmer (1971:44-45) for a defence of what Postal (1966:177) calls "our traditional lore about English grammar", e.g., the work of Jespersen. Even for Postal it seems a little brash to relegate Jespersen to a purveyor of old wives' tales.

pronouns are underlying 'articles', rather than the is an underlying pronoun. We shall examine Sommerstein's analysis more closely in §§12.2 - 12.3, but we may note one immediate advantage: whereas Postal, by retaining the concept of 'article', is still committed to a contrast between a and the, cf. Postal (1966:179), Sommerstein does not need to introduce such a contrast, and, indeed, never does so; from the evidence both of this section and Chapter 11 above, this would appear fortunate.

Whether or not the and the pronouns are to be assigned an identical underlying structure is, however, a rather technical point at present; what is more relevant is that they are certainly closely related. And there is already at hand a grammatical notion which will relate the two (sub-) categories, namely deixis. We may define deixis as the linguistic feature which serves to relate lexical items to the situation of utterance. Thus I signifies the speaker of the utterance, now signifies that an event described in the utterance is taking place at the time of uttering, and there signifies that some object or action mentioned in the utterance is at some place other than where the utterance is deemed to be being made, cf. Lyons (1968:275-76). Within this general class we can easily perceive a subclass of three items including the 'definite article', namely the, this and that. They are indisputably

deictics, not only by virtue of their semantics, but also because of their clear surface relation in many languages to the pronoun system, see Lyons (1968:279). And what we may call their internal semantics is no less certain: this normally implies proximity, that implies distance, and the is the unmarked member of the trio.³ In this respect we may note the possible collocations of the contrasting deictics here and there, especially in colloquial forms of English:

- (12.7) a this here book
 b *this there book
- (12.8) a *that here book
 b that there book
- (12.9) a *the here book
 b *the there book

We shall discuss some further examples of this below.

The classification of the as a deictic may be objected to on the grounds that it is no more than a terminological variation on the theme, already briefly mentioned, that 'articles' are really pronouns (or vice versa). Against that we may firstly note that since it is not certain that all deictics are pronouns, although all pronouns are deictics, we can claim to have asserted

³ We should observe that "proximity" and "distance" are not necessarily used spatially, for the relation may also be temporal.

a wider linguistic classification. And secondly, although it may be true that the two (sub-) classes of the and the pronouns are closely related, it does not necessarily follow that they ought to have virtually identical underlying structures. Clearly it would be desirable if their underlying structures were not totally dissimilar, but surely we ought not to go so far as to assume such identity, at least at present and in view of the number of distributional contrasts. For example, whereas the can freely collocate with a dependent restrictive relative clause, pronouns do so regularly only under a generic interpretation (this and that are rather more complex in their distribution):

- (12.10) a The man who was wearing a hat
yesterday is actually bald
- b *He who was wearing a hat yesterday
is actually bald

Secondly, and relatedly perhaps, in certain contexts the may appear with the first mention of a noun, whereas a pronoun cannot (at least, if it is a third person pronoun), compare:

- (12.11) a /The man came down the stairs; he
was wearing pyjamas
- b *He came down the stairs; the man
was wearing pyjamas

Thirdly, the must always collocate with some noun, this or that may do so, but he, she and it can never do so,

pace Postal (1966:191). Alleged counter-examples are either better analysed in some other way, as with we men, or they are not true examples of 'pronouns, e.g., she-wolf'. We ought neither to deny that the and the pronouns are related, nor to claim that they are identical. The classification of the as a deictic enables a middle position to be established, and the grouping of the with this and that, under a general heading of 'demonstratives', seems a more fruitful starting point. We must not take the unity of pronouns and demonstratives as an a priori.

12.2 'The' and relative clauses

Apart from the other demonstratives, there is another group of deictics with which the has an especially close relation, and that is the group of relative pronouns. In particular, historically the can be paradigmatically and (probably) analogically connected with the demonstrative that, which is also the historical source of the relative pronoun that, and we may also note that in, for example, German, there is a formal identity between the 'definite article' and the relative pronoun, cf. §1.2. It has further been observed by a number of scholars that this connection is reinforced by some distributional features of the, more specifically that in certain cases the may only occur with a noun which has a dependent restrictive relative clause or

phrase, and that in other cases what Robbins (1968:236) calls a Right Adjunct to a noun demands the presence of a collocating the. An example of the first type involves proper nouns: the cannot normally collocate with a proper noun, but this condition does not apply if there is a dependent restrictive. Compare the examples in (12) with those in (13):

- (12.12) a *The Edinburgh is fast falling
 into ruin
 b *The Edinburgh was an intellectual
 centre of the world
- (12.13) a The Edinburgh that I know is fast
 falling into ruin
 b The Edinburgh of David Hume was
 an intellectual centre of the
 world

An example of the second type is found in the occurrence of sentence complements, which can only cooccur with the + noun:

- (12.14) a *A fact that John likes cheese is
 irrelevant
 b /*That fact that John likes cheese
 is irrelevant
 c The fact that John likes cheese
 is irrelevant

A third factor which links the with relative clauses is that an NP with anaphoric reference (signalled by the,

cf. §12.3) cannot have a dependent restrictive relative. Thus in (15a) the referents of the two instances of flowers cannot be identical, although the second set of referents may be a subset of the first set. On the other hand, anaphoric reference is perfectly possible in (15b), where the relative clause is nonrestrictive:⁴

- (12.15) a There were many flowers in the
garden and I picked the flowers
which were pretty
- b There were many flowers in the
garden and I picked the flowers,
which were pretty

Given the obviously close connection between the and restrictive relatives, it is therefore not surprising that most recent discussion of the has attempted to justify a derivation for the which involves relativisation. There have been three principal attempts to do this: one by Vendler (1967) and Robbins (1968); another by Thorne (1972, 1974); and a third by Sommerstein (1972). Let us examine each of these in turn.

⁴ Neither sentence in (15) is particularly elegant, and this appears to be for two reasons. Firstly, a sequence of two lexically identical and (partially) referentially identical nouns is normally avoided by a deletion rule; secondly, second mention of a noun usually demands this, that or a pronoun, rather than the.

Since the proposals of Vendler and Robbins have already been discussed in §3.4, we need only be quite brief, but they are worth discussing once more, both because of the further evidence we have been able to obtain and also because their suggestions are very similar to the others which we shall discuss and to that of Perlmutter (1970), whose approach, indeed, is perhaps insufficiently distinguishable to merit separate close analysis. We may remind ourselves of the position adopted by Vendler and Robbins by re quoting their own remarks. Vendler (1967:46) says:

"The definite article in front of a noun is always and infallibly the sign of a restrictive adjunct, present or recoverable, attached to the noun."

Robbins' hypothesis is in principle the same, but more detailed in its formulation (1968:54):

"Determinative the is always indicative of sentence combination: either a noun-sharing combination of one sentence with a transformed other sentence, or the inclusion in a Pred of a sentence nominalized into a definite noun-phrase ... In this essay anaphoric the is treated as a special kind of occurrence of determinative the."

If we convert these remarks into formalised generative grammar, it would appear that the is to be introduced by

the relativisation rule which raises the relative clause into the matrix sentence; this is also the suggestion of Perlmutter (1970:241-43). There are two problems with such a hypothesis, one concerning anaphoric the, the other concerning what we shall call, following Smith (1963:15), cataphoric the, that is, occurrences of the which appear to be induced by a restrictive clause or adjunct. Let us consider the cataphoric instances firstly.

The problem here, already noted in §3.4, is that the is not obligatory when there is a restrictive clause. Therefore both (16a) and (16b) are grammatical:

- (12.16) a I know the girl who is wearing a
red hat
b I know a girl who is wearing a
red hat

If the is introduced by a relativisation rule it will therefore, apparently, have to be optional. But the two sentences in (16) are different in meaning. Thus it would seem to be the case that the-formation is an optional meaning-changing rule. Vendler (1967) notes both this problem and the problem that in certain cases the-formation is obligatory: witness the unacceptability of (17b):

- (12.17) a I know the man who killed Kennedy
b *I know a man who killed Kennedy

Vendler's explanation is as follows (1967:50-51):

"Since the verb kill suggests a unique agent, the definite article replaces the indefinite one, and we get (16) [= (17a):RMH]. If the relevant verb has no connotation of uniqueness, no such replacement need take place; for instance,

I know a man who fought in Korea.

Of course we can say, in the plural,

(17) I know the men who fought in Korea.

In this case I imply that, in some sense or other, I know all those men. If I just say

I know men who fought in Korea

no completeness is implied; it is enough if

I know some such men."

What Vendler would appear to be suggesting is that we attach a feature [\pm unique] to the relevant NP; if this feature had the value [+unique], then the-formation would follow. Assignment of plus values for this feature would be due either to selectional restriction rules or to some arbitrary situational relation. The unsatisfactoriness of this as a solution is that [+unique] is no more than an ad hoc intermediary in the process of deriving the. Although the-formation itself will no longer be meaning-changing, there will still be a meaning-changing device present, namely that which would assign [+unique] in, say, (16a) as opposed to [-unique] in (16b).

