

INTRODUCTION

I

THE purpose of this collection of papers is to give an idea of the essential issues involved in the development, in the wake of Chomsky's *Aspects of the Theory of Syntax* (1965), of what is widely known as 'generative semantics'. (For reasons explained in paper IV, I shall use the term 'semantic syntax' instead of 'generative semantics'.) Given considerations of size and scope, it seemed that this purpose was best served by concentrating on two main issues, the insertion of lexical items and the grammatical status of logical elements, especially operators. It is around these two categories of problems that the mainstream of linguistic theory has revolved since 1965.

It was inevitable that a number of theoretical developments should be excluded. No attention could be paid to such interesting aspects of contemporary philosophical and linguistic thought as the theory of presupposition, the status of variables in grammatical analysis, the interaction of reference and grammar, the grammatical status of performative verbs, questions of complementation, universal constraints on rules of grammar, Fillmore's 'Case Grammar', Postal's theory of anaphoric islands, etc. No value judgement is implicit in their exclusion.

This book is partisan in that a favourable view is taken of semantic syntax as against the theory of autonomous syntax defended by Chomsky and others. This is, in a way, a matter of course, since the writers who have expounded the theory of semantic syntax most clearly have also promoted it. Apart from that, however, the partisan attitude is motivated by the feeling that the arguments advanced against semantic syntax do not, on the whole, carry conviction. They are sometimes based on a false conception of the essential issues involved, sometimes also on incorrect reasoning. And where they do cut ice, they reveal problems which are indifferent to the issue at hand because they will have to be faced by anyone who attempts an explanation of the facts of language by providing an adequate description of linguistic competence and the innate capacity for language acquisition. There is, moreover, a conspicuous tendency in autonomous syntax to be casual about the explanation of semantic facts.

This introduction is mainly devoted to a demonstration that these blunt allegations are well founded. It is, to a large extent, polemical. In arguing against autonomous syntax I have singled out Chomsky as the target of

attack, mainly because he is by far the most prominent defender of autonomous syntax in its most tenable form. The arguments against the positions defended by him apply, essentially, to all other writings in the same vein, on which Chomsky often relies heavily anyway. My critical attitude towards Chomsky's work after 1965 should not, however, be taken to imply a disposition to detract from his standing as a great innovator in linguistic theory.

11

On a number of essential points we agree with Chomsky: these are points universally accepted in Transformational Grammar. A grammar is taken to be a *theory of a language*, in so far as it attempts to reconstruct, as true to nature as is possible, the internal organization and the contents of what constitutes a native speaker's knowledge of his language, his linguistic competence. Careful observation of the facts of a language reveals that such a reconstruction is a far from trivial matter. For a native speaker to be able to know what sentences are well formed or grammatical, what the sentences can mean, and how they can be pronounced, he must have at his disposal a vast complex of highly specific rules or principles. Since these are, for the most part, well beyond possible awareness (they constitute 'tacit' or 'implicit' knowledge), and since they are not open to inspection in any way, there seems to be little else one can do in order to arrive at an adequate description of them than set up sensible hypotheses, which, together, will form a theory, a proposed grammar of the language. We require of the theory (grammar) not only that it should consistently make correct predictions about the facts of the language, but also that, in doing so, it should make as small a number of assumptions as possible to account for as large a quantity of facts as possible: the theory should have a maximum explanatory 'yield' at a minimum 'cost'. This latter requirement is known as the simplicity criterion.

Moreover, for an infant to be able to acquire essential competence in the language (or languages) of its environment in the course of a few years on the basis of scattered utterances which are often sloppily, and hardly ever systematically, presented, it must have at its disposal a set of highly specific expectations and strategies concerning human language. We speak of an innate language acquisition capacity, without which an infant would not, under any rational assumption, be able to settle infallibly for the correct grammar in such a short time. The fact that all infants go through essentially the same stages in the same order during the process of language acquisition, irrespective of intelligence, race, or cultural background (but

not irrespective of age), compared with the boundless range of mathematically possible or thinkable grammars, makes it necessary to assume that the infant's inductive process is strictly pre-programmed. Universal linguistic theory can be regarded as an attempt at reconstructing the innate human language acquisition device.

If a grammar G_1 of a language L conforms better with what is known of grammars of other languages than a rival grammar G_2 of the same language, G_1 is considered more highly valued than G_2 because G_1 allows us to generalize in greater detail about the nature of human language than G_2 . The more specific our notions of what constitutes a possible human language (or its grammar), the closer we are to understanding how an infant goes about in selecting the correct grammar on the basis of scarce evidence: it will be constrained, in its selection, by highly specific limitations on the form of possible grammars. On the other hand, we wish to restrict to a minimum the number of principles defining the notion 'possible human language', while accounting for the largest possible number of specific restrictions which our findings enable us to formulate for that notion. The simplicity criterion applies again, at a higher level of abstraction.¹ To the extent that a grammar makes correct predictions, achieves simplicity with respect to the language it describes, and helps to define the notion 'possible human language', to that extent it comes close to the ideal of explanatory adequacy: a fully correct account of linguistic competence as a result of learning guided by an innate language acquisition capacity.

III

The *Aspects*-theory of grammar was unsatisfactory in a number of respects. It was soon felt, not only that the semantic notions involved ('projection rule', 'semantic representation', etc.) were too vague and cloudy to be satisfactory, but also, and more specifically, that the very detailed notions of the lexicon and lexical insertion developed in *Aspects* did not do justice to the facts.

Two central claims are made, in *Aspects*, regarding lexical items. First, a lexical item is defined, in the lexicon, by a set of, presumably unordered, features of (sub)categorization and selection: each lexical item is given a positive or a negative value with regard to certain features, such as [\pm Noun], [\pm Animate], [\pm —NP]. Secondly, insertion of lexical items is said to take place 'in a block' at the end of the base, before any transformation can operate. All lexical items figure in syntactic deep structure. We shall refer to this latter principle as *pretransformational unitary lexical*

¹ This is very much what Chomsky stresses (1972a, p. 125), where he speaks of the child's 'inductive leap'. The point is taken up again in paper IV of this collection.

insertion. Although the *Aspects*-theory implies unitary lexical insertion, it does not imply that the items enter deep structures in their actual phonological form: later rules may add refinements as to the form which will be input to the phonological rules. In particular, so-called derived nominals, such as *destruction*, *refusal*, *laughter*, *proof*, are taken to be represented in syntactic deep structure (SDS) in the shape of their corresponding verbs: *destroy*, *refuse*, *laugh*, *prove*. The assumption is made that these nominals reflect embedded S's of a special kind, and are the result of certain nominalization transformations (in accordance with R. B. Lees, *The Grammar of English Nominalizations*, 1960). The main motivation for this theory of nominalizations is that their adjuncts reflect subjects or objects, and that they allow for passivization, as in *the destruction of the city by the enemy*.

