

Determiner and Quantifier Systems in Contemporary English

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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 their 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 Kruisinga 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 συνδεσμος ("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 $\acute{\omicron}$, $\acute{\eta}$, $\tau\acute{\omicron}$ and the latter was $\acute{\omicron}$, $\acute{\eta}$, $\acute{\omicron}$. 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 percipient 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 Kruisnga (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 Kruisinga'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, Chrstitophersen (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."

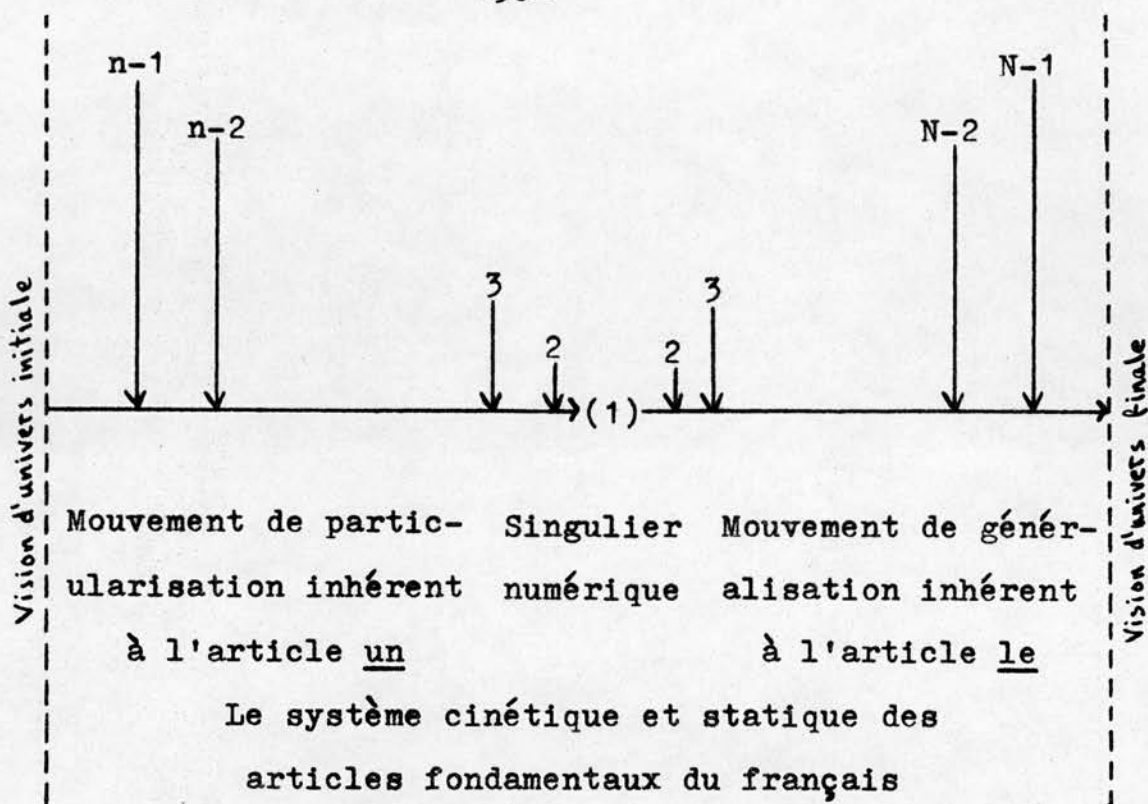
The topic of all the above-mentioned works by Guillaume is the two French 'articles' le (la, les, l') and un (une), and it is rarely that he strays further than the partitives (de, etc.). As we have seen above, in our discussion of some of the classical grammarians, this is almost certainly a mistaken 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

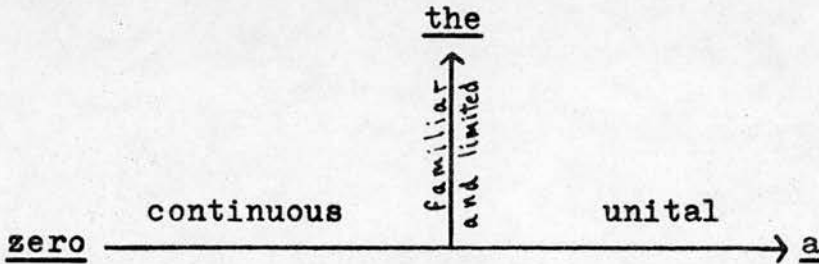
³ As will be seen, Christophersen's work goes far beyond Guillaume's both in merit and in its implications for a linguistic theory of the 'articles'. Thus it is somewhat unfair to describe him as a follower of Guillaume. At the same time, however, we have to recognise that Christophersen's theories rest 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 (1949:479) 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 quantifier systems ~~it is~~ meant those studies which have as their basis the attempt at a classificatory description 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 'numeral'. 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 semantics

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 ecpora, 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 two-fold. 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
teetotaller

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):