Nevertheless, Vendler's observation that the indicates a unique referent for the collocating NP and that such uniqueness may be determined by the internal semantics of the relevant structure is sufficiently acute to demand that it not be ignored. What we ought therefore to attempt to do is to find an underlying representation which explicitly demonstrates that in a sentence such as (16a) the referent of girl is uniquely defined, whereas that is not the case with the referent of girl in (16b). If we are able to do this we shall not need any meaning-changing transformation, although it still remains an open question whether or not it will be possible to formulate a plausible derivation to the given surface structure. Now at first sight a reasonable paraphrase of (16a) appears to be:

(12.18) I know a girl who is wearing a red
hat and only one girl is wearing a
red hat

But (18) can hardly be regarded as a putative source for the underlying structure of (16a). The principal reason for this is that the second conjoined sentence is most probably a nonrestrictive clause and that therefore (18) is a better paraphrase of:

(12.19) I know the only girl who is wearing
a red hat

If we attempt to remove the nonrestrictive element in (18) we are no further forward, for in:

(12.20) I know a girl; only one girl is
wearing a red hat

the identity conditions which hold for relativisation, namely lexical and referential identity between the antecedent and dependent NP's, do not both hold, for at least lexical identity fails. From the failure of these attempted paraphrases which explicitly state, by only, the element of uniqueness discussed by Vendler to provide any possible underlying structure for cataphoric the, we may reasonably conclude that a plausible solution to the problem will have to come from some quite different source. Let us therefore turn our attention away from cataphoric the for the present, in order to consider briefly Vendler's and Robbins' approach to anaphoric the.

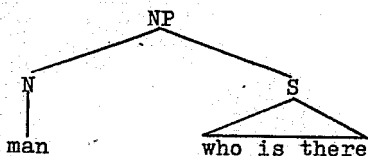
Our earlier discussion of those works and the quotations from them given above make it clear that both Vendler and Robbins assign to anaphoric the a status identical to cataphoric the. Since the latter is derived by relativisation so too must the former. But no anaphoric the cooccurs with a restrictive relative, cf. examples (15). Their solution is to assume a deleted restrictive relative, identical to the sentence in which the collocating NP is first mentioned ('indefinitely'). Apart from several other disadvantages which we noted in §3.4, this suggestion has the fatal flaw that it cannot account for anaphoric the in a sentence such as:

(12.21) When John arrived at the hall, the
lecturer had already been speaking
for 15 minutes

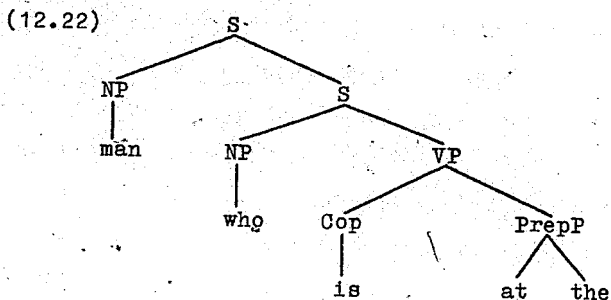
where the anaphora is due to hyponymy rather than repetition. And so, even if we do eventually find some solution to cataphoric the involving restrictive relatives, it will not be possible to extend it to anaphoric the. Since it appears that the work of Vendler and Robbins, despite their interesting observations, does not offer any immediate hope of resolving the issues at hand, we shall now move on to examine the proposals in Thorne (1972, 1974).

In one aspect Thorne's analysis of the is very like that of Vendler and Robbins, in another it contrasts. The similarity is that Thorne also associates occurrences of the with restrictive relatives; the contrast is that he apparently deals only with anaphoric the. Thorne's position is as follows (1972:563):

"Essentially my proposal concerning noun phrases like the man is that they should be derived from underlying structures containing a deictic sentence as a relative clause attached to the noun, deictic sentences being sentences like There is a Lotus Elan, which I assume has an underlying form equivalent to A Lotus Elan is there. Thus the underlying structure of the man would be



Thorne (1974:111, fn. 1) suggests that some modification of this will be necessary, but that is basically unimportant for our purposes. The transformations which produce the surface string the man are, Thorne claims, similar to those required elsewhere in the grammar. The major problems of Thorne's analysis appear to centre upon there. Firstly, no distinction is made between existential and locative there, yet this decision seems vital, cf. §7.4 and Allan (1971, 1972). The main result here is confusion, but it appears most profitable (if not necessary) to accept that there is a distinction to be made and thence assume that Thorne is employing locative there only. This agrees with his remark (1972: 563) that there is the locative form of the item which has the nominative form of the. But this leads to the second problem: if there is none other than the locative of the, then Thorne's underlying representation is alternatively expressed prepositionally, i.e., as:



This reveals the basic circularity of Thorne's proposal: surface the is derived from an underlying the in the locative case.

However Thorne has an interesting argument in favour of his position; it is that by establishing a relationship between the and there we can account for the semantics of the in as far as it relates to the presence (physical or mental) of the referent of the collocating NP. Indeed the position is more interesting than that, for that can be related to there, this to here, and the to either there or here, cf. examples (7) - (9) and the discussion in §12.1; also note that there are further possible collocations:

- (12.23) a This table here
 b That table there
 c The table here
 d The table there

The advantage here is that we are able to relate the directly to this and that, and all three to other members of the deictic system. Yet this is scarcely an

adequate justification. If this is derived from here, i.e., at this, and that derived from there, i.e., at that, to claim that the is derived from there is to claim that it is some variant of that. This Thorne does (1972:565). But the evidence of (23c) suggests that it is an unmarked variant of either this or that, and despite his footnote referred to above, Thorne does not suggest an analysis which will explain this. Furthermore, consider the difference between an analysis which generates this, that and the directly and Thorne's analysis. In the former case there will have to be some ad hoc feature assignment which will state the semantic differences between the three; in the latter the same procedure will have to apply to this and that. The only advantage this latter has is that there will not have to be a repetition of the assignment to the adverbials. But that would not be the case if here and there were derived from this and that, and the problems caused by the grammaticality of both (23c) and (23d), rather than just one of them, would be solved by considering the as the unmarked member of the triple. We thus retain the generalisations noted by Thorne, without at the same time having the problems which his analysis has.

We would appear, however, to lose one important advantage, namely that Thorne's analysis preserves the connection with restrictive relative clauses. Yet this may not be the disadvantage it appears to be. As we have

already stated, Thorne does not discuss the status of cataphoric the. But it seems probable that he would derive all instances of the in the manner described above. Now it looks as if this would help to solve a difficulty we have discussed already, namely how to distinguish between sentences of the types (24a) and (24b):

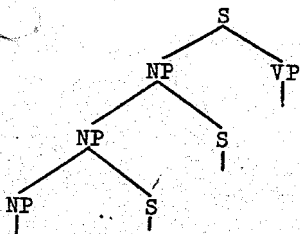
- (12.24) a A girl I know kissed Bill
b The girl I know kissed Bill

(24b) would have an additional restrictive relative in underlying structure, along the lines of:

- (12.25) Girl who is there who I know kissed
Bill

But (25) raises an important problem, which is whether the relative clauses are 'stacked' or conjoined.⁵ If the relatives are stacked, then the underlying structure of (25) is schematically:

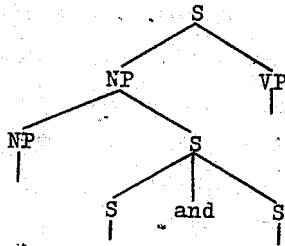
- (12.26)



If the relatives are conjoined, then the structure is as below:

⁵ For a good discussion of stacking see Stockwell et al (1972:442-47).

(12.27)



Now if (25) is analysed as an example of stacking there are no major problems with Thorne's probable analysis as such. However there are some strong arguments against the stacking proposal. Thus we have observed that NP's with anaphoric reference cannot have a dependent restrictive relative, cf. the examples in (15). Let us now assume that cataphoric the is introduced by a so far unformalised rule of relativisation of relative clauses. If we can accept the conjunction analysis then we can generalise our constraint on the collocation of the with such relatives as follows:

(12.28) No string of the structure:

$$[_{NP}[_{NP} \text{the} + W + N_1 + X][_{S_1}[_{S_2} \text{wh-N}_2 + Y]Z]]$$

is grammatical/unless relativisation and the-formation has applied, where N_1 and N_2 are identical. If Z is zero then S_1 is also zero.

(28) states nothing other than that the + N sequences may not have a dependent restrictive relative clause unless that the has been introduced by the relativisation

transformation on that restrictive clause. Under the conjunction analysis if there is more than one restrictive clause they will be conjoined and only one relativisation will take place, thus meeting the constraint. However under the stacking analysis more than one relativisation transformation takes place, since it is cyclical, and (28) cannot reasonably apply, without being stated in an over-complex manner.