In his 'Remarks on Nominalization', however, which was written in 1967, but not published until 1970 (I refer to 1972a), Chomsky draws attention to a number of difficulties which arise in the light of *Aspects*. First, the presumed S-embeddings must be of a special kind: derived nominals do not contain an element 'tense'.² Their internal structure is that of a noun phrase, with adjectives (not adverbs), and prepositional adjuncts. Their phonological shape is highly idiosyncratic (only, we should add, when they contain the aorist aspect: see footnote 2; in the repetitive-continuous aspect they are all regularly formed in *-ing*,—a not unfamiliar fact for anyone acquainted with various Indo-European languages). And finally, they exhibit specialized meanings, a fact which would infringe on the principle of the meaning-invariance of transformations (which Chomsky still upheld in 1967): in *the proof took two hours* the derived nominal has certain semantic elements which differ from those occurring in, e.g. *the proof is written on page 12*. These considerations led Chomsky to formulate his *lexicalist hypothesis*, according to which derived nominals are not derived from verbs in embedded S's: they originate as nouns. He assumes that there are certain lexical items which are double-faced: they can be inserted as nouns or as verbs, in base structures of essentially the same composition. That is, in the base the structure of a noun phrase is taken to mirror that of a sentence.

Chomsky writes (1972a, p. 17): 'We might extend the base rules to accommodate the derived nominals directly (I will refer to this as the "lexicalist position"), thus simplifying the transformational component.' And on p. 54: 'The strongest and most interesting conclusion that

² In the paper under discussion Chomsky does not mention the presence of verbal aspect in derived nominals. Later, in 'Some Empirical Issues' (1972a, p. 159) he denies this: 'derived nominals do not contain elements that are unique to verb phrases and never appear in other noun phrases, specifically aspect.' He must be wrong, however, since in *the theft of the jewellery* reference is made to a single event (aorist aspect), while in *the thieving of the jewellery* there is a repetitive-continuous aspect.

follows from the lexicalist hypothesis is that derived nominals should have the form of base sentences, whereas gerundive nominals [e.g. *John's carelessly driving the car*] may in general have the form of transforms.'

Notwithstanding this, Chomsky allows transformations to change the internal structure of derived nominals. In order to account for, e.g. *the destruction of the city by the enemy*, he proposes to formulate the passive transformation in such a way that it applies not only to sentences but also to noun phrases: if there is the optional element 'by + Δ ', the passive rule is put into action. Agent-postposing then results in the structure quoted above. Subsequent NP-Preposing gives *the city's destruction* (p. 43). A similar device would probably have to account for cases such as *the agreement between Bill and Fred*, which corresponds with the transform *it be agreed between Bill and Fred*, and not with any base structure. One can only conclude that, on Chomsky's own account, the lexicalist hypothesis is falsified, since it entails that 'derived nominals should have the form of base sentences' (p. 54). It is not clear why a structure such as *the city's being systematically destroyed by the enemy* should be said to have the form of a transform, but *the systematic destruction of the city by the enemy* the form of a 'base sentence'.

IV

Others take a view of lexical items which is, in many ways, directly opposed to Chomsky's. Instead of reducing the role of transformations in the grammar, they seek to increase it. Not only do they reject the lexicalist hypothesis, they posit a great deal of transformational syntax inside, or behind, lexical items, especially verbs. They claim that unordered sets of features are inadequate for a semantic definition of lexical items: such a definition requires a phrase-marker (labelled tree), often a transform, with more primitive, 'sub-lexical' items at the ends of branches. A lexical insertion rule is seen as a particular kind of transformation, replacing part of an underlying tree by a specific lexical item.

The first to develop a theory of the lexicon along these lines was Gruber, whose thesis of 1965 (*Studies in Lexical Relations*) already meant a radical departure from the views held by Chomsky. It was followed by *Functions of the Lexicon in Formal Descriptive Grammars* (1967). At the beginning of paper II, de Rijk discusses Gruber's theory in some detail, comparing it with McCawley's position with respect to the lexicon (paper I). Gruber still adhered to the principle of unitary lexical insertion, albeit with qualifications and some exceptions. It was soon found, however, that this principle was an obstacle, rather than a help, in explaining observed facts, and none of the later investigators maintained it. Insertion rules are now

generally held to operate in alternation with other syntactic transformations. Sometimes, however, a syntactic transformation (such as Predicate Raising in English: see the papers I, II, and III) is constrained in the sense that it creates a structure which *must* then be replaced by a lexical item. Such a constraint will fall under Lakoff's (perhaps somewhat heterogeneous) term 'global rule' or 'global constraint' (paper VI). Rules which are thus constrained belong exclusively to what is called 'prelexical syntax' (paper I).

Theories of the lexicon in terms of semantic syntax came about mainly as a result of attempts to account for certain regularities within the lexicon. Take, for example, the English verb *escape*, as in *John escaped from prison*, or *The Queen escaped the crowd*, or *That escapes me*. In none of these cases does the language allow for a passive: **Prison was escaped from by John*; **The crowd was escaped by the queen*; **I am escaped by that*. The same holds true for the verbs *go away from*, *get away from*: when used in their 'literal' sense i.e. as verbs of movement, they lack a passive, as is generally the case with *go* or *get* as verbs of movement. (Compare the difference between, e.g. **The church was gone into*, and *The problem was gone into*.) Rather than mark every individual verb for its passivizability, a grammar is to be preferred which predicts these cases wherever possible. In this case, one is tempted to think of a possible generalization to the effect that *escape* might be analysed as a verb of 'going', i.e. as 'go/get away from'. Two possibilities now come to mind. Either we assign to the verb a feature [+go], and define the passive in such a way that the presence of this feature makes it inapplicable, which would be an *Aspects*-type solution. Or else we assume *go* or *get* to be sub-lexical items, figuring in SDS. Passive is then ordered before the insertion of *escape*, so that it is automatically ruled out for this verb. No features are necessary now, since *escape* replaces a sub-tree. This would be a semantic syntax solution.

The proponents of semantic syntax consider lexical description in terms of features unsatisfactory for a number of reasons. In their opinion, items call for definitions in the form of syntactic trees, with hierarchies of labelled constituents, not unordered sets of features. Their reasons are both semantic and syntactic. Among the semantic arguments we can give the following. Take the verb *kill* (McCawley's stock example for his causative analysis, as in papers I and III). The sentence *Jack killed Fred* entails, among other things, that Fred was alive but is now dead. A semantic analysis of this sentence must, in order to be adequate, enable us to derive this entailment from it. If the semantic definition of *kill* were an unordered set of features, such as [+event], [+causative], [+dead], no entailments would follow but only vague associations. But if *kill* were to be represented as, for instance, 'cause to become dead', as part of a syntactic tree.

with a subject for 'cause', i.e. Jack, and a subject for 'dead', i.e. Fred, then the element 'become' will be seen to entail a transition from Fred's being alive to his being not alive, with Jack as the initiator of the process.

Another semantic argument is the following. Both the word *false* and the word *deny* contain, somehow, a negative element. This appears from a number of observations, some of which are presented in paper IV (examples 34, 36, 40). Thus, we can say *It's false to say anything like that*, but not **It's true to say anything like that*: the item *any* requires, somehow, a negative context. Likewise, we can say *He can eat more than would be true to say of me*, but not **He can eat more than would be false to say of me*: negative elements do not occur after comparative *than* (at least when there is also a finite verb form: *I am worse than nobody in this house*, but **I am worse than nobody is in this house*). The same tests apply to *deny* versus *assert*: *I deny (*assert) having said anything like that: He is richer than I asserted (*denied) he was*. If the lexical definitions of *false* and *deny* were to be given in feature-sets, both would have to be given the feature [+ not] or the like. But then it would not be expressed that in *false* the negation is the element with the wider scope, the 'higher' element, whereas it is the 'lower' element in *deny*. That is, *false* is definable as 'not true', but *deny that p* as 'assert that not p'. This difference is relevant for a semantic description of the sentences in which these words occur, as appears, for example, from the fact that there is a semantic near-equivalence between *I don't think that's true* and *In my opinion, that is false*, but not between *I don't think he asserted that p*, and *In my opinion, he denied that p*. The lack of equivalence in the latter case can be explained by the rule of Negative Raising (see papers VIII and IX), which is controlled by negative-raising verbs, such as *think*, and raises the negation from the complement-S into the higher S, but only if the negation comprises the whole remainder of the complement-S in its scope, i.e. if it is the highest operator of the complement-S. Thus, the sentence *I don't think he often overeats* has a near-equivalence to *I think he doesn't often overeat*, and not to *I think he often doesn't overeat*: in the latter, *often* is the highest operator, and *not* cannot be raised. It thus follows that *I don't think that's true* is nearly equivalent to *I think that's not true*, which is the same as *I think that's false*. But *I don't think he asserted that p* corresponds to *I think he didn't assert that p*, and not to *I think he asserted that not p*, which is the same as *I think he denied that p*.