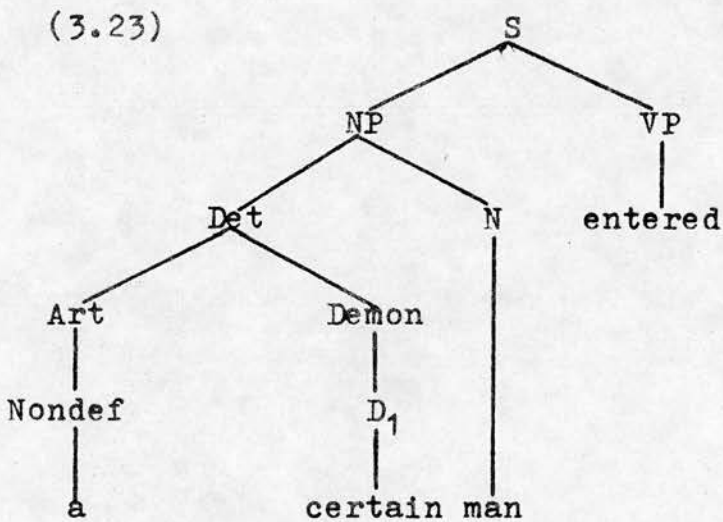
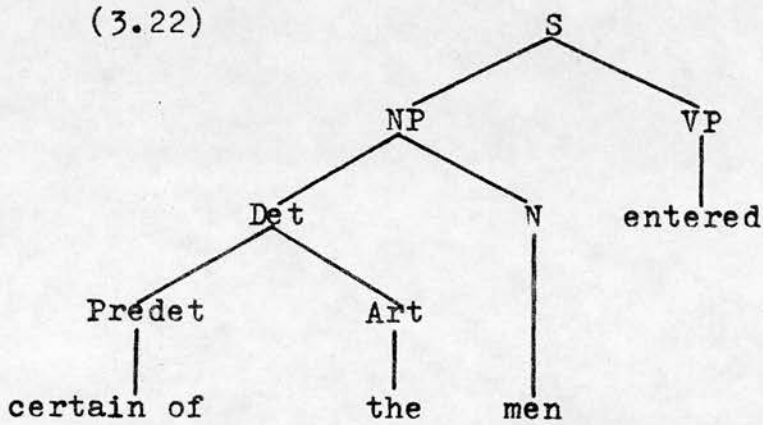
" D_1 and D_2 in combination with Def clearly contain the meanings nearness and remoteness; this and that. In combination with Nondef, this meaning contrast is not so clear, though one could perhaps argue that a certain is more 'near' than some."

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:

(i) 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.

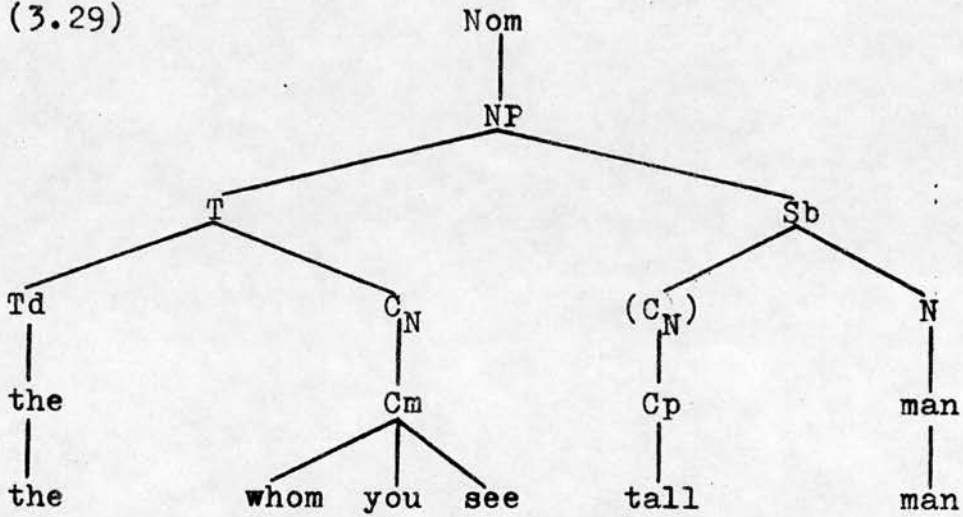
solvable by the kind of simple there-transformation which Roberts suggests. At the least, and this is a question to which we shall have to return, it necessarily involves an analysis of the internal syntax of quantifiers, at which Roberts makes no serious attempt.

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)

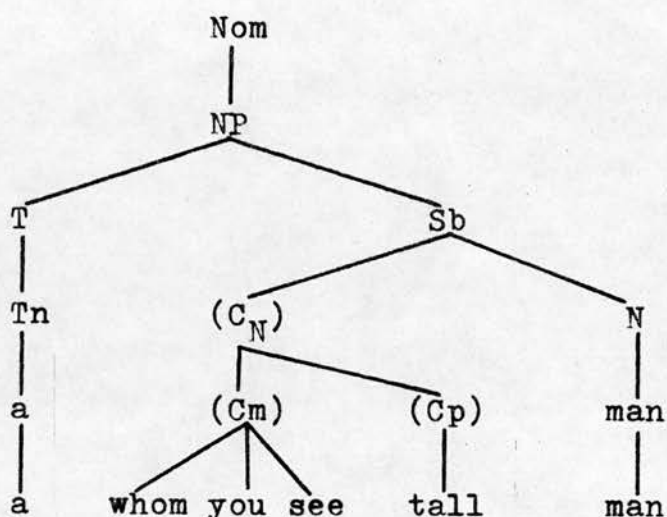


On the other hand, for (30) the underlying phrase marker (31) is suggested by Lees (1961:165):

(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 published 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 Chomskyan 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 definitive, 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 motor-bike 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:

- (i) 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 [±dual], 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. Non-count 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

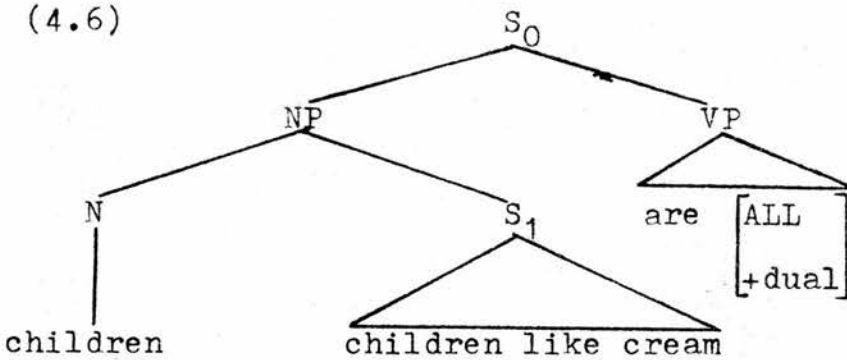
Let us accept, pro tempore, the analysis for quantifiers commonly known as the 'Lakoff-Carden hypothesis', for despite the inadequacies pointed out by, for example, Lakoff (1970d), it is quite adequate for our argument at present.¹ We can now, if we incorporate the