Let us accept, therefore, if only for the sake of argument, that the conjunction analysis is to be preferred. Thus the two relative clauses in (25) are conjoined. Now Ross (1967:§4.84) has postulated a Coordinate Structure Constraint which is as follows:

"In a coordinate structure, no conjunct may be moved, nor may any element contained in a conjunct be moved out of that conjunct."

In other words, if two sentences are conjoined in underlying structure, it is not permissible to cause one of these sentences to be moved out of that coordinate structure. But if (24b) is to be derived from (25) when that sentence has the initial structure to which (27) approximates, then Ross' constraint is violated. Therefore Thorne's proposal demands that the relative clauses be stacked. And then, of course, constraint (28) cannot be retained.

But in support of the stacking hypothesis it is possible to quote:

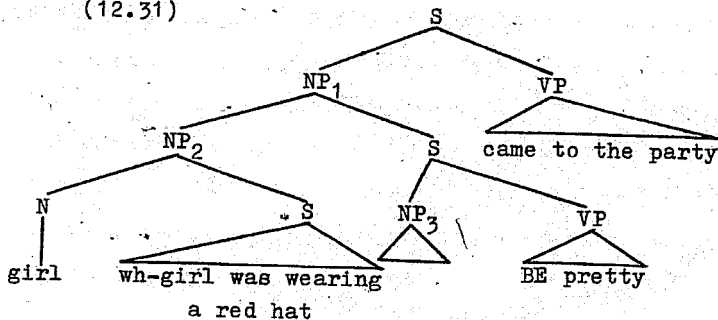
(12.29) The pretty girl who kissed Bill
came to the party

where both pretty and the full relative clause are apparently restrictive. If they are conjoined in underlying structure, then the Coordinate Structure Constraint is again violated. But intuitively it seems as if in (29) the phrase girl who kissed Bill is an anaphoric subset of the set previously referred to by:

(12.30) girls who kissed Bill

Such an intuition cannot be captured either by the conjunction or by the stacking proposal, both of which treat the restrictive adjective and the restrictive clause on the same level, i.e., as 'new' information. Indeed there does not seem to be any current proposal which adequately differentiates the two. Perhaps one way to do this would be to consider the relative clause in (29) as immediately dominated by the same NP node as dominates the N dominating girl. Thus (29) would have the structure of (31), in which for ease of presentation NP_3 has been omitted; it is identical to NP_2 :

(12.31)

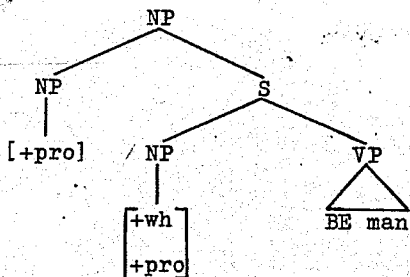


If something rather like (31) can be accepted as the underlying structure of (29), then that sentence will not contain a violation of the Coordinate Structure Constraint. But that point, important and uncertain as it is, will not be further discussed here, since it is of more relevance to note that (24b) - derived from (25) - is quite different from (29) in its implications. (29) implies that more than one girl kissed Bill (although only one of them was pretty); in (24b) there is no implication, indeed there is a denial, that I know more than one girl who kissed Bill. The semantic difference ought, we may suggest, to be expressed by a difference in underlying structure. Hence non-violation of Ross' constraint by (29) has no implications for Thorne's analysis of (24b). In its turn, therefore, (25) can only be assigned an underlying structure of stacking, i.e., (26), if it is not to violate the constraint in question. But this renders the constraint (28) unstatable. Therefore the Coordinate Structure

Constraint carries insufficient weight to support stacking. And in addition to the evidence already discussed which supports conjunction, there is the further point that apparently many speakers reject all cases of stacked relative clauses, see Stockwell et al (1972:443). For such speakers, Thorne's analysis seems to predict that (24b) will be ungrammatical, which is indisputably incorrect.

Since the problems surrounding Thorne's proposal are so great that it is improbable that it can be acceptably modified, let us now turn to a consideration of the third attempt to link the to relative clauses, that of Sommerstein (1972). Basically his proposal is that an NP such as the man has the underlying structure of a complex NP whose head is a pronoun with a dependent restrictive relative which is a predicate nominal (1972: 198). In other words, it is of the form:

(12.32)



Sommerstein's proposal has two clear advantages over those which we have discussed above. The first of these is that by introducing an underlying pronoun as part of

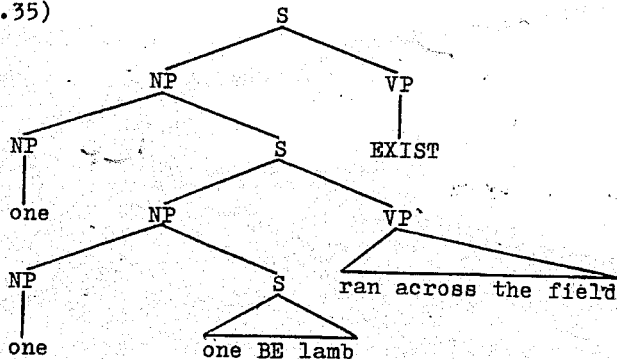
the source for the, the relation between the and the other deictics is more explicitly stated than in the analyses of Vendler and Robbins and yet it avoids the circularity found in Thorne's work. Secondly it permits us to generalise the claim of Bach (1968) that all nouns are derived from predicate nominals, which so far we have been forced to restrict to 'indefinite' NP's, to NP's containing the. Apart from its other advantages, this enables us to contrast the and a in an interesting way. The underlying structures of the two sentences:

(12.33) A lamb ran across the field

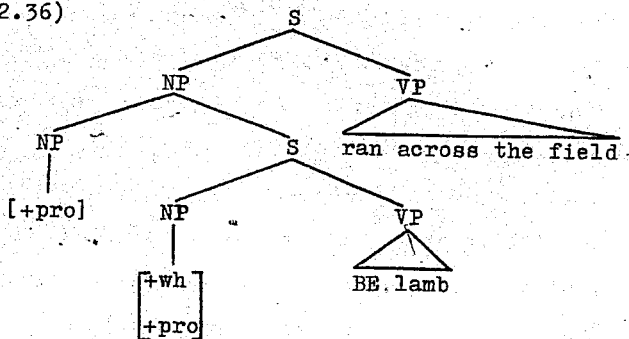
(12.34) The lamb ran across the field

will be (35) and (36) respectively, ignoring the structure of the VP, cf. §11.4:

(12.35)



(12.36)



There are only two differences between (35) and (36), and these are not only related to one another, but they can be shown to reflect the semantic and syntactic differences between (33) and (34). Firstly, there is a higher existential sentence in (35) which is absent in (36). But this explains the difference in grammaticality between (37a) and (37b):

(12.37) a . There was a lamb ran across the
field

b *There was the lamb ran across the
field

It also clarifies a semantic distinction, namely that the referent of a lamb is 'new', whereas the referent of the lamb is 'given', that is to say, in the former case the speaker assumes no knowledge on the part of the hearer regarding the referent of lamb, whereas in the latter case he assumes that the hearer has (been given) sufficient information already to pick out the exact referent. In the situation where the referent is 'new',

and the hearer is assumed to be ignorant of it, one of the purposes of the higher existential is to assert that there actually is a referent. Clearly this is redundant in the case of a 'given' referent, and therefore in such a case no existential is present. Therefore this first contrast between (35) and (36) is syntactically and semantically justified. The second difference is that where (35) has the dummy place-holder one, (36) has a pronoun. Now one, being a dummy, is devoid of any semantic meaning, which relates closely to our theory that a is a purely morphological, and hence semantically empty, creation. On the other hand, the underlying pronoun in (36) has the semantic implication of previous reference, which of course is inherent in the description of the occurrence of this the as anaphoric, and the description of the referent of the lamb as 'given'. Syntactically, as Sommerstein (1972) shows, the similar behaviour of the + N phrases and pronouns is thereby explained, and the predicate nominal structure of the noun is accounted for. Further, Sommerstein (1972:205) suggests that this and that may be derived from a further clause containing here and there respectively, in a very similar way to Thorne (1972, 1974). But the rather different underlying structures and transformations employed mean that this does not appear to violate the Coordinate Structure Constraint, and since the is not derived from there it avoids some of the other problems in Thorne's proposal.

In the following section we shall see that there are a number of other advantages held by Sommerstein's hypothesis, but we have already been able to give sufficient evidence not only to demonstrate that his is the most successful of the three attempts to provide an underlying source for the which we have examined here, but also to suggest that it may indeed be the nearest approximation to a solution that we can find. However, this is not to deny that it has its own problems, and we shall look at some of these below, when we examine how satisfactory it may be as an explanation of occurrences of the other than the purely anaphoric.