The above argument is not purely semantic, it can easily be construed as having some relevance for syntax too. In other words, the mapping from 'not true' on to *false*, etc., is not just a matter of semantic description of sentences; the form 'not true' is likely to represent a stage in the syntactic transformational derivation of sentences containing the word *false*.

This follows from the fact that Negative Raising is, in fact, a rule of syntax (see papers VIII and IX), as can be seen, in brief, from the ungrammaticality of, e.g. **I don't think Fred may yet be at home*. A sentence of the form **Fred may yet be at home* is ungrammatical in its own right, and a pre-Negative-Raising 'I think Fred may not yet be at home' is ruled out because *not* is not the highest operator of the complement-S: the possibility operator *may* is the highest there.

A more directly syntactic argument for the thesis that lexical items are to be analysed in terms of syntactic trees is to be found in the fact that one often comes across lexical items with two different 'senses' which differ only in that one of the senses is the passive of the other, or involves the passive where the other does not. Thus Greek *typhlós*, or Latin *caecus*, means not only 'blind', 'who cannot see', but also 'obscure', 'invisible', 'which cannot be seen'. The same applies to English *blind* in certain idiomatic phrases, such as *blind turning* ('which cannot be seen around'), or *blind alley* ('which cannot be seen through'), or *blind rocks* ('which cannot be seen'). Likewise, the French *défendre* occurs not only in the sense of 'not allow to take', as in *défendre le vin à l'enfant* ('forbid the child to take wine'), but also as 'not allow to be taken', 'defend': *défendre la ville* ('defend the city').³ In general, the specification of lexical items often critically involves transforms, and therefore trees.

V

An interesting argument for semantic tree structure, rather than features, and in particular for McCawley's analysis of *kill* as 'cause to become dead', was presented by Morgan (1969, pp. 62-4). He observes the three-fold ambiguity of sentences such as *I almost killed Fred*, which can mean either (a) 'I almost acted in such a way that I killed Fred' (I was so enraged that I almost grabbed a knife to kill him), or (b) 'I acted in a way which almost killed Fred' (I almost gave him the wrong medicine, but I noticed the mistake in time), or (c) 'I caused Fred to become almost dead' (I beat him up very badly).⁴ A natural way of rendering these differences in semantic representations is to accept an analysis of the type proposed by McCawley for causative verbs such as *kill*: 'cause to become dead', and let *almost*, or whatever semantic structure corresponds to it, modify either *cause*, or *become*, or *dead*, for the readings (a), (b), and (c), respectively.

³ For a fuller account of the *défendre* argument, see my as yet unpublished paper (1972b), 'Predicate Raising and Dative in French and Sundry Languages'. The impossibility of a dative with the second sense of *défendre* indicates a passive source. The issue will be taken up again below.

⁴ It is interesting to note that in French the three readings translate differently. For (a): *J'aurais tué Fred*; for (b): *J'ai failli tuer Fred*; for (c): *J'ai presque tué Fred*.

Chomsky (1972a, p. 150) relates the ambiguity to:

verbs that specify a process that can reach a terminal point: e.g. *I almost solved the problem, I almost persuaded Bill to leave, The planes almost destroyed the city, etc.* . . . Yet in this case, there is little sense to the idea of an internal analysis, and surely no causative analysis. Again, the factual judgments seem to me insecure, but so far as they are clear at all, it seems that the whole matter simply has to do with verbs of process with a terminal point.

If Chomsky is right here, the problem of relating semantic structures to surface structures still remains. It is, moreover, unclear why there should be 'little sense to the idea of an internal analysis', or why *destroy* should not be a causative verb. Since Chomsky presents no arguments, but only impressions, Morgan's proposal is in no way weakened by Chomsky's counterproposal.

McCawley's analysis of *kill* does not meet with Chomsky's approval. In various papers Chomsky points out that there are serious difficulties connected with it. Thus, speaking of the relation between the actual lexical item (*kill*) and its postulated semantic analysis, he says (1972a, p. 72):

The relation could not be identity, however. As has often been remarked, 'causative' verbs such as *kill, raise, burn* (as in *John burned the toast*), etc., differ in meaning from the associated phrases *cause to die, cause to rise, cause to burn*, etc., in that they imply a directness of connection between the agent and the resulting event that is lacking in the latter case. Thus John's negligence can cause the toast to burn, but it cannot burn the toast. Similarly, I can cause someone to die by arranging for him to drive cross-country with a pathological murderer, but I could not properly be said to have killed him, in this case.

This difficulty is not very serious, however, in the present context. All that is at issue is a more precise definition of the notion of causality involved in the causative analysis of verbs. And it seems, moreover, that Chomsky's proposal for a more precise definition of that notion is inadequate. Rather than say that there must be 'a directness of connection between the agent and the resulting event' in the case of lexical items, one should stipulate that there must be fully determined causation (the result must be inescapably caused), not possible causation: John's negligence does not necessarily cause the toast to burn—there must be other contributing factors to enable one to predict with certainty that the toast will burn. Likewise with the pathological murderer. Directness of connection is not necessary, since a sinister agent can give me a fatal injection with delayed effect while I board the plane in Tokyo, so that I die upon arrival in London half a day later. In this case the agent has undoubtedly killed me.

More serious is the difficulty (1972a, pp. 72, 143) that items such as *murder* or *assassinate* will equally require the semantic structure 'cause to become dead', but for *murder*, the sub-tree would have to contain, more or less, the following elements: 'cause to die by unlawful means and with

malice aforethought', and it must be specified that the object is human. *Assassinate* would be like *murder*, Chomsky says, but with the further specification that the object is an important public personality.⁵ Such elaborate sub-trees would make a mockery of the programme of reduction of lexical items to semantic primitives, is Chomsky's implied objection. This in itself does not seem a valid objection: there is no reason why semantic specifications of lexical items could not be very elaborate. What seems a more delicate problem is the question of what is asserted in a lexical item, and what is 'presupposed' in some sense of the term 'presupposition' yet to be defined. It will not do to say, as is suggested by G. Lakoff (1971, p. 263), that only 'cause to become dead' is semantic structure, and all the rest presupposition: there is no truth value gap when I use the term *murder* improperly. If there was only killing, but no malice aforethought, or if the killing was legally justified, then *Fred murdered Harry* is false, not uninterpretable. Yet, although it is far from clear how this problem could be solved, it should be recognized that it is entirely independent of the specific proposal put forward by McCawley: it exists for any descriptive theory of language, if the semantic specification of sentences and lexical items is to be as much part of the description of linguistic competence as the syntax or the phonology. De Rijk, quite properly, stresses this point at the beginning of his contribution to this book.⁶

VI

There are, however, specific objections against McCawley's analysis of *kill*, of which it would not be true to say that they would arise anyway in any linguistic description. They are to be found in Fodor's article (1970) 'Three Reasons for not Deriving "kill" from "cause to die"'. The first of these three arguments is, perhaps, less interesting than the other two. Fodor contrasts the following sentences:

- (a) Floyd melted the glass though it surprised me that it would do so.
- (b) John killed Mary and it surprised me that she did so.