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:

- (i) 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 children, 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 corresponding 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:

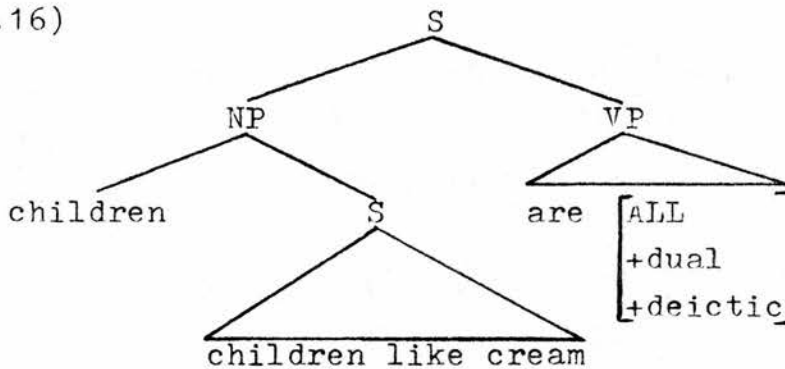
(4.15) Do you happen to know which
particular children?

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.

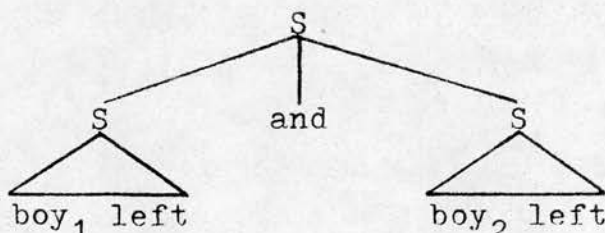
4.2 'Both' as a deep structure coordinator

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)



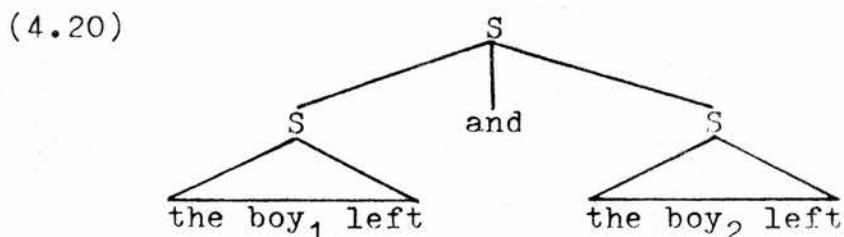
A rule of "Both-Formation" (BF), which is a variant of

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:



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

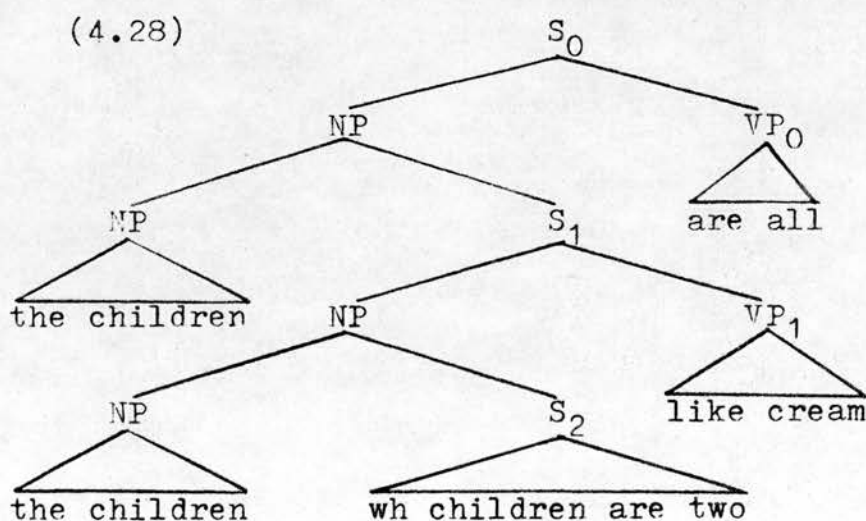
(4.27) All of the six children like cream

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:

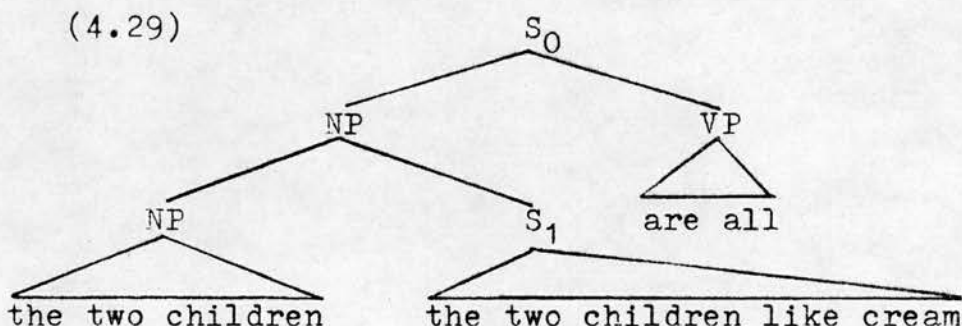
⁶ Other, more marked, members of this class include the demonstratives this and that and the possessive pronouns. For further discussion see Chapter 12.