12.3 Anaphoric, ecphoric, cataphoric and generic

The various discussions of the which we have considered, and perhaps even our own discussion, tend to suggest that there are four different linguistic sources for the: these are anaphora, ecphora, cataphora and genericness. The first three have in common the feature that they are processes by which an NP is 'defined', i.e., that enough information is believed by the speaker to have been given to the hearer to permit the speaker to assume that the hearer can determine uniquely the referent(s) of the NP which the speaker has in mind. The normal, but not exclusive, linguistic sign of this process is the. Anaphoric reference is reference to some already mentioned object; the previous reference

may be either in the same sentence or earlier in the discourse, and it is interesting to note that if the previous reference is in the same sentence or one sentence immediately before there is normally some pronominalisation process - which may account for the unusual restriction by Dougherty (1969:488), cf. §4.4, of anaphora to within one sentence. The use of the with the NP in question is typically restricted to previous reference over one or (preferably) more sentence boundaries. Thus compare the sentences below:⁶

- (12.38) a A lamb decided it would run away
b *A lamb decided the lamb would run away

- (12.39) a A man shot himself
b *A man shot the man

Since the (b) sentences are grammatical, although very strange in interpretations where there is no referential identity between the noun phrases, this discrimination is a useful disambiguation of two anaphoric processes. Ephemora, cf. §3.2 and Smith (1963:17), is previous reference due to the context of situation. This is best seen by example; in (40):

- (12.40) The sun was shining yesterday
sun can only be regarded as being 'defined' by the

⁶ The referents of a lamb and the lamb in (38b) are taken as identical here; similarly for a man and the man in (39b).

situational fact that for most purposes it is assumed that there is only one sun (the physical existence of other suns being irrelevant). It is worthy of note that ecphora does not cause pronominalisation. As a result, (40) is not equivalent to:

(12.41). It was shining yesterday
although this does not exclude:

(12.42) The sun has not been seen today,
but it was shining yesterday

Cataphora is 'defining' reference due to some immediately following element or elements, which, as we have already observed, is very often a restrictive relative clause. Included here is a restrictive adjective, even though in surface structure it normally precedes.

If these three sources reflect different processes by which an NP is 'defined', it must surely follow that Sommerstein's analysis can only apply to one of them, and that another two underlying sources for the will have to be found, even if we exclude, as we are at present doing, generic the. Since the examples given by Sommerstein (1972) are clearly anaphoric, we can assume that that analysis will be preferable for anaphoric the. Now we have observed above that anaphora induces either the or a pronoun. Therefore by postulating an underlying pronominal source for anaphoric the, not only has Sommerstein captured the relationship between the and the other deictics in general, he has also made explicit

the most obvious characteristic of anaphora. There is, however, a possible objection to Sommerstein's thesis, on the following grounds. If we look closely at (37) it can be seen that it comes very near to generating the directly in the base, and it certainly directly generates an underlying pronoun. But in most transformational work on pronouns it has been claimed that pronouns are regularly introduced transformationally. The relevant transformations are always based upon repeated NP's within a single sentence. Now we have noted above that this is the characteristic, although it is not absolute, of pronoun anaphora, which distinguishes it from anaphoric the. Therefore the only method for introducing anaphoric the transformationally would have to be a transformation whose structural description contained two disjoint sentences. But this is impossible since the theory has only one initial symbol, namely S, and that precludes transformations over more than one disjoint sentence.

The inadequacy, therefore, lies in the theory rather than in Sommerstein's analysis. For that analysis captures the essential generalisation that pronouns and anaphoric the are both derived from previous reference. That it does so at the cost of generating pronouns non-transformationally is an unfortunate necessity. Note also that there are other cases where pronouns have to be generated in this way, as in:

(12.43) The sun is a bright yellow object.

I saw it yesterday

It is due to the previous reference to the sun, but that reference is in a different sentence. Thus there is no identical NP in the sentence which contains it which might cause the pronominalisation transformation to operate. Therefore it must be generated in the base. This suggests that the inadequacy may simply be in the theory of pronominalisation, for to incorporate (43) into the most generally accepted theories of pronominalisation would be to claim that surface pronouns may be either generated in the base or derived transformationally. Such duality is grossly inefficient. Sommerstein (1972:206) avoids this by apparently generating all pronouns in the base, but whether or not this is the correct solution cannot be argued out here.

Another reason for agreeing that it would be incorrect to allow transformations to operate over more than one disjoint sentence is to be derived from ecphoric reference. Now Christophersen (1939:72) is surely correct in defining the principal function of the as follows, cf. too Jespersen (1949:479):

"The article the brings it about that to the potential meaning (the idea) of the word [i.e., the collocating noun:RMH] is attached a certain association with previously acquired knowledge, by which it can be

inferred that only one definite individual is meant. This is what is understood by familiarity."

This suggests that ecphoric reference should be connected as closely as possible with anaphoric reference, for we only say the sun by virtue of what Christophersen calls its "familiarity". The difference between anaphora and ecphora is that the former is linguistically explicit, the latter is not. Therefore in the latter case no possible transformational operation is available, and the advantage of Sommerstein's proposal is that this is no defect. We can generate anaphoric and ecphoric the in precisely the same way. This is also useful in connection with another problem which we have touched upon previously, namely hyponymic anaphora, as in (44), see too §12.2:

(12.44) That book is most interesting, for the author displays a wide knowledge of his subject

Such reference occupies a midway position between anaphora and ecphora, and although it is not fully explicable (possibly because the explanation cannot be wholly linguistic) this is not too problematic in a theory, such as Sommerstein's, in which anaphora and ecphora are not systematically distinguished, for the very good reason that within one sentence they are not distinguishable. Furthermore, some very interesting remarks

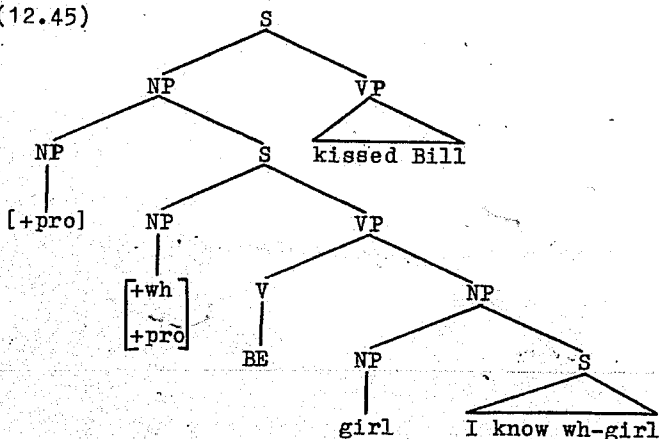
by Leech (1974:167-68) support this theory that hyponymy is inextricably connected with anaphoric reference, although there may still be many problems to solve.

We are now left with the problem of cataphora. This too is soluble in terms of Sommerstein's theory, in contrast to, for example, that of Thorne, for there are no unusual problems of stacking or conjunction. If we take example (24b), repeated below:

(12.24) b The girl I know kissed Bill

its underlying structure will be of the form:

(12.45)



The standard processes of relativisation will produce the surface structure of (24b). However although there are no formal problems, this solution is not appealing. Firstly, it contradicts the notion that cataphoric reference is due to some following restrictive clause not present with anaphoric reference, since the in (24b)

will be derived from the same NP as in anaphoric cases, and is not due to the relativisation of the lowest S. Secondly, the constraint outlined in (28) is violated, since its purpose is expressly to exclude structures such as (45). As it appears to be a useful constraint which accords with our intuitions, it would be most unfortunate if it had to be rejected.

But the only alternative appears to be to introduce the by means of a relativisation transformation, and as we were able to ascertain in §12.2, this meets with many problems. Yet there may be a solution in such terms. Thompson (1971) suggests that all relative clauses, restrictive and nonrestrictive, ought to be derived from an underlying conjunction. This, of course, is already the accepted source for nonrestrictive clauses. At first sight this hardly eases our difficulties, but there might be a solution if it could be shown that all relative clauses except restrictive ones dependent upon a 'definite' NP ought to be derived from a conjunction source. This would make the embedded source for restrictive relatives unique to 'definite' NP's, and thus the rule combining relativisation and the-formation would be obligatory and meaning-preserving. Now Fairclough (1973) has given some useful evidence that this may indeed be the case, for he shows that the so-called 'style disjuncts' occur more happily in restrictive clauses with an 'indefinite' antecedent than in those

with a 'definite' antecedent; thus compare (46a) and (46b) from Fairclough (1973:528):

- (12.46) a That waiter served me a steak
 that honestly made me sick
 b That waiter served me the steak
 that honestly made me sick

Clearly (46a) is preferable to (46b). As Fairclough (1973:529) points out, there is a feeling that in (46a) two separate statements are being made, whereas only one statement is made in (46b). The connection of style disjuncts with performatives supports this intuition. And the most appropriate structural method for showing the distinction would be to assume underlying conjunction in (46a) but not in (46b).