⁵ McCawley, in a personal communication, remarked that the situation is actually worse: for *assassinate* to be properly used, the killing must also take place for reasons connected with the victim's public function. If, he says, someone, on coming home, finds his wife in bed with the President of the United States, and, more hurt than proud, goes and buys a gun with which he shoots the President while the latter furtively leaves the flat, then that may be murder, but it is not assassination. Further observations can be found in Gallagher (1970).

⁶ Dixon (1971) draws convincing evidence from the Australian aboriginal language Dyirbal for a distinction between 'nuclear' and 'non-nuclear' verbs. *Kill* would be nuclear, but not, e.g. *murder*. It is tempting to think that such a distinction might contribute to a solution of the problem discussed here.

the second of which he qualifies as ungrammatical, but the first as grammatical. If the causative analysis were correct, he then says, one would expect the *do-so* replacement to be similar in both causative *melt* and *kill*. Yet, he says, in the case of *melt* the *do-so* substitution can operate on the embedded intransitive *melt*, whereas it cannot on the embedded intransitive *die*. There is, however, sufficient uncertainty as to the data to make the argument uncoercive: sentence (a) seems as ungrammatical as (b).

Fodor's second and third arguments are reducible to one. In the expression *cause to die* both the verb *cause* and the verb *die* can be modified in their own right. Thus we can say:

(c) John caused Bill to die on Sunday by stabbing him on Saturday.

In this case, however, insertion of *kill* leads to a different meaning:

(d) John killed Bill on Sunday by stabbing him on Saturday.

(Fodor calls this sentence ungrammatical; it is, however, a perfectly good way of expressing in English the somehow contradictory proposition it expresses.) Likewise, the following sentence is ambiguous as to the subject of *shaving*:

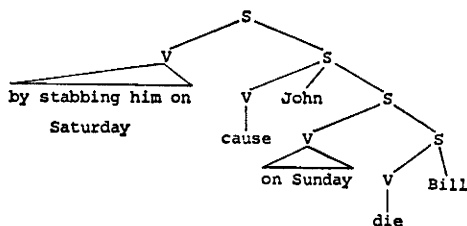
(e) John caused Bill to die while shaving.

If *kill* is inserted here, the ambiguity disappears:

(f) John killed Bill while shaving.

In terms of analyses currently presented in semantic syntax, one might be inclined to see an easy explanation for these observations. Temporal adverbials as well as instrumental *by*-phrases are considered derived from higher predicates (operators). Considering *by stabbing him on Saturday* a complex operator (whose internal structure does not concern us here), we can give the following analysis for (c):

(g)



We might then propose that the intervening operator *on Sunday* prevents the unification of 'cause to die' into one item *kill*. In the same way, (f) would imply that *while shaving* can only be the highest operator in the

semantic analysis of that sentence. Accordingly, a form of Equi-NP-Deletion can only have deleted the subject *John* from the *while*-phrase. (Fodor argues convincingly for this form of deletion.) Such a proposal would have some further justification in that a verb such as *reject*, which contains a negation element and is analysable as 'not accept', still does not allow a negatively quantified *any* under its own S:

(h) I didn't accept anything ever.

(i) *I rejected anything ever.

This might be thought to be so because in (h) there is an existential element between *not* and *accept*, in semantic analysis: 'not—there is a thing—I accepted it'. This element would then prevent the insertion of *reject*. Similar observations can be made with respect to many other verbs.

If we adopt this explanation, however, the question arises why *almost*, which must be considered an operator too in terms of semantic syntax, should be allowed to modify embedded sub-lexical verbs freely enough to give rise to the ambiguity observed by Morgan. Without going into these problems too deeply here, we might suggest that a possible answer could be found in a distinction between referring and non-referring expressions. We speak of a referring expression when it maps on to an element in a universe of interpretation. (Such elements may correspond to what, in a reasonably sound ontology, we take to be entities in the world, but may also be the product of 'internal' cognitive processes: fictional characters, or elements such as 'the average age of Londoners'.) Referring expressions are opposed to purely logical expressions (such as *not*, *or*, *and*, *almost*), and to intensional expressions, such as *fish* in *Fred wants to eat fish tonight*, or *catching fish is forbidden here*.

Let us now formulate the following constraint on the formation of lexical items:

No material can be made part of a lexical item when, thereby, it moves into or out of the scope of an operator which contains a referring expression.

This constraint solves the apparent contradiction between Fodor's and Morgan's observations, and the ungrammaticality of sentences of the type of (i).

VII

Another, puzzling but related problem is raised by de Rijk in paper II of this volume. He observes that the verb *forget*, in some of its senses, invites a semantic analysis 'no longer know', or 'no longer remember', at

least as a first step. If this is so, however, the following difficulty arises. The sentences *I no longer know the names of my students* and *I have forgotten the names of my students* show a curious semantic difference. The former is ambiguous in a way the latter is not. In the latter, reference is made to one set of names of one set of students: it is those which the speaker used to know but no longer knows. In the former, either this single reference is made, or there is multiple reference in roughly the following way: 'In the past, at any given time, I used to know the names of whoever were my students at that time, but nowadays I tend not to know the names of whoever are my students now.'

The problem is why *forget* (or *have forgotten*) should be related to the single reference reading only, and not to the reading involving multiple reference. G. Lakoff (1971, p. 272) maintains that an argument of lexical decomposition does not imply complete synonymy between the lexical item and its analysans, but only an 'inclusion of meaning': 'McCawley's conjecture . . . only requires that the meaning of *cease to know* be contained in the meaning of *forget*, which it is.' He proposes that 'it would be true to say that *forget* cannot be derived from an underlying structure containing only the meaning of *cease to know*, since *forget* means *to cease to know due to a change in the mental state of the subject*.' This analysis, however, seems all too facile, and *ad hoc*. It does not rule out the multiple reference reading for *forget*, since I may cease to know the names of my students (although I used to be able to retain their names in the old days) due to a change in my mental state: my memory is no longer what it used to be. Moreover, the addition of 'due to a change in the mental state of the subject' appears *ad hoc* in the light of parallel, and presumably related, phenomena. Thus, the same semantic difference as that observed by de Rijk occurs in *I am no longer satisfied with my teachers* and *I have become dissatisfied with my teachers*, or *I no longer have my watch with me* and *I have lost my watch*, or *Alcoholics are getting younger* and *Alcoholics are rejuvenating*.

De Rijk himself proposes, in an appendix to his paper, a solution suggested by Janet Dean-Fodor in correspondence. He traces the problem back to *wh*-complements, and proposes a constraint to the effect that a predicate allows for a *wh*-complement only if the next higher predicate is an existential quantifier. Apart from general difficulties arising from existential quantifiers for *wh*-complements, this proposal appears inadequate because the phenomenon occurs quite independently of *wh*-complements, as in the examples just quoted.