(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).

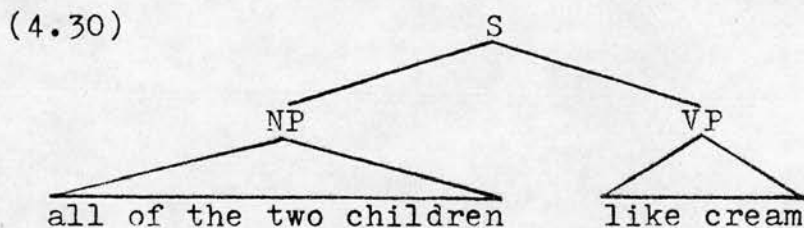
In order to capture the generalisations which flow from an analysis of (26), let us assume that we can derive quantifiers from a higher predicate even when there is a 'definite' NP collocating, perhaps in contrast to the unclear statement of Lakoff (1970d), but apparently in line with Carden (1968), 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

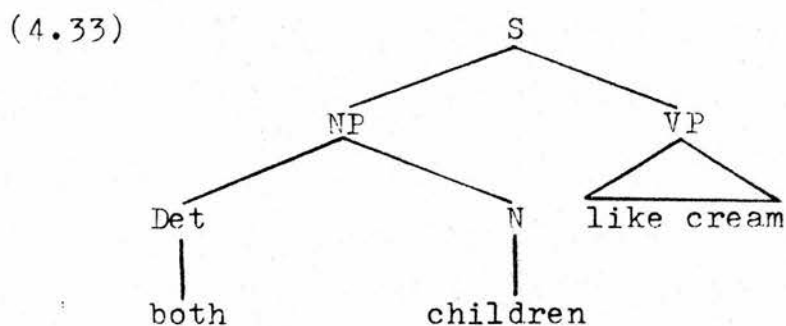
where X and Y may be zero; the reason for this will become clearer in the discussion below, and if we were to discuss the syntax of other quantifiers, for example either, a more precise formulation of it would be necessary, since Dual Copy has rather wider powers than are accounted for by (31). It might 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:



4.4 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.

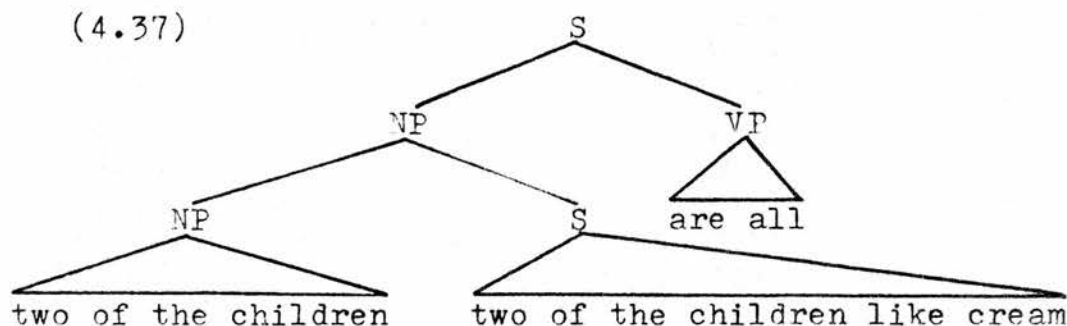
But now note that (35) might be expected to show exactly those features represented in (34):

(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

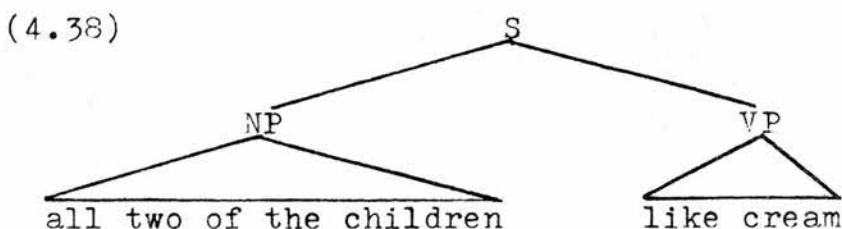
permissible surface structure sequence. Further, our previous arguments point to the fact that (35) is identical in meaning to:

(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-Carden 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.

Chapter 5

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 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.

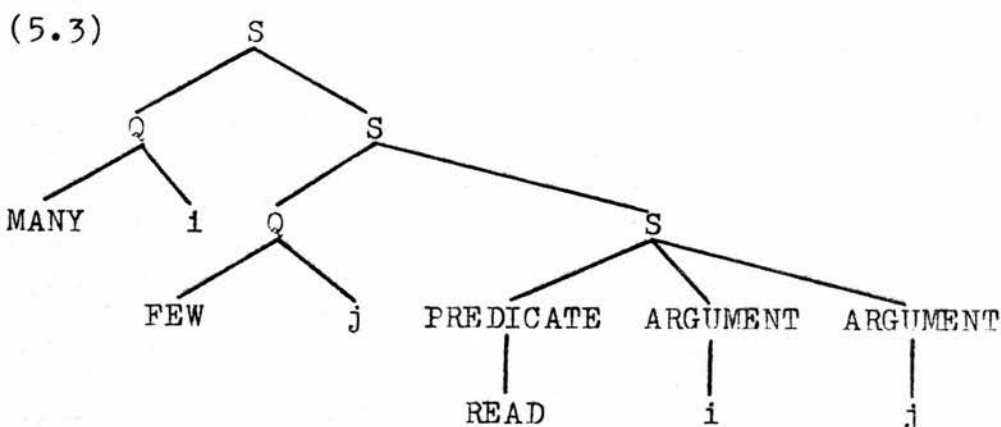
In this respect it might be mentioned that the structures footnoted in Lakoff (1971c:239) do not appear to correspond to this theory, for here, although quantifiers are in a higher S, they are directly dominated by "Q" and appear to be neither 'predicates' nor 'arguments'. Thus, for the interpretation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