Therefore there is some support for claiming that relativisation of an embedded restrictive clause obligatorily and unambiguously introduces a cataphoric the collocating with the antecedent NP. This enables (28) to be retained as a deep constraint, and anaphoric the and cataphoric the to be clearly distinguished, the first (virtually) being generated in the base, the second being transformationally derived. An interesting fact in favour of this distinction cropped up in our discussion of both, cf. §10.4 and, especially, Chapter 4. We proposed that there was a Dual Copy rule, part of whose function was to delete the in order to generate both children. But we were able to observe that only

anaphorically-derived the could be deleted by the transformation in question. At the time the only solution was to mark non-anaphoric the so that it was not deleted. But now an alternative solution presents itself, namely that if the Dual Copy rule is ordered before the transformation introducing cataphoric the, then no special marking of cataphoric the will be necessary. It is not certain that such an ordering will be possible, but it can scarcely be denied that postulation of different sources for anaphoric and cataphoric the provides a useful starting point.

It must be surprising that so far no attempt has been made to give the transformation which introduces cataphoric the. The reason for this is not that it is difficult to state: it is simply necessary to extend the rule creating the relative pronoun (e.g., who) so that at the same time it introduces the. Rather, the reason is that, whilst this may provide an excellent solution to the problem of cataphoric the, it does so only at the cost of creating a new problem elsewhere, for we now have no way of distinguishing between (47a) and (47b) in underlying structure:

- (12.47) a The boy kicked a girl who was
smaller than he was
b The boy kicked a girl, who was
smaller than he was

Whilst this may be tolerable if Huddleston (1971:212-15)

is correct in claiming that the difference between the two sentences is minimal, cf. §3.3, there is the further difficulty that in (48) only the restrictive interpretation is grammatical (as above, this is marked by the absence of commas):

- (12.48) a The boy kicked a horse that was
standing in the field
b *The boy kicked a horse, that was
standing in the field

We have now no way to account for this contrast, and until we do we can hardly claim to have provided a satisfactory answer to the problem of cataphoric the. Perhaps the only answer will be one which allows a third source for restrictive clauses, in which case the underlying structures of sentences such as (24b), (47a) and (47b) will each be unique. But until that third source, or some equally appealing solution, is found, we can only regard our hypothesis regarding cataphoric the as the most temporary and tentative of solutions.

So far our discussion of the has been largely independent of quantifiers, but a glance back, especially to §3.3, would suggest that if we wish for conformity anaphoric the would simply be derived from THE NUMBER, and the structure suggested there for postdeterminer quantifiers differs from any considered here in the important fact that whereas for:

- (12.49) The boys ran away

the predicate nominal would be lower than the matrix sentence, the reverse would be true of:

(12.50) The many boys ran away

But to leave it at that would be to miss the point, which is that in (50) there are two pieces of information:

i) the number of boys was large; ii) those boys ran away. And in Part III the suggested underlying structure for sentences such as (50) sandwiched, as it were, the predicate nominal from which boys is to be derived between these two pieces of information. The immediate contradiction which seems apparent is therefore not present. It may well be the case, indeed it must necessarily be so, that a more elegant analysis of postdeterminers can be found, but that which we proposed is in large measure satisfactory. Clearly the "THE" which was left unanalysed previously can be now given a deeper structure, but this presents no difficulty. A much more radical difficulty is created by the fact that we have continually used restrictive relative clauses in our proposals for deriving quantifiers, but those are restrictive clauses which do not introduce cataphoric the. This implies that new structures must be found, but, as was stated in the previous paragraph, these are not to hand. Therefore this reanalysis must wait until they are, and we can only console ourselves with the fact that the reanalysis will in all probability be trivial.

However it is a salutary reminder of the final inadequacy

of our hypothesis, to which we can only plead in mitigation that the other theories which we have examined have even greater deficiencies.

We have left the case of generic the to the end, for it appears to present special difficulties of its own. In fact I hope to show below that generic the is only a special instance of ecphoric the, and hence of anaphoric the. But this does not contradict our earlier statement, in §9.3, to the effect that generic the does not indicate reference to some object known to the speaker and presumed by him to be known to the hearer. It may well appear to do so, but only, I would maintain, because the earlier statement is correct in a rather misleading fashion. It is true that in:

(12.51) The lion is a dangerous animal
a referent of lion is not 'given', and that was the intended meaning of the earlier statement. But it will be remembered that it was also claimed that the lion in (51) was to be derived from a structure which included the notion SET and that it was so stated as to be uniquely determinable, cf. (9.55)/in §9.3. This process is surely a linguistic equivalent to extralinguistic ecphoric mention of the sun. The difference between generic the and anaphoric the (in its widest sense) is that the former collocates with SET, whereas the latter collocates with the attendant noun. It is the presence of SET which determines a generic interpretation.

It therefore seems reasonable to suppose that generic the is derived from a pronominal source which has a dependent restrictive relative which contains a predicate nominal referring to SET; this parallels the analysis of anaphoric and ecphoric the. But there appears to be some difficulty with regard to:

- (12.52) The elephant which lives in Africa
has long ears

Should not this be treated as an instance of cataphoric the which is for some reason generic? One argument against this is the existence of generic sentences such as:

(12.53) He who pays the piper calls the tune
In nongeneric sentences, as we have seen in §12.1, it is at best extremely dubious to have a restrictive relative with an antecedent pronoun:

- (12.54) a *He who came to see me yesterday
has won a prize for pedantry
b *A prize for pedantry was given to
him who came to see me yesterday

To analyse the in (52) as cataphoric will be to fail to explain the difference between (53) and (54), a difference which is fairly simple to explain. All occurrences of pronouns are anaphoric, whether within one sentence or over several sentences. No NP which refers anaphorically can have a dependent restrictive clause. Therefore a satisfactory analysis of (52) or (53) will have

to deny the possibility of cataphoric status to the head NP in question. One possibility is to provide a structure such as in (31), which will assign to the elephant which lives in Africa the status of an NP containing no other NP (other than that directly dominating Africa). But whatever drawbacks this proposal might have, one stands out as compelling: it is quite unable to explain the difference between (53) and (54) except in an ad hoc fashion.

Now if we look again at our analysis of generic the in S9.3, and add to it our proposals concerning sentences such as (51) above, we can suggest that the underlying structure of (52) must be something like:

the non-cataphoric status of the in (52) more explicitly. Thus at worst (55) appears to be a step in the correct direction if we wish to reach a unitary analysis of generic the. Of course we have not yet explained the grammaticality of (53), but this has the appearance of idiosyncrasy, and in this connection it is useful to remember that the derivation of the lion in a generic interpretation, as proposed in §9.3, is rather unusual. The fact that the lion has a unique derivation makes it easier to handle the use of he in (53). However it will always require the use of an exception rule, or some similar device, for notice that (56) is quite unacceptable:

(12.56) *It that lives in Africa is a dangerous
 animal

12.4 Conclusion

The premise underlying Chapter 11 in which we considered the grammar of a, the so-called 'indefinite article', was that there was no justification for hypothesising an 'article' node in deep structure which would dominate only either the or a. It was assumed that the arguments in Part I, where the status of 'article' as a part of speech had been discussed, were sufficient to discredit such an approach. Therefore in §11.2 we examined the proposals of Perlmutter (1970) which, if correct, would imply that a was simply a

reduced variant of the numeral one, and hence a member of the compound existential system of quantifiers, most closely analysed in Chapter 8. It was concluded, however, that although Perlmutter's hypothesis was initially appealing it was both internally contradictory and insufficiently adequate as a description of the varied occurrences of a. A more adequate proposal, it was suggested in §11.3, was to consider a simply as the surface realisation of the morphological feature [+singular], without any semantic significance of its own. Finally, in §11.4 we discussed two serious difficulties facing any analysis of a, and concluded that the morphological analysis was more satisfactory than most. Thus we were able to confirm not only that it would be incorrect to consider a as an 'article', but also that it would be equally unfortunate to treat a as a quantifier. It is neither to be contrasted with the nor compared with one; rather, its closest connections are with the plural suffix -s.

Such a claim, of course, contains the implicit assertion that the proper analysis of the must be quite different, but we were able to observe in §12.1 that a and the had at least two factors in common, for it makes little sense to consider the as an 'article' and it cannot possibly be analysed as a quantifier, given the evidence presented there. Rather, it was suggested, the ought to be analysed as a deictic, closely related to

the demonstratives this and that and at least similar in its grammar to the pronoun system. Since most recent grammarians have been concerned with the relation between the and restrictive relative clauses we then turned our attention to this point, and the theories based upon it, in §12.2. We were able to conclude that the most appealing theory was that of Sommerstein (1972), although the type of analysis first proposed by Vendler (1967), in which the is always derived from a restrictive relative, was also interesting, if problematic to handle. In §12.3 it was claimed that there were two principal types of the: anaphoric and cataphoric. In the case of the former we were able to accept, and indeed give further support to, Sommerstein's hypothesis, which had the merit of making explicit the relation between the and the English pronouns. However the hypothesis was not easily extended to cataphoric the, where it was felt that a variant of Vendler's suggestion was most plausible. This was despite the fact that it was not fully formalisable and also that it created problems elsewhere in the grammar. We could not, therefore, pretend to a lasting solution for cataphoric the. Then we turned our attention to generic the, where we attempted to show that it was best analysed as a variant of anaphoric the, signalled by differences in underlying structure first proposed in §9.3.