In a paper read at the Cambridge Colloquium on Formal Semantics (8-12 April 1973), 'Referential Constraints on Lexical Items', I propose a solution to this problem which, again, relies on the difference between

referring and non-referring expressions. Apart from the constraint given at the end of section VI, the following constraint is formulated:

If a referring expression *R* commands a node *A* in the semantic representation of a sentence, then any lexical island absorbing *A* must still be commanded by a constituent *R'* whose head is derived from *R* or from any expression making the same reference.

This constraint has a certain explanatory value not only for the cases mentioned so far in this section, but also for other observations. As regards de Rijk's observation, we can consider the sentence *I no longer know the names of my students* as being ambiguous in two ways which can be approximately rendered as follows:

- (a) It used to be the case that I knew *x* [*x* were the names of my then students] and, now, not I know *y* [*y* are the names of my now students].
- (b) It used to be the case that I knew *x* [*x* are/were the names of my now/sometime students] and, now, not I know *x*.

Assuming two kinds of pronominalization, i.e. pronouns of 'laziness' and pronouns for variables (see Hall Partee, 1973), both could be taken to reduce to:

- (c) I used to know the names of my students and now I do not know them.

or a suitably abstract form corresponding to this. (Notice that the definite pronoun *them* in (c) is ambiguous between single and multiple reference.) A further reduction could conceivably lead to:

- (d) I no longer know the names of my students.

But the lexical item *forget* (*have forgotten*) must be prevented from being inserted when the sentence derives from (a). This can be done by defining *forget* as a substitute for something like 'used to know and then not know'. The second *know* is commanded in (a) by the referring expression under the variable *y*: '*y* [*y* are the names of my now students]'. If this *know* is to be absorbed into a lexical island (a sub-tree dominated by a single category-node, in this case *V*) this island must still be commanded by a descendant of the referring expression under *y*. But this expression has been deleted, and its 'deletor', the expression '*x* [*x* were the names of my then students]', which does command *forget* after lexicalization, does not

make the same reference. Hence the impossibility of *forget* for (a).⁷ The parallel cases quoted above will be subject to similar analyses.

It also follows from the constraint formulated in this section that lexical items are free to contain incorporated intensional objects, such as 'beer' in *brew*, which is analysed as 'make beer', or *advise*, which is 'give advice', but never an object which contains a referring expression. There are no verbs which are to be analysed as, e.g. 'make the beer', where *the beer* is a referring expression. This follows from the constraint since, after incorporation into the verb, the referring expression would no longer command the lexical item, the new verb, of which it has become a part. Again, it is not possible to investigate these matters more fully here, taking into account the numerous problems which present themselves (especially in connection with proper names). Yet, it is worth pointing out that Morgan (1968) also suggests the impossibility of incorporating a referring expression into a lexical item.

VIII

On p. 85 of (1972a), Chomsky raises another criticism against semantic syntax. The noun *uncle*, he says, will have to be represented semantically in such a way that:

- (a) John's uncle
- (b) the person who is the brother of John's mother or father or the husband of the sister of John's mother or father
- and (c) the person who is the son of one of John's grandparents or the husband of a daughter of one of John's grandparents, but is not his father

have the same semantic representation, 'if the concept "semantic representation" ("reading") is to play any role at all in linguistic theory'. If, however, he continues, we insert (a), (b), or (c) in the context:

Bill realized that the bank robber was—

the resulting three sentences will have different meanings. On p. 86 he writes:

The basic point is that what one believes, realizes, etc., depends not only on the proposition expressed, but also on some aspects of the form in which it is expressed. In particular, then, people can perfectly well have contradictory beliefs, can correctly be said to fail to realize that *p* even though (in another sense)

⁷ Notice that the constraint works for lexical islands (sub-trees under a category-node) as well: the sentence *I used to know but no longer know now the names of my students* is interpretable only in the single reference reading, contrary to (c) and (d) above. *Used to know but no longer know now* will, in surface structure analysis, come under one single V-node.

they know that *p*, to be aware that *p* but be unaware that *q* where *p* and *q* are different expressions of the same proposition, etc. . . . Given these observations, let us return to the standard [= *Aspects*] and semantically based theories. In the standard theory, [(a), (b), and (c)] would derive from three different deep structures, all mapped on to the same semantic representation. To assign a different meaning to [the three resulting sentences] it is necessary to define *realize* (i.e. assign it intrinsic lexical semantic properties) in such a way that the meaning assigned to 'NP realizes that *p*' depends not only on the semantic interpretation of *p* but also on the deep structure of *p*. In the case in question, at least, there is no contradiction in this requirement, though it remains to meet it in an interesting way.

It would be useful if Chomsky had given at least some indication of how he envisages a linguistic description meeting the rather heavy demands he puts on semantic rules 'in an interesting way'. But it is perhaps more fruitful to point out that Chomsky is putting up problems here which are entirely of his own making and are in no way connected with the data to be explained. Chomsky's *uncle*-problem vanishes when two sources of confusion have been cleared away.

First, when it is true of a person that he is John's uncle, it follows that either he is the brother of John's mother, or of John's father, or the husband of the sister of John's mother or father, or the son of John's grandparents, etc. It is not unusual that the truth of a proposition entails a lengthy disjunction. There is, however, no reason to take it for granted that if a person realizes that *p*, he must also realize all the entailments of *p*. There is certainly no reason to require that this be made part of the semantic specification of *realize*. If this were done, then *to realize that p*, and *to realize that q, r, s, etc.* (where 'q', 'r', 's', etc., are entailments of *p*), would be synonymous expressions, which they are not. In short, Chomsky fails to distinguish between meaning and entailments.

Secondly, any theory of grammar requiring that (a), (b), and (c) should have the same semantic representation would be gravely inadequate. What could, conceivably, be proposed is that *uncle* should be inserted for something like 'parent's brother', or for something like 'parent's sister's husband', but not for anything like 'parents brother or parent's sister's husband'. In fact, it seems that there are no lexical items at all which contain an *or*-disjunction in their semantic analysis, although there are many which can stand either for one or for another semantic analysis (sub-tree). In these cases one speaks of lexical homonymy.

The word *uncle*, however, is not an example of lexical homonymy. It does not correspond to the way we understand English to say that if a person is John's father's brother, the sentence *He is John's uncle* is true on one reading but false on the others. Nor do we find it misleading or false to give as the number of John's uncles the sum total of all his parents'

brothers and brothers-in-law. The word *uncle* seems to correspond to a unified concept, to be defined in terms of the kinship relations system of Western society, perhaps as 'parent's lateral male relative of nearest degree'. A better example of lexical homonymy within kinship terminology is the Italian *nipote*, which, when masculine, means either 'grandson' or 'nephew' (when feminine, 'granddaughter' or 'niece'), or the Dutch *neef*, which is either 'cousin' or 'nephew'. In Italian, the sentence *Ecco mio nipote* ('Here is my grandson/nephew') is indeed clearly true on one reading and false on the other if it is true of the speaker's grandson (or nephew). It is also false, or at least seriously misleading, to give as the number of one's *nipoti* the total of one's grandchildren plus one's nephews and nieces. In order to account for the ambiguity of *nipote* in terms of primitive notions of kinship relations one might propose that it substitutes either for 'child's son' or for 'sibling's son', but not for 'son of child or sibling'. It would, accordingly, be quite inappropriate to assert that 'if the concept "semantic representation" ("reading") is to play any role at all in linguistic theory' the expression *il nipote di Carlo* must have the same semantic representation as, for example, *the person who is the son of Carlo's son or daughter or of Carlo's brother or sister*. Quite the opposite is true: if these two expressions were to be given the same semantic representation, under some definition of that term, then the notion of semantic representation would have no role to play in linguistic theory.⁸

This suffices to explain the fact that the three sentences resulting from the insertion of (a), (b), and (c), respectively, into the context *Bill realized that the bank robber was*— have different meanings. If (a) is inserted, then what Bill realized is that somebody was John's uncle. But if (b) or (c) are inserted, then what Bill realized is a disjunction. The difference is clear. Chomsky's problem with *uncle* exists only when one confuses meaning with entailments, and the language in which semantic specifications are given with the meta-language of the grammar.