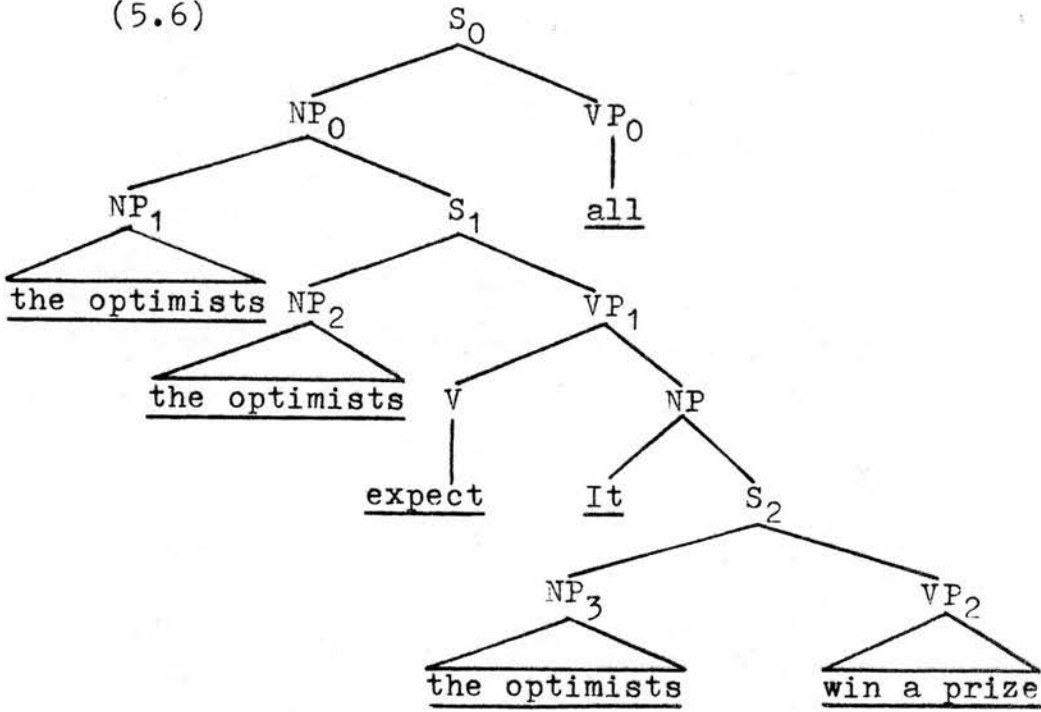
(5.4) All optimists expect to be President

(5.5) All optimists expect all optimists
to be President

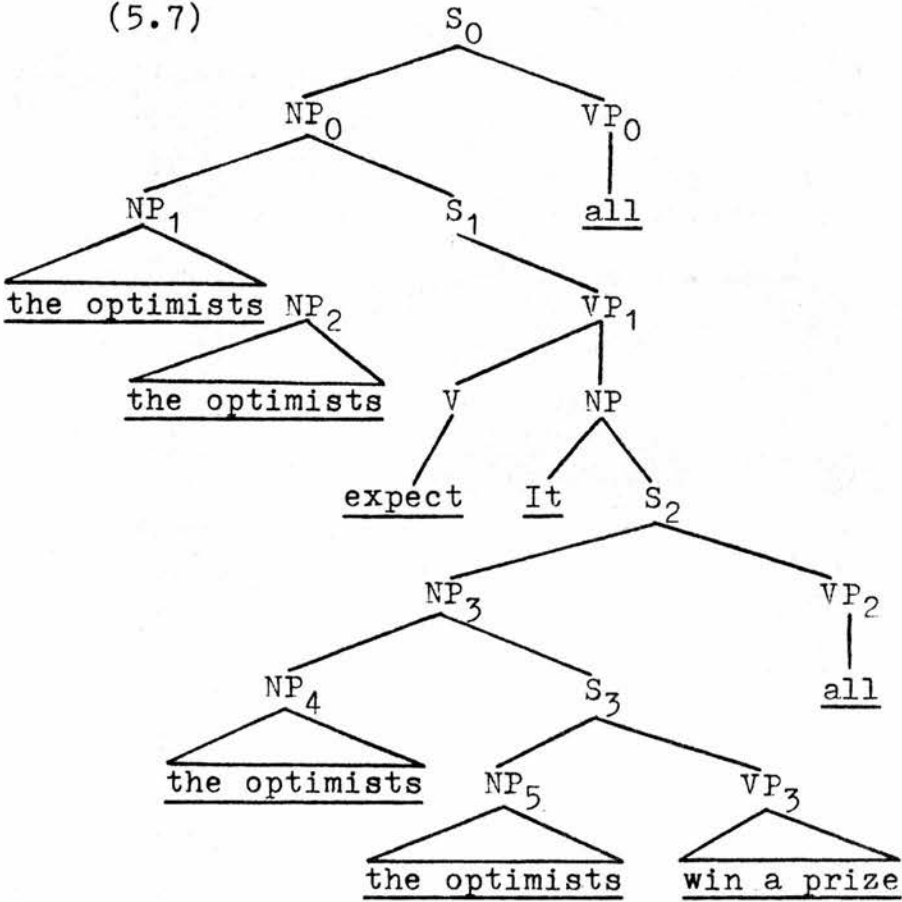
are not paraphrases of one another, the generative semantics theory of grammar demands that they have different underlying structures, the one for (4) permitting Equi-NP, the one for (5) not doing so. (6) and (7) are such underlying structures, for if we accept quantifier-lowering as ordered after Equi-NP 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 sena-
tors 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 [*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.