Most of the important points have been mentioned in the preceding two paragraphs, but it is worth emphasising one or two of them before we stop. Firstly, it has to be recognised that the grammatical tradition which has led to the establishment of an 'article' category in which a and the, and only a and the, contrast is fundamentally mistaken. Virtually the only factor in common between the two is that they are not 'articles', and it disguises the fact that the is more like a pronoun than anything else, whereas a is a semantically empty morphological creation. It may yet be possible that for teaching purposes the two ought to be brought together, but even here the centuries of tradition may have led us away from a more useful approach which might contain some of the points raised above. Secondly, and in contrast to the above, it will be recalled that the original 'parts of speech' theory, as proposed by Aristotle and, after him, the Stoics, did recognise that the was a type of pronoun, just as we have claimed it to be. It may well be a matter for regret that Ancient Greek had no equivalent of a, for if there had been one, a satisfactory analysis of a could possibly have been obtained long before now. But that is not to claim that one has now been obtained.

Bibliography

All abbreviations of periodical titles are those listed in:

Linguistic Bibliography for the Year 1971, Utrecht, 1974: Spectrum, for the Permanent International Committee of Linguists

with the exception of the following:

EWPL : Edinburgh Working Papers in Linguistics, University of Edinburgh, Departments of English Language and Linguistics.

YPL : York Papers in Linguistics, University of York, Department of Language.

Where a reprinted version is indicated, all references in the body of the work are to the text of the reprinted version; however, in such references the date of the original version is retained, in order to clarify chronological relations.

- Abercrombie, D. 1967. Elements of General Phonetics.
Edinburgh U.P.
- Aitken, A.J., A. McIntosh and H. Palsson (eds.), 1971.
Edinburgh Studies in English and Scots. London:
Longmans.
- Akmajian, A. 1970. "On deriving cleft sentences from
pseudo-cleft sentences." LIn 1, 149-168.
- Allan, K. 1971. "A note on the source of there in
existential sentences." FL 7, 1-18.
- Allan, K. 1972. "In reply to 'There₁, there₂'." JL 8,
119-124.
- Anderson, J.M. 1968. "On the status of 'lexical forma-
tives'." FL 4, 308-318.
- Anderson, J.M. 1971a. "Dependency and grammatical
functions." FL 7, 30-37.
- Anderson, J.M. 1971b. The Grammar of Case. Cambridge:
Cambridge U.P.
- Anderson, J.M. 1971c. "Outline of a proposal for the
lexicalisation of complex structures." SL 25, 1-8.
- Anderson, J.M. 1971d. "Some proposals concerning the
modal verb in English." In Aitken et al (1971:69-
120).
- Anderson, J.M. 1973a. "The ghost of times past." FL 9,
481-491.
- Anderson, J.M. 1973b. "A note on the placement of
universal quantifiers." EWPL 2, 24-36.

- Anderson, J.M. 1973c. "Universal quantifiers." Lingua 31, 125-176.
- Anderson, J.M. 1974. "All and equi ride again." ArchL (ns) 5, 1-10.
- Anderson, J.M. forthcoming. "Existential quantifiers." AL 15; 1-27.
- Bach, E. 1964. An Introduction to Transformational Grammars. New York: Holt, Rinehart and Winston.
- Bach, E. 1968. "Nouns and noun phrases." In Bach and Harms (1968:90-122).
- Bach, E. and R.T. Harms (eds.), 1968. Universals in Linguistic Theory. New York: Holt, Rinehart and Winston.
- Bell, J.M. 1972. "Referential opacity and deep structure." YPL 2, 7-32.
- Bever, T.G. and D.T. Langendoen, 1972. "The interaction of speech perception and grammatical structure in the evolution of language." In Stockwell and Macaulay (1972:32-95).
- Bierwisch, M. and K.E. Heidolph (eds.), 1970. Progress in Linguistics. The Hague: Mouton.
- Bloomfield, L. 1935. Language. London: Allen and Unwin.
- Bodelsen, C.A. 1939. Review of Christophersen (1939). ES 21, 232-236.
- Bodelsen, C.A. 1949. Review of Jespersen (1949). ES 30, 281-287.

- Bolinger, D.L. 1960. "Linguistic science and linguistic engineering." Word 16, 374-391.
- Bolinger, D.L. 1971. "Semantic overloading: a restudy of the verb remind." Lg 47, 522-547.
- Bolinger, D.L. 1972. Degree Words. The Hague: Mouton.
- Campbell, A. 1959. Old English Grammar. Oxford: Oxford U.P.
- Carden, G. 1967. English Quantifiers. Harvard University: M.A. Thesis
- Carden, G. 1968. "English quantifiers." Harvard Computation Laboratory, Cambridge (Mass.): Report NSF-20, SIX.
- Carden, G. 1970a. "The deep structure of both." PCLS 6, 178-189.
- Carden, G. 1970b. "A note on conflicting idiolects." LIn 1, 281-289.
- Carden, G. 1970c. "On post-determiner quantifiers." LIn 1, 415-427.
- Carden, G. 1972. "Multiple dialects in multiple negation." PCLS 8, 32-40.
- Chomsky, N. 1957. Syntactic Structures. The Hague: Mouton.
- Chomsky, N. 1961. "On the notion 'Rule of Grammar'." Proceedings of the 12th Symposium in Applied Mathematics, 6-24. Providence (R.I.): American Mathematical Society. Reprinted in Fodor and Katz (1964:119-136).

Chomsky, N. 1965. Aspects of the Theory of Syntax.

Cambridge (Mass.): MIT Press.

Chomsky, N. 1966. Cartesian Linguistics. New York:

Harper and Row.

Chomsky, N. 1970. "Remarks on nominalisation." In
Jacobs and Rosenbaum (1970:184-221). Reprinted in
Chomsky (1972b:11-61).

Chomsky, N. 1971. "Deep structure, surface structure
and semantic interpretation." In Steinberg and
Jakobovits (1971:183-216). Reprinted in Chomsky
(1972b:62-119).

Chomsky, N. 1972a. "Some empirical issues in the theory
of transformational grammar." In Peters (1972:63-
130). Reprinted in Chomsky (1972b:120-202).

Chomsky, N. 1972b. Studies on Semantics in Generative
Grammar. The Hague: Mouton.

Chomsky, N. and M. Halle, 1968. The Sound Pattern of
English. New York: Harper and Row.

1939. Christophersen, P. The Articles. Copenhagen/Oxford:
Munksgaard/Oxford U.P.

Christophersen, P. and A.O. Sandved, 1969. An Advanced
English Grammar. London: Macmillan.

Collinson, W.E. 1937. Indication. Language Monograph
17, ed. A. Morris. Philadelphia: Linguistic Society
of America.

Curme, G.O. 1931. Syntax (A Grammar of the English
Language III). Boston: D.C. Heath.

- Delorme, E. and R.C. Dougherty, 1972. "Appositive NP constructions: we, the men; we men; I, a man; etc." FL 8, 2-29.
- Dineen, F.P. 1967. An Introduction to General Linguistics. New York: Holt, Rinehart and Winston.
- Dougherty, R.C. 1969. "An interpretive theory of pronominal reference." FL 5, 488-519.
- Dougherty, R.C. 1970. "A grammar of coordinate conjoined structures, I." Lg 46, 850-898.
- Dougherty, R.C. 1971. "A grammar of coordinate conjoined structures, II." Lg 47, 298-339.
- Emonds, J.E. 1970. Root and Structure-Preserving Transformations. Bloomington (Ind.): Indiana University Linguistics Club.
- Fairclough, N.L. 1973. "Relative clauses and performative verbs." LIn 4, 526-531.
- Fiengo, R. and H. Lasnik, 1972. "On nonrecoverable deletion in syntax." LIn 3, 528.
- Fiengo, R. and H. Lasnik, 1973. "The logical structure of reciprocal sentences in English." FL 9, 447-468.
- Fillmore, C.J. 1963. "The position of embedding transformations in a grammar." Word 19, 208-231.
- Fillmore, C.J. 1966a. "Deictic categories in the semantics of come." FL 2, 219-227.
- Fillmore, C.J. 1966b. "A proposal concerning English prepositions." MSLL 19, 19-33.