IX

What appears from all this is the following. In semantic syntax two independent claims are made with regard to lexical insertion. First, it is claimed that the semantic analysis of lexical items, i.e. the way in which they are to be represented in SR's, implies at least a tree structure, or phrase-marker, of the type well known in syntax. Secondly, the mapping rules relating lexical items to their semantic analyses fulfil not only the

⁸ It is not unlikely that there is some universal constraint on the formation of lexical items which would prevent the taking together of *the person who is the son of X's son or daughter or of X's brother or sister* into one single lexical item. It seems a very unnatural analysis for a lexical item.

function of specifying possible meanings but also of relating surface structures to their underlying syntactic structures, and finally their SDS (syntactic deep structure). It is the latter claim which distinguishes semantic syntax from autonomous syntax.

The first claim seems reasonably well established, despite the difficulties which have come to light in connection with it. Rather than show the falsity of the claim, they show that our views are still highly incomplete. At any rate, no alternative of any generality or precision has been proposed so far. The allusions made by Chomsky and other writers in autonomous syntax to SR's are, on the whole, non-specific and non-committal ('assign it intrinsic lexical semantic properties'—Chomsky, 1972a, p. 86). The only example I know of a specific formulation of a semantic rule in terms of autonomous syntax is Jackendoff's 'Adverb Scope Rule', quoted from Jackendoff (1969b, p. 216) by G. Lakoff in 'Global Rules' (1970c, footnote 8; paper VI, p. 154 in this volume). Lakoff correctly remarks that this rule implies that SR's must be represented in terms of syntactic phrase-markers, which is precisely what is claimed in semantic syntax. The issue will be taken up again later.

The second claim is denied by autonomous syntax. It is denied that the replacement of sub-trees by lexical items is a syntactic process, or mapping. More specifically, autonomous syntax denies that, at least as far as the syntax is concerned (i.e. the theory accounting for data of wellformedness), there is any relation between lexical items and sub-trees, be it at deep structure level or anywhere later in the derivation. According to it, lexical items are selected at the end of the base, within limits set by accumulations of features of various kinds, which have been accumulated as a result of certain base rules. Although the two claims put forward by semantic syntax are independent of each other, Chomsky's criticisms do not, on the whole, distinguish between them: they are directed at both indiscriminately. Likewise, in semantic syntax writings, the two claims have not been distinguished, generally speaking, with sufficient clarity. The discussions on both sides have suffered from a certain degree of conceptual nebulosity.

As we have seen above, the claim that lexical semantic mapping is also syntactic is made more specific by the added assertion that it is not necessarily deep structure (i.e. SR) sub-trees which are replaced by lexical items, but also, or mainly, transformed trees. That is, lexical insertion alternates with other transformational rules, and in a non-unitary way. To assume that there is a level in syntax before which all lexical insertion rules have been operative and after which other transformations apply, would not *per se* be incompatible with semantic syntax. But this assumption is not made since no evidence in support of it has come to light.

As regards the way in which items are inserted into trees, views have

differed among writers in semantic syntax. As we have seen above, Gruber (1967c) proposes (mainly) pretransformational polycategorical lexical insertion: although the lexical item itself will be one single constituent in the derived tree, it is not considered necessary that the sub-tree replaced by it should also be one single constituent. A lexical item may be defined so as to correspond to a Boolean structural analysis of trees. Contrary to this view, McCawley proposes non-unitary monocategorical lexical attachment, whereby lexical items can only substitute for sub-trees dominated by a single node (lexical islands), usually created transformationally. The latter view is now generally adhered to.

The arguments in favour of the second, syntactic, claim of semantic syntax have not been nearly as convincing as those for the first claim. It has proved difficult to muster sufficient evidence for the bits of transformational syntax believed to be hidden in lexical items, especially verbs. Evidence abounds for mappings of lexical items on semantically analytic tree structures. But to show that these mappings are part of syntax pure and proper is not so simple a matter.

In spite of the lack of sufficient evidence, McCawley has boldly proposed a fairly detailed theory of semantic-syntactic deep structure, and a rule for uniting diverse parts of semantic-syntactic trees under one single node, so that lexical substitution can take place. His deep structure theory is expounded in paper III of this collection, 'English as a VSO Language'. On the evidence that a number of transformational rules are simplified by that hypothesis, he proposes that the deep structure of English and many other languages is not NP-VP, but V-NP-NP (Verb-Subject-Object). Languages which are not VSO are SOV (such as Japanese). One prelexical rule enabling lexical substitution to take place is Predicate Raising, explained in papers I, II, and III. This rule is one of those whose formulation would be complicated considerably if deep structure were to contain a VP-node, as in the traditional NP-VP analysis.

Despite their obvious speculative character, McCawley's proposals are seen to appeal to large numbers of linguists. It is important, however, that one should not allow one's critical senses to be dulled by what appears to be an eminently appealing theory. The ring of truth is no guarantee of truth. Chomsky points out (1972a, pp. 151-2):

The first question to be asked is whether these [i.e. prelexical] transformations are syntactically motivated. Predicate Raising surely is not; it is simply a device to convert phrases that are to be replaced by a lexical item into a single constituent.

I believe it is fair to admit that, indeed, nowhere near sufficient evidence has been brought to light from English to support Predicate Raising as a rule of syntax. One should, however, also give full weight to considerations

of universal linguistic theory. The fact that there are non-trivial, highly specific constraints on the form of possible grammars entails that the grammar of a language is severely under-determined by the data obtainable from that language. This is true both for the language-learning infant and for the linguist attempting a description of the language. The inductive process is only partially dependent on the data collected from the language in question. Many inductive choices will be determined by considerations of universal linguistic theory, or, for the child, by an innate set of specific expectations concerning human language. One must, therefore, reckon with the possibility that Predicate Raising is a valid rule of English (prelexical) grammar, even though English itself does not yield sufficient evidence to support that view. In that case, the evidence will have to come from other languages.

Such evidence is indeed available. French, and a number of other languages, have a causative construction with *faire* (make, do), which clearly involves Predicate Raising in ordinary, postlexical syntax. This is not the place to expound the relevant arguments.⁹ But it may be noted here that in French and many other languages, datives arise in the *faire*-construction: the subject of an embedded transitive, non-passivized S becomes a dative in surface structure, as in *Je ferai voir la lettre à Jean* (I shall make/let Jean see the letter). This is derived from an underlying *Je faire [Jean voir la lettre]*. If the embedded S is intransitive, or passivized, the (derived) subject becomes accusative in surface structure. In the *faire*-construction the dative is clearly of syntactic origin: it is not an original 'case' in deep structure. There is, however, another sentence in French, which is entirely synonymous with the one quoted above: *Je montrerai la lettre à Jean*. In semantic syntax one would say that 'faire voir' (make-see) has been replaced by the lexical item *montrer* (show), after Predicate Raising has applied. The datives would thus be of the same origin in both sentences. In terms of the *Aspects*-theory, however, one would be forced to maintain that, although the dative under *faire voir* is the result of a transformational process, the one under *montrer* occupies a selectional 'slot' defined in the lexicon for that verb. Since the situation is the same for a large number of verbs which take the dative and are semantically decomposable into 'cause to . . .' or 'allow to . . .', the *Aspects*-theory clearly involves a loss of generality in syntactic description.¹⁰

⁹ See Seuren (1972b, unpublished) and R. Kayne (1969). Kayne's treatment of the *faire*-construction is equivalent to Predicate Raising in terms of NP-VP as a deep structure schema.