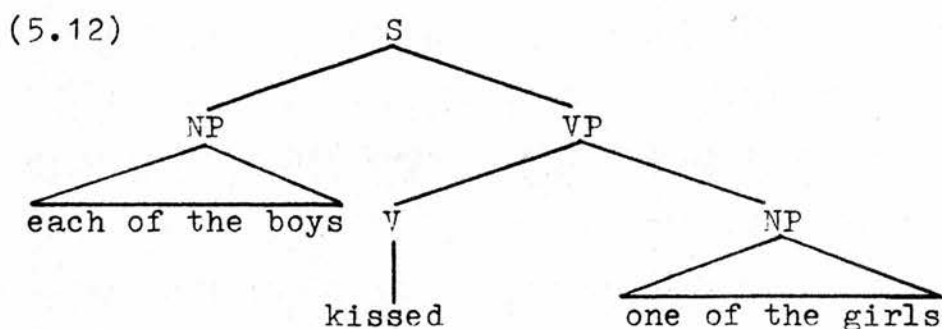
This complication, together with the facts mentioned in footnote 5, suggest that it is basically wrong to

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 unwieldly - 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

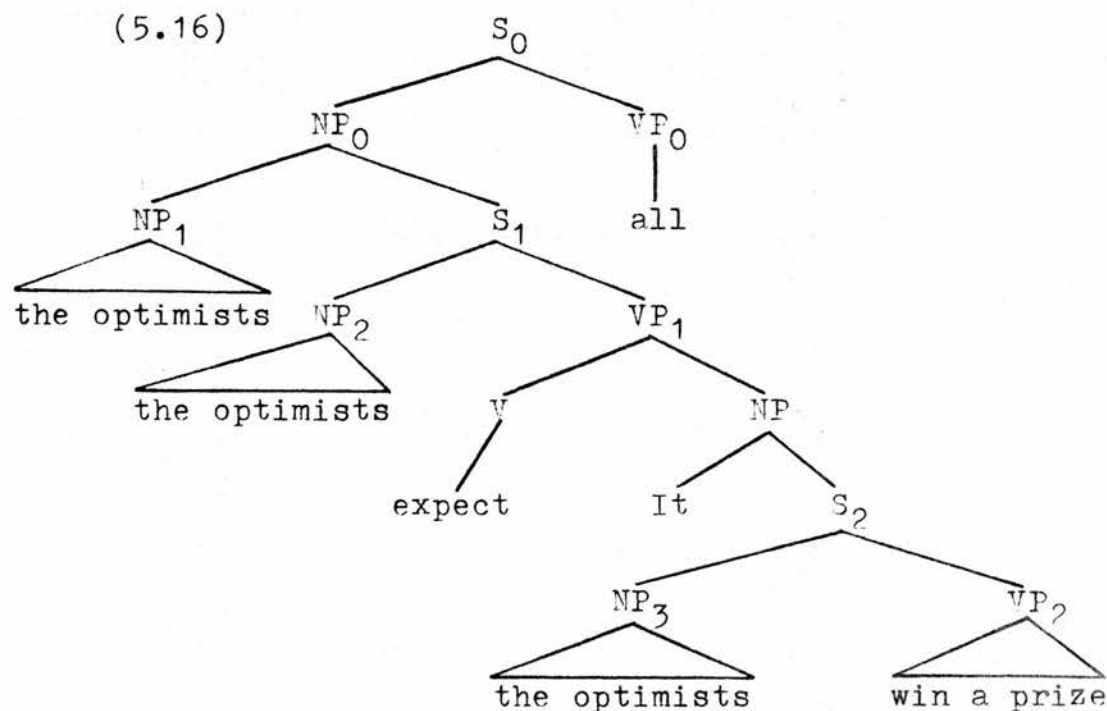
⁸ Since the consequences follow in either case, I have merely taken the simpler of the alternatives.

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 Partee (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

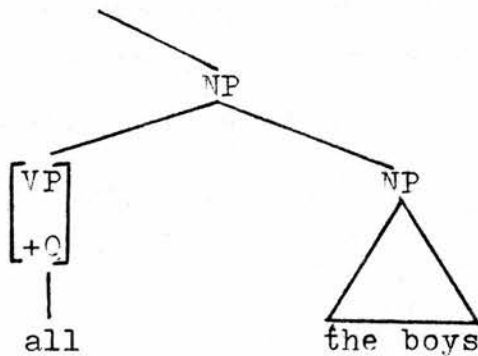
that can appear in predicate position can appear as postdeterminer quantifiers. Therefore, the question which is at issue is whether or not the mechanism of quantifier-lowering adequately explains the surface structure patterning. Lakoff's argument in favour of quantifier-lowering, and thus considering 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 Carden 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 Carden 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.