- Fillmore, C.J. 1967. "On the syntax of preverbs." Glossa 1, 91-125.
- Fillmore, C.J. 1968. "The case for case." In Bach and Harms (1968:1-88).
- Fillmore, C.J. and D.T. Langendoen (eds.), 1971. Studies in Linguistic Semantics. New York: Holt, Rinehart and Winston.
- Fodor, J.A. and J.J. Katz (eds.), 1964. The Structure of Language. Englewood Cliffs (N.J.): Prentice-Hall.
- Force, B. 1968. "Quantifiers as quantifiers." Washington: Center for Applied Linguistics.
- Fries, C.C. 1940. "On the development of the structural use of word-order in Modern English." Lg 16, 199-208.
- Fries, C.C. 1957. The Structure of English. London: Longmans.
- Geach, P.T. 1968. Reference and Generality, emended edition. Ithaca (N.Y.): Cornell U.P.
- Guillaume, G. 1919. Le Problème de l'Article. Paris: Librairie Hachette.
- Guillaume, G. 1944. "Particularisation et généralisation dans le système des articles français." FM 12, 89-107.
- Guillaume, G. 1945a. "Logique constructive interne du système des articles français." FM 13, 207-229.

- Guillaume, G. 1945b. "La question de l'article." FM 13, 70-82.
- Guillaume, G. 1971. Leçons de Linguistique de Gustave Guillaume 1948-1949. Structure Sémiologique et Structure Psychique de la Langue Française I, ed R. Lavin. Paris/Quebec: Klincksieck/Les Presses de l'Université Laval.
- Hale, A. 1970. "Conditions on English comparative clause pairings." In Jacobs and Rosenbaum (1970: 30-55).
- Hall, R.A. 1964. Introductory Linguistics. Philadelphia: Chilton.
- Hall, R.A. 1968. An Essay on Language. Philadelphia: Chilton.
- Harms, K.T. 1968. Introduction to Phonological Theory. Englewood Cliffs (N.J.): Prentice-Hall.
- Harris, J. 1771.³ Hermes. London.
- Heny, F.W. 1973. "Sentence and predicate modifiers in English." In Kimball (1973:217-245).
- Hewson, J. 1972. Article and Noun in English. The Hague: Mouton.
- Hill, A.A. 1966. "A re-examination of the English articles." MSLL 19, 217-231.
- Hjelmslev, L. 1928. "Principes de grammaire générale." HFM 16, I.
- Hockett, C.F. 1958. A Course in Modern Linguistics. New York: Macmillan.

- Hogg, R.M. 1970. Review of Hall (1968). Nph 54, 94-95.
- Hogg, R.M. 1972. "Quantifiers and possessives." Lingua 30, 227-233.
- Hogg, R.M. 1973a. "A grammar for both." EWPL 2, 93-108.
- Hogg, R.M. 1973b. "Is parentage a matter of coordination?" EWPL 2, 63-66.
- Hogg, R.M. 1974. "Another look at negation and quantifiers." EWPL 4, 20-23.
- Horn, L.R. 1971. "Negative transportation: unsafe at any speed?" PCLS 7, 120-133.
- Horne Tooke, J. 1798-1805. Diversions of Purley. London.
- Huddleston, R.D. 1971. The Sentence in Written English. Cambridge: Cambridge U.P.
- Iannucci, J.E. 1952. Lexical Number in Spanish Nouns. Philadelphia: University of Philadelphia Press.
- Jackendoff, R.S. 1968. "Quantifiers in English." FL 4, 422-442.
- Jackendoff, R.S. 1969. "An interpretive theory of negation." FL 5, 218-241.
- Jackendoff, R.S. 1971a. "Modal structure in semantic representation." LIn 2, 479-514.
- Jackendoff, R.S. 1971b. "On some questionable arguments about quantifiers and negation." Lg 47, 282-297.
- Jackendoff, R.S. 1971c. Review of Robbins (1968). FL 7, 138-142.
- Jackendoff, R.S. 1972a. "Any vs. every." LIn 3, 119-120.

- Jackendoff, R.S. 1972b. Semantic Interpretation in Generative Grammar. Cambridge (Mass.): MIT Press.
- Jacobs, R.A. and P.S. Rosenbaum (eds), 1970. Readings in English Transformational Grammar. Waltham (Mass.): Ginn-Blaisdell.
- Jespersen, O. 1914. A Modern English Grammar, II. Heidelberg: Carl Winters Universitätsverlag.
- Jespersen, O. 1924. The Philosophy of Grammar. London: Allen and Unwin.
- Jespersen, O. 1933. Essentials of English Grammar. London: Allen and Unwin.
- Jespersen, O. 1940. A Modern English Grammar, VI. Copenhagen: Munksgaard.
- Jespersen, O. 1949. A Modern English Grammar, VII. Copenhagen: Munksgaard.
- Johansson, S. 1974. "Yet another look at negation and quantifiers." EWPL 4, 24-30.
- Jones, C. 1967. "The functional motivation of linguistic change." ES 48, 97-111.
- Karttunen, L. 1971. "Definite descriptions with crossing coreference." FL 7, 157-182.
- Katz, J.J. and J.A. Fodor, 1963. "The structure of a semantic theory." Lg 39, 170-210. Reprinted in Fodor and Katz (1964:479-518).
- Katz, J.J. and P.M. Postal, 1964. An Integrated Theory of Linguistic Descriptions. Cambridge (Mass.): MIT Press.

- Kent, R.G. 1944. "The Old Persian relative and article." Lg 20, 1-10.
- Kimball, J.P. (ed.), 1973. Syntax and Semantics 2. New York: Seminar Press.
- Kirwin, W. 1968. "Either for any in Newfoundland." RLS 1, 8-10. St John's: Memorial University, Department of English Language.
- Kjellmer, G. 1971. Context and Meaning. Gothenburg: Acta Universitatis Gothoburgensis.
- Klima, E.S. 1964. "Negation in English." In Fodor and Katz (1964:246-323).
- Klooster, W.G. 1971. The Structure Underlying Measure Phrase Sentences. Utrecht State University: Ph.D.
- Kruisinga, E. 1932a.⁵ A Handbook of Present-Day English II. English Accidence and Syntax 2. Groningen: Noordhoff.
- Kruisinga, E. 1932b.⁵ A Handbook of Present-Day English II. English Accidence and Syntax 3. Groningen: Noordhoff.
- Lakoff, G. 1968. "Instrumental adverbs and the concept of deep structure." FL 4, 4-29.
- Lakoff, G. 1969. "On derivational constraints." PCLS 5, 117-139.
- Lakoff, G. 1970a. "Global rules." Lg 46, 627-638.
- Lakoff, G. 1970b. Irregularity in Syntax. New York: Holt, Rinehart and Winston.

- Lakoff, G. 1970c. "Pronominalisation, negation and the analysis of adverbs." In Jacobs and Rosenbaum (1970:145-165).
- Lakoff, G. 1970d. "Repartee." FL 6, 389-422.
- Lakoff, G. 1971a. "Adverbs and modal operators." Gothenburg: OSCULD.
- Lakoff, G. 1971b. "Adverbs and opacity: a reply to Stalnaker." Gothenburg: OSCULD.
- Lakoff, G. 1971c. "On generative semantics." In Steinberg and Jakobovits (1971:232-296).
- Lakoff, G. and S. Peters, 1966. "Phrasal conjunction and symmetric predicates." Harvard Computation Laboratory, Cambridge (Mass.): Report NSF-17, SVI. Reprinted in Reibel and Schane (1969:113-142).
- Lakoff, G. and J.R. Ross, 1968. "Ordering of transformational rules." Bloomington (Ind.): Indiana University Linguistics Club.
- Lakoff, R. 1969a. "Some reasons why there can't be any 'some-any' rule." Lg 45, 608-615.
- Lakoff, R. 1969b. "A syntactic argument for negative transportation." PCLS 5, 140-147.
- Lakoff, R. 1971. "If's, and's, and but's about conjunction." In Fillmore and Langendoen (1971:114-149).
- Langacker, R.W. 1969. "On pronominalisation and the chain of command." In Reibel and Schane (1969:160-186).

- Langacker, R.W. 1972. Fundamentals of Linguistic Analysis. New York: Harcourt, Brace, Jovanovich.
- Langendoen, D.T. 1969. The Study of Syntax. New York: Holt, Rinehart and Winston.
- Lee, D.A. 1971. "Quantifiers and identity in relativisation." Lingua 27, 1-19.
- Lee, D.A. 1972. "A reply to 'Quantifiers and possessives'." Lingua 30, 233-242.
- Leech, G.N. 1974. Semantics. Harmondsworth (Middx.): Penguin Books.
- Lees, R.B. 1961. "The constituent structure of noun phrases." AS 36, 159-168.
- Lindholm, J.M. 1969. "Negative-raising and sentence pronominalisation." PCLS 5, 148-158.
- Lyons, J. 1966. "Towards a 'notional' theory of the 'parts of speech'." JL 2, 209-236.
- Lyons, J. 1967. "A note on possessive, existential and locative sentences." FL 3, 390-396.
- Lyons, J. 1968. Introduction to Theoretical Linguistics. Cambridge: Cambridge U.P.
- McCawley, J.D. 1968. "The role of semantics in a grammar." In Bach and Harms (1968:124-169).
- McCawley, J.D. 1969. "A note on multiple negations." Chicago: unpublished.
- McCawley, J.D. 1970. "English as a VSO language." Ig 46, 286-299.