¹⁰ In French, not only *faire*, but also *laisser* (allow, let) takes the *faire*-construction which yields datives, although the latter does so only optionally. It is noteworthy that these two verbs correlate logically in that 'cause not to . . .' is equivalent with 'not allow to . . .'.

There is, moreover, the fact that certain French, or English, verbs which take an optional object and an optional indirect object, have their indirect object in the dative only when there is also an object: if the latter is missing, the constituent which would otherwise occur in the dative then occurs in the accusative. Thus, French has *servir le potage au client*, *servir le potage*, *servir le client* (serve the soup to the client, serve the soup, serve the client), or *inspirer du courage aux troupes*, *inspirer du courage*, *inspirer les troupes* (inspire courage (in)to the troops, inspire courage, inspire the troops). This would again indicate that it is not an inherent feature of these verbs that they take objects and/or indirect objects, but, rather, that their objects and indirect objects are the regular results of prelexical Predicate Raising.

It is clear that any syntactic evidence for Predicate Raising and subsequent lexical insertion will provide support not only for the particular theory proposed by McCawley, but also for the wider and more general theory of semantic syntax which McCawley's theory presupposes.

X

The two main theses proposed in semantic syntax with regard to lexical insertion—that lexical items correspond to semantic analyses which have the shape of, or at least imply, syntactic tree-structures, and that this mapping is part of syntax—are, in fact, specific forms of two corresponding, but more general points of theory. That is, semantic syntax is defined by the two claims that all semantic analyses are to be given in the shape of, or at least are to imply, tree-structures consisting of predicates and arguments, and that semantic mappings coincide with those of syntax.

That there must be some underlying semantic form for sentences is an idea which has, in some form or other, lived for centuries ('innere Form'). And although the terminology may be new, the thought that sentences are mappings of these hypothetical semantic structures is natural and unsurprising to anyone brought up in a Western tradition. It was, however, never clearly envisaged until very recently that underlying structures, and, ultimately, a deep structure, had to be posited for reasons of *syntactic* description and explanation. Due to an emphasis on formal descriptions in twentieth-century linguistics, and to the recent developments in transformational grammar, the insight that every sentence has a *syntactic* deep structure is now firmly established. It was inevitable that the purely empirical question should arise of whether the abstract object posited by syntactic theory, SDS, is different from or the same as the abstract object posited for reasons of semantic clarification. The question becomes acute when one sees that both kinds of abstract object are of essentially the same nature: both are constituent structure analyses, or trees.

If SDS is identical with SR, it follows that semantic and syntactic mappings are also identical. For us to maintain this it must be shown that all rules posited for purely syntactic reasons are also necessary for semantic reasons, and vice versa. There must be both syntactic and semantic evidence for all rules which are posited in the grammar. It is not, as appears to be often thought, an *a priori* matter of logical necessity that an explanation of wellformedness in terms of transformations and an explanation of possible meanings should amount to one single theory. The matter is a purely empirical one.

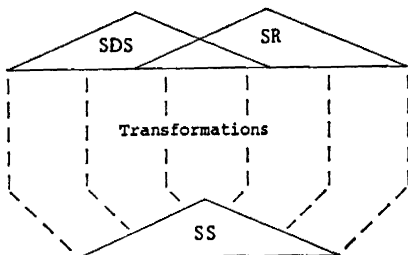
As regards lexical insertions, Chomsky's position has not changed essentially since *Aspects*, although he has changed his terminology in an attempt to bring out whatever similarities there are between the *Aspects*-theory and semantic syntax. The lexicalist hypothesis, of 1967, was less a departure from the *Aspects*-theory than an attempt at refining it. With regard to the wider issues of semantic syntax, however, Chomsky has revised his position. Referring to the *Aspects*-theory as the 'standard theory', he proposes a revision of that theory, which he names the 'extended standard theory' (EST). He distinguishes three questions (1972a, p. 130). Question (A) relates to the nature of lexical items and lexical insertion; question (B) to the nature of SR's and of the relation between syntactic structures and SR's; question (C) bears on the existence of input and output conditions (which does not concern us here). On p. 134 he then says:

EST retains the assumptions of the standard theory with regard to (A) and (C), but modifies the answers proposed for (B), as follows: semantic interpretation is held to be determined by the pair (deep structure, surface structure) of Σ [i.e. a transformational derivation: sequence of phrase markers], rather than by the deep structure alone; further, it is proposed that insofar as grammatical relations play a role in determining meaning, it is the grammatical relations of the deep structure that are relevant (as before), but that such matters as scope of 'logical elements' and quantifiers, coreference, focus and certain kinds of pre-supposition, and certain other properties, are determined by rules that take surface structure (more precisely, phonetically interpreted surface structure) into account.

What appears from this is that semantic representations of sentences are held to coincide with SDS's only in certain aspects, in particular as regards grammatical relations (subject, object, indirect object, main verb, etc.). Other aspects of SDS, such as the scope of operators, focus, and pre-supposition, are left unspecified in SDS, and are to be inferred from surface structure.¹¹ One must, consequently, distinguish between those trans-

¹¹ Chomsky is inconsistent with respect to the notion 'surface structure' as a determining factor for certain aspects of meaning. In the quotation given above he says: 'more precisely, phonetically interpreted surface structure'. On p. 196 and elsewhere, he speaks of 'surface (or shallow) structure'.

formations which are common to the syntax and the grammatical semantics, and which must be meaning-invariant, and those which are not shared by the semantics but are merely syntactic and will often have a semantic effect (change the meaning of the structure or specify meaning where it was unspecified before). Moreover, there must be, in EST, semantic transformations which are irrelevant for the syntax. The situation can be graphically represented as follows:



As regards the specification of the scope of operators, it is clear that there is, in most cases, some correspondence between their left-to-right order in surface structure and their scope. In semantic syntax an attempt is made (see the papers VI and VII in this volume, by G. Lakoff) to express this correspondence by means of global, or derivational, constraints: an operator's position of command in SR must be translated in terms of its left-to-right position in surface structure.¹² In autonomous syntax (EST) it is said that this correspondence will have to be expressed by surface structure interpretation rules (although no specific proposals for such rules have been put forward). In both cases we have to do with derivationally constrained transformational processes.

We can, therefore, in a sense, understand Chomsky when he writes (1972a, p. 194): 'We can use the term "derivational constraint" to refer to the rules that relate the surface structure to the semantic representation, but this is merely a terminological point' but not when he goes on: '—and a misleading one, if it serves as an avenue for the introduction of much richer descriptive devices into linguistic theory.'