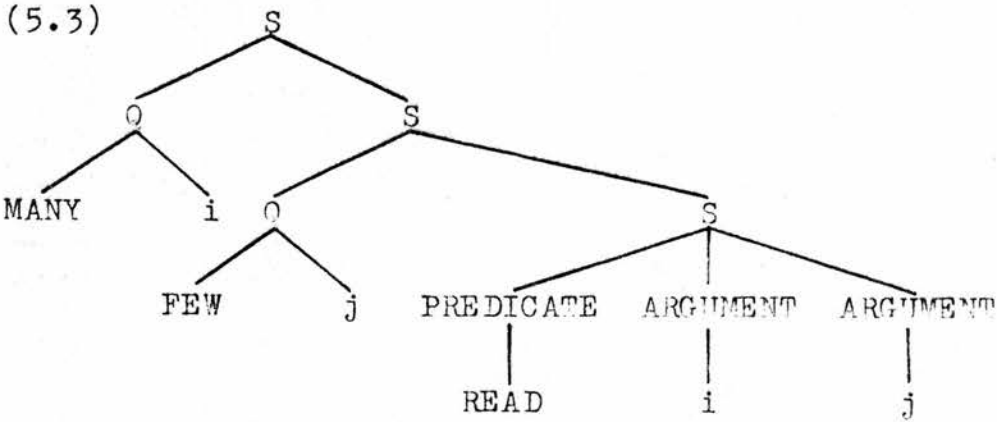
The best that can be stated for the quantifier-lowering hypothesis, therefore, is that it is in need of considerable reformulation in order to account for the discrepancies mentioned here. Nevertheless, any alternative hypothesis will have to account for such facts in a more consistent and well-motivated 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:

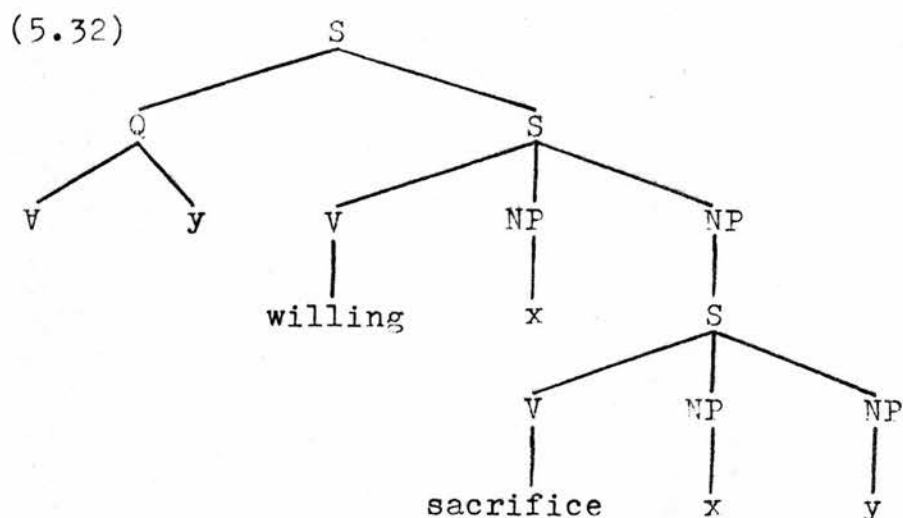
(5.2) Many men read few books



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:



(5.33) [(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: 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.

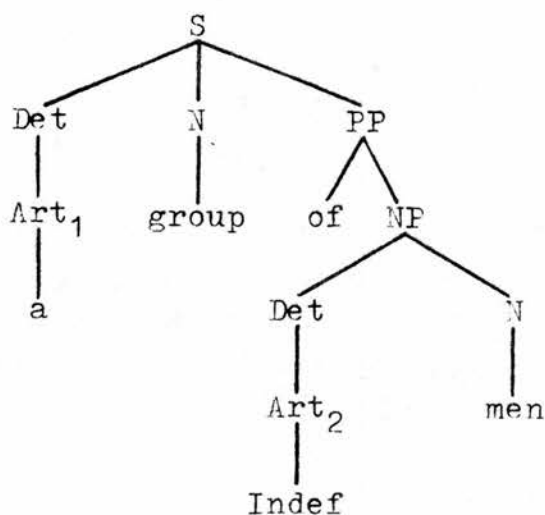
6.3 Quantifiers as nouns

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, and 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 Carden (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 reducible 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 reducable 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.

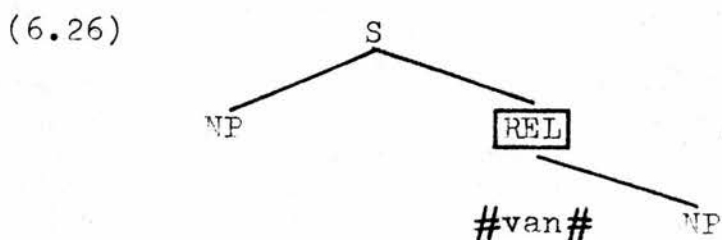
It will be necessary now to omit from our discussion the partitive-type constructions illustrated by (6b), which, contrary to my suggestion in Hogg (1972), are rather different from (6a). It must be said in passing, however, that this fact is also a disadvantage for both the Lakoff-Carden theory and 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:

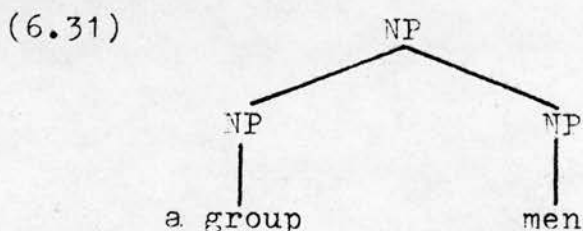
(6.29) John gave the girl a kiss

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

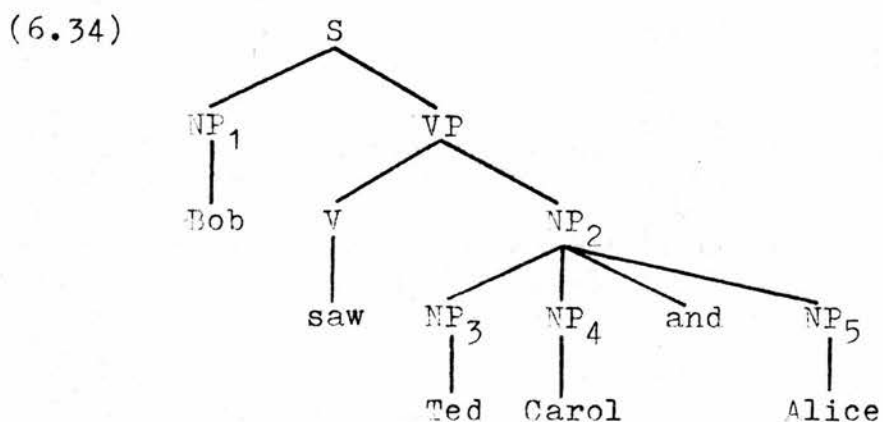
³ Of course, it remains to be shown that the two nouns in the examples of (30) are dominated by identical case nodes. Also, it is useful to compare here the Dutch forms een groep mannen and een liter whisky.