- McCawley, J.D. 1971. "Where do noun phrases come from?"
In Steirberg and Jakobovits (1971:217-231).
- McIntosh, A. 1968. "Determiners." Edinburgh:unpublished.
- Maetzner, E. 1874a. An English Grammar I, translated by
C.J. Grece. London: Murray.
- Maetzner, E. 1874b. An English Grammar II, translated
by C.J. Grece. London: Murray.
- Michael, I, 1970. English Grammatical Categories and
the Tradition to 1800. Cambridge: Cambridge U.P.
- Miller, J.E. 1972. "Towards an account of quantifiers
in Russian." EWPL 1, 73-91.
- Morgan, J.L. 1972. "Verb agreement as a rule of English."
PCLS 8, 278-286.
- Morris, I. 1969. "The relative clause in broad perspec-
tive." EIT 23, 246-253.
- Mustanoja, T.F. 1960. A Middle English Syntax I.
Helsinki: Société Néophilologique (Memoires de la
Société Néophilologique 23).
- Newton, B.E. 1971. "Ordering paradoxes in phonology."
JL 7, 31-53.
- OED 1933. The Oxford English Dictionary, ed. J.A.H.
Murray, H. Bradley, W.A. Craigie and C.T. Onions.
Oxford: Oxford U.P.
- Olszewsky, T.M. (ed.) 1969. Problems in the Philosophy
of Language. New York: Holt, Rinehart and Winston.
- Partee, B. Hall, 1970. "Negation, conjunction and
quantifiers: syntax vs. semantics." FL 6, 153-165.

- Partee, B. Hall, 1971. "On the requirement that transformations preserve meaning." In Fillmore and Langendoen (1971:1-21).
- Perlmutter, D.M. 1970. "On the article in English." In Bierwisch and Heidolph (1970:233-248).
- Perlmutter, D.M. 1972. "A note on syntactic and semantic number in English." LIn 3, 243-246.
- Peters, S. (ed.), 1972. Goals of Linguistic Theory. Englewood Cliffs (N.J.): Prentice-Hall.
- Plötz, S. 1972. Simple Copula Structures in English. Frankfurt: Athenäum Verlag.
- Postal, P.M. 1966. "On so-called 'pronouns' in English." MSLL 19, 177-206.
- Postal, P.M. 1970. "On the surface verb remind." LIn 1, 37-120. Reprinted in Fillmore and Langendoen (1971:180-270).
- Postal, P.M. 1971. Cross-Over Phenomena. New York: Holt, Rinehart and Winston.
- Postal, P.M. 1972. "A global constraint on pronominalisation." LIn 3, 35-59.
- Poutsma, H. 1904. A Grammar of Late Modern English I. Groningen: Noordhoff.
- Poutsma, H. 1914-1916. A Grammar of Late Modern English II. Groningen: Noordhoff.
- Pullum, G. and L. Humberstone, 1971. "Science, theory and dogma." YPL 1, 123-131.

- Quine, W.V.O. 1960. Word and Object. Cambridge (Mass.): MIT Press.
- Quine, W.V.O. 1961. "Logic as a source of syntactical insights." Proceedings of the 12th Symposium in Applied Mathematics, 1-5. Providence (R.I.): American Mathematical Society.
- Quirk, R., S. Greenbaum, G.N. Leech and J. Svartvik, 1972. A Grammar of Contemporary English. London: Longmans.
- Reibel, D.A. and S.A. Schane (eds.), 1969. Modern Studies in English. Englewood Cliffs (N.J.): Prentice-Hall.
- Reichenbach, H. 1947. Elements of Symbolic Logic. New York: Macmillan.
- Robbins, B.L. 1968. The Definite Article in English Transformations. The Hague: Mouton.
- Roberts, P. 1962. English Sentences. New York: Harcourt, Brace and World.
- Roberts, P. 1964. English Syntax. New York: Harcourt, Brace and World.
- Robins, R.H. 1951. Ancient and Medieval Grammatical Theory in Europe. London: Bell.
- Robins, R.H. 1967. A Short History of Linguistics. London: Longmans.
- Rosenbaum, P.S. 1967. The Grammar of English Predicate Complement Constructions. Cambridge (Mass.): MIT Press.

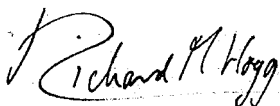
- Ross, J.R. 1967. Constraints on Variables in Syntax.
MIT: Ph.D.
- Ross, J.R. 1970. "On declarative sentences." In Jacobs
and Rosenbaum (1970:222-272).
- Ross, J.R. 1972. "The category Squish: endstation
hauptwort." PCLS 8, 316-328.
- Ross, J.R. 1973. "Clause-matiness." Paper given at the
Formal Semantics Colloquium, Cambridge, April 1973.
- Salmon, V. 1969. Review of Chomsky (1966). JL 5, 165-
187.
- Sampson, G. 1972. "There₁, there₂." JL 8, 111-117.
- Scheurweghs, G. 1959. Present-Day English Syntax.
London: Longmans
- Seuren, P.A.M. 1969. Operators and Nucleus. Cambridge:
Cambridge U.P.
- Smith, C.S. 1964. "A class of complex modifiers in
English." Lg 37, 342-365.
- Smith, C.S. 1964. "Determiners and relative clauses in
a generative-grammar of English." Lg 40, 37-52.
- Smith, I.G. 1963. The Articles. Edinburgh: Dissertation
for the Diploma in Applied Linguistics.
- Smith, S.B. 1972. "Relations of inclusion." Lg 48,
276-284.
- Sommerstein, A.H. 1972. "On the so-called definite
article." LIn 3, 197-209.
- Sommerstein, A.H. 1974. Review of Hewson (1972).
Lingua 33, 367-376.

- Sopher, H. 1969. "The classification of relative clauses." ELT 23, 254-258.
- Sørensen, H.S. 1958. Word-Classes in Modern English. Copenhagen: C.E. Gad.
- Steinberg, D.D. and L.A. Jakobovits (eds.), 1971. Semantics. Cambridge: Cambridge U.P.
- Stockwell, R.P. and K.S. Macaulay (eds.), 1972. Linguistic Change and Generative Theory. Bloomington (Ind.): Indiana U.P.
- Stockwell, R.P., P. Schachter and B. Hall Partee, 1972. The Major Syntactic Structures of English. New York: Holt, Rinehart and Winston.
- Strang, B.M.H. 1962. Modern English Structure. London: Longmans.
- Strawson, P.F. 1970. "Grammar and philosophy." Proceedings of the Aristotelian Society 70, 1-20.
- Sweet, H. 1898. A New English Grammar II. Oxford: Oxford U.P.
- Thomas, O. 1965. Transformational Grammar and the Teacher of English. New York: Holt, Rinehart and Winston.
- Thompson, S. Annear, 1971. "The deep structure of relative clauses." In Fillmore and Langendoen (1971:78-94).
- Thorne, J.P. 1966. "English imperative sentences." JL 2, 69-78.
- Thorne, J.P. 1972. "On the notion 'definite'." FL 8, 562-568.

- Thorne, J.P. 1974. "Notes on 'Notes on the notion
"definite'". FL 11, 111-114.
- TLS 1972. Anonymous review of Guillaume (1971). Times
Literary Supplement 3679 (1/9/72), 1030.
- Toussaint, M. 1967. "Gustave Guillaume et l'actualité
linguistique." Langages 7, 93-100.
- Turner, W. 1710. A Short Grammar for the English Tongue.
London.
- Vendler, Z. 1967. Linguistics in Philosophy. Ithaca
(N.Y.): Cornell U.P.
- Vetter, D. 1968. "Quantifiers, definite articles, and
relative clauses." MIT: unpublished.
- Wallis, J. 1653. Grammatica Linguae Anglicanae. Oxford.
Facsimile reprint 1970, Menston (Yorks.): Scolar
Press.
- Wharton, J. 1654. The English Grammar. London. Facsim-
ile reprint 1970, Menston (Yorks.): Scolar Press.
- Wright, J. 1954.² Grammar of the Gothic Language, ed.
O. Sayce. Oxford: Oxford U.P.
- Yotsukura, S. 1970. The Articles in English. The
Hague: Mouton.
- Zandvoort, R.W. 1957. A Handbook of English Grammar.
London: Longmans.

Declaration

I hereby certify that the work for this thesis
was completed by me:

A handwritten signature in cursive script that reads "Richard M. Hogg". The signature is written in dark ink on a light background.

Richard M. Hogg

Farleton, Carnforth,

6th September, 1974.

M.

ND QUANTIFIER

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2 VOLUMES