Chomsky criticizes Lakoff, and semantic syntax, in part for offering little more than a notational variant for EST, in part for introducing

¹² This is a simplified account of G. Lakoff's constraint. In paper IX it is argued, moreover, that left-to-right ordering of operators does not correspond to SR-positions of operators, but to the order in which they have been lowered in the cycle.

uncalled-for, over-complex, and *ad hoc* machinery into linguistic theory. On p. 192 we read:

Summarizing, a consideration of the properties of quantifiers and negations seems to support the view that syntactically motivated rules and the structures they involve suffice to account for whatever facts are clear, in a fairly natural way, and that the introduction of more abstract underlying structures and transformations with no syntactic motivation (such as quantifier-lowering) simply leads to new problems and complexity, while requiring that linguistic theory be enriched to incorporate a broader class of rules (derivational constraints). . . . In the cases so far discussed, however, and others to which I will turn directly, postulation of more abstract structures to express semantic content and rules designed *ad hoc* to incorporate these more abstract structures in derivations offers at best a notational alternative, and at worst, leads to new complications.

Against this, it must be pointed out that a criticism of complexity and undue enrichment of the theory is void when it is not backed up by any specific counterproposal. Chomsky seems to imply that rules of semantic interpretation fall outside the domain of linguistic theory. He does not, at any rate, show that less complex structures or rules would be involved in an autonomous-syntax-type treatment of the semantic properties of operators in sentences. Autonomous syntax has remained almost totally unspecific with respect to the area of linguistic description which it calls 'semantic interpretation'. In semantic syntax, on the contrary, serious attempts have been made to formulate semantic analyses, as well as the mappings relating them to surface structures, in some consistent and precise way.

The central issue, however, between autonomous and semantic syntax, is not the question of unitary lexical insertion, or of prelexical syntax, or of derivational constraints: these are independent empirical issues. It is the question of the identity or non-identity of SDS and SR, and therefore of syntactic and semantic mappings. This issue is often mentioned by Chomsky, but hardly ever explicitly gone into. He is thus led to say (1972a, p. 136):

The clearest and probably most important difference between EST and generative semantics has to do with the ordering of lexical and nonlexical transformations. This, in fact, seems to me perhaps the only fairly clear issue with empirical import that distinguishes these theories.

Quite the opposite seems true. Although the question of the ordering of the lexical and non-lexical transformations has figured prominently in the discussions, it is not conceptually central.¹³ The question is relevant to the extent that if the lexical position taken by those who defend semantic

¹³ In fact, Gruber's position as regards the ordering of lexical and non-lexical rules does not essentially differ from Chomsky's, as we have noted above. Yet it would be absurd to regard Gruber as a representative of EST.

syntax turns out to be correct, the central claim of semantic syntax will be reinforced considerably. The same holds for other particular proposals made in the context of semantic syntax, such as the VSO hypothesis, Quantifier Lowering, Negative Raising, the assumption of variables in deep structure, etc. Syntactic confirmation of these proposals will vindicate the theory of semantic syntax to the extent that they bring semantic analysis and syntactic deep structure closer together.

So much for the second claim made by semantic syntax. The first claim—that semantic specifications imply, as their central element, a tree-structure displaying a particular constituent analysis, so that SR's are themselves syntactic objects—is treated somewhat lightheartedly by Chomsky. He tries, in effect, to reduce it to triviality (1972a, p. 196):

The semantic characterization of lexical items and the structures in which they appear can be given in terms of phrase-markers and transformations, for the uninteresting reason that virtually anything intelligible can be presented in these terms.

I would be inclined to say that anything intelligible has to have a representation in these terms, which, if true, is far from uninteresting.

Chomsky's thoughts on this subject seem to vary considerably from one moment to another, sometimes even within the space of half a page. After the quotation from p. 196 given above, he continues:

This is of course a weakness, not a strength of this mode of expression, to be overcome, one hopes, as more insight is gained into the detailed structure of the lexicon. In contrast, generative semantics maintains that lexical items replace phrase-markers that express their meaning. This is an attractive idea, and if it were tenable, there would be good reason to take it very seriously as one approach to the description of meaning.

In 1967, when writing his 'Remarks on Nominalizations', Chomsky apparently did not think that the claim that anything intelligible can be presented in terms of phrase-markers was uninteresting or unempirical. Let us remember that the transformationalist position implies the substitution of derived nominals for a transformed sub-tree, whereas the lexicalist position seems to imply a substitution of derived nominals for sets of features. In this context he says (1972a, p. 17):

In the earliest work on transformational grammar (cf. Lees (1960)), the correctness of the transformationalist position was taken for granted; and, in fact, there was really no alternative as the theory of grammar was formulated at that time. However, the extension of grammatical theory to incorporate syntactic features (as in Chomsky (1965, Chapter 2)) permits a formulation of the lexicalist position, and therefore raises the issue of choice between the alternatives.

In fact, it makes a great deal of empirical difference whether mapping relations are established between surface structure elements and phrase-markers (or trees), posited for reasons of syntactic or semantic explanation,

or between surface structure elements and other kinds of hypothetical structures, such as sets of unordered features. (From the point of view of the empiricalness of this difference it is, of course, irrelevant whether the mappings in question are syntactic or semantic or both.) The difference becomes crucial when specific proposals are made with regard to the precise underlying structure and the mapping relating it to surface structure.

One interesting specific proposal was made by C. L. Baker in an article published in *Foundations of Language* in 1970, and reprinted here as paper V. It concerns the precise status of the question morpheme in underlying structures. Baker concludes, on both syntactic and semantic grounds, that there must be an abstract question morpheme, that its precise position in deep structure is crucial for both the form and the meaning of sentences containing a question (direct or indirect), and, finally, that for specific questions ('*wh*-questions') the question morpheme acts as a variable-binding operator. This last point he illustrates with the example *Who remembers where we bought which book?* This sentence is ambiguous as to the content of the question, since two types of answer are possible. *John and Martha remember where we bought which book*, is a possible answer, but also: *John remembers where we bought the physics book and Martha and Ted remember where we bought 'The Wizard of Oz'*. Baker explains convincingly how this ambiguity can be related to positional differences of variable-binding question morphemes in underlying structures. This paper opens very interesting perspectives in a number of directions. Not only does it seem crucial for any theory of sentential complementation, but the variable-binding property of the *wh*-morpheme also calls for a comparison with other operators of that type, such as quantifiers. In fact, the *wh*-morpheme seems to interact in interesting ways with quantifiers, as appears, e.g. from the fact that *How many of you remember where we bought which book?* is not ambiguous in the way Baker's sentence is. We cannot, however, go into a discussion of these matters here. What concerns us here is that Baker's proposals are not vacuous, unempirical, or uninteresting, but quite the opposite. And they depend crucially on the notion of phrase-marker to indicate, e.g. the scope of the *wh*-morpheme. Unordered feature sets cannot express this. It is part of the empirical impact of Baker's contribution that, if it is correct, it reinforces the position of semantic syntax.

The same is true for the papers VIII and IX, which deal with Negative Raising. Both papers aim at showing that this rule is part of syntax. Paper VIII contains an elegant demonstration that it is a cyclic rule, with the most interesting corollary that SDS contains performative verbs. In paper IX it is argued that Negative Raising applies also with *until*, *all*, *and*, and the necessity operator as the controlling elements. It follows from the

argument that what is reflected by the left-to-right order of surface elements corresponding to logical operators is not so much their scope in SR as the order in which they have been lowered. Quantifier Lowering, as well as G. Lakoff's global constraint on this rule, are thus seen to be insolubly linked up with syntax in the strictest sense of the term. In view of the evidence available to date, it seems a promising, though also daring, hypothesis to say that this holds for all rules of grammatical semantics.