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.

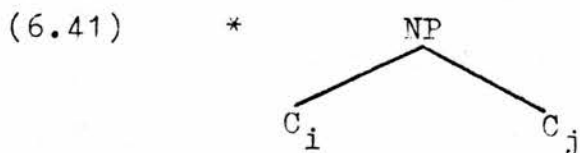
Jackendoff (1968:428) claims, however, to have found several counter-examples to this solution, which are to be found in the paradigm:

- (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:

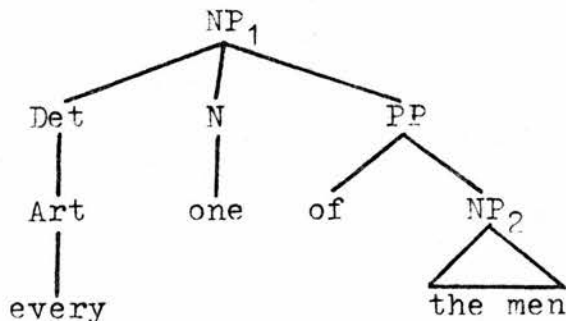
- (6.46) a *The some men
- b Each (one) of the men

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:

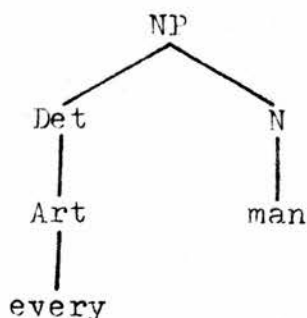
(6.47) Every one of the men

(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 Carden (1970c) use the same fact to

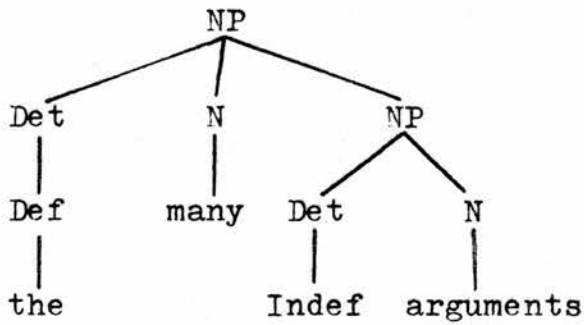
justify their claim that all quantifiers are predicates in underlying structure. In §5.3 we discussed the validity of that claim and concluded that although the evidence which had been cited did suggest that the 'absolute' quantifiers involved an underlying predication, the hypothesis that any or all 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-Carden 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 Carden 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 [\pm 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, \underline{a}_1 which would be [+X] and \underline{a}_2 which would be [-X]:

(6.55) a John bought \underline{a}_1 house

b John didn't buy \underline{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 \underline{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 \underline{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 blockes 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

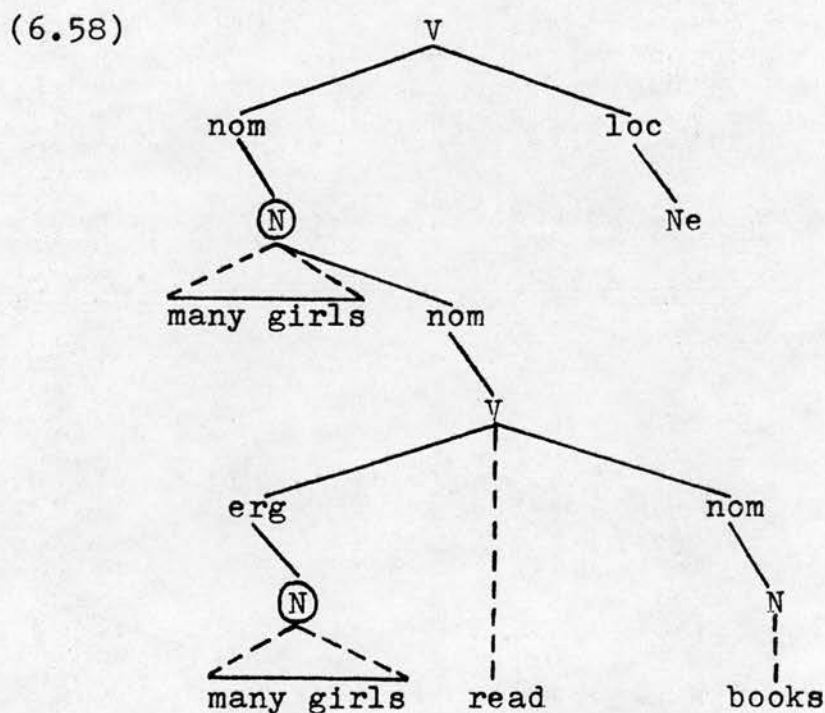
the one that I wish to propose, and so the differences which do exist are discussed then, otherwise the proposed solution will be over-anticipated and thus prejudiced.

One point, however, is worth mentioning now, and that is that Anderson works within a theory of dependency case grammar, as set out in Anderson (1971b).

Therefore, for:

(6.57) Many girls read books

Anderson (1973c:125) gives the following structure:



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.