

# IV

## AUTONOMOUS VERSUS SEMANTIC SYNTAX<sup>1</sup>

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It is a widely accepted view nowadays that a grammar must give an explicit account of a native speaker's knowledge of his language, his implicit competence. Anyone who knows his language knows, in some implicit, intuitive way, at least what a proper sentence in his language should sound like. He is able to separate proper from improper pronunciations. He also knows, in some largely unknown sense, what the sentences of his language mean, or how to interpret them. He is able to separate possible from impossible interpretations.

How to elicit information from native speakers about their tacit knowledge of the language they speak is a different problem that I shall not go into here. Nor do I intend to discuss the well-known difficulty of doubtful or unclear evidence. Let it suffice here to point out that a grammar, in order to be an adequate account of the native speaker's knowledge of his language, will have to specify for each sentence at least what its proper pronunciations and what its possible interpretations are. Accordingly, the descriptive linguist will have to specify for each sentence a *phonetic representation* (PR), containing instructions for proper pronunciation, and a *semantic representation* (SR), containing instructions for proper interpretation. A grammar thus establishes in some way a mapping relation for each sentence of the language between the set of its possible pronunciations and the set of its possible interpretations.

It should be noted that we cannot expect a grammar to specify which pronunciation or interpretation should be selected for each specific occasion on which a sentence is used. It does not seem to be linguistic knowledge which tells us, speakers of the language, when to speak angrily, or with surprise, joy, or the like. Some variations of speech carry a dialectal

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or sociolinguistic mark. In these cases the selection of the proper form for the proper occasion depends on one's knowledge of the correlations between certain possible alternative linguistic forms on the one hand, and some social classification on the other. It is a matter of debate whether one can say of a man that he possesses complete knowledge of a language if he does not know the social correlates of its varieties. Usually it is assumed that varieties with social or local correlates are part of a different system or sub system, whereas varieties within the same dialect and social register are part of one homogeneous system. But it is not entirely clear what this means or what such a distinction could be based on. We shall not be concerned with such problems here, however.

As for appropriate interpretations, their actual selection, in every situation, from those possible for every sentence is clearly not a matter of linguistic knowledge, but rather of one's knowledge of the world, one's understanding of the particular issue that is being discussed, one's general sense of proportion, etc. In fact, a case can be made for saying that a man who is able to select, in actual linguistic communication, interpretations which are 'wrong', or not intended by the speaker, although still within the limits imposed by the grammar, betrays a very high degree of competence in his language indeed. We take it, therefore, that a grammar will have to specify at least the *possible* pronunciations and interpretations for each sentence of the language. But it will not provide instructions for *actual* selection of either pronunciations or interpretations in communication.

Clearly, since the notion 'possible sentence of a language' is not given in advance, a grammar will not be able to specify possible interpretations or pronunciations for sentences if it does not also specify the set of possible sentences. This has, in fact, been the central concern of Transformational Grammar ever since its inception: a grammar was seen as a device to separate grammatical from ungrammatical strings in the language.<sup>2</sup>

It has been assumed widely and for a long time that grammaticality and proper pronunciation must be kept separate in the sense that there are different sets of rules, or explanatory principles, accounting for them. This assumption has never been proved wrong, and there is a great deal of *prima facie* evidence for it. It is possible to say of an utterance that it is pronounced correctly but that its syntax is wrong, or vice versa. It is possible to know a language extremely well and use it with great subtlety and effect without being able to discriminate at all well between good and bad pronunciations. Accordingly, every sentence is assumed to have a syntactic structure which, though related in several ways with its PR, is, in fact, quite distinct from it. I take it for granted that this distinction is

<sup>2</sup> Chomsky (1957, p. 13).

correct. That is, I shall assume that the grammar of a language will contain a syntax, specifying possible syntactic structures, but not containing phonological rules. The grammar will also contain a phonology, mapping syntactic structures on to phonetic representations. If this assumption is, wholly or in part, incorrect the present argument will remain unaffected.

The present argument is about the relation between the syntactic structure and the semantic representation of a sentence, a problem which has become more and more central in theoretical linguistic discussions over the past few years. There is no clear *prima facie* evidence that the two must be kept separate in the sense indicated above, as there is for syntactic structure and PR. It is not clear that one can say of a sentence that its syntax is good but its meaning is in any sense 'wrong', although it does seem possible to say that it is clear what a sentence means or should mean though its syntax is wrong.<sup>3</sup> Whereas there is a sense in which one can say that a man knows English although his phonology is poor, there is no sense in which one can say that a man knows English if he constructs perfect sentences but does not know what they mean. *Prima facie*, therefore, one is inclined to expect that the relation between syntax and phonology is different from that between syntax and semantics. It is the latter that will be the subject of investigation in the present paper.

## II

The descriptive linguist has to rely on data that is the direct result of native speakers' attitudes of acceptance or non-acceptance with regard to the sentences of their language. In most cases the linguist still has to work with explicit judgements made by native speakers about sentences or utterances. But often this kind of evidence is unclear or unreliable: one is often unable to say explicitly what one's attitudes of acceptability are with respect to a particular sentence. Not only does a native speaker have no explicit knowledge of the grammar of his language (a point that hardly needs stressing), he is also only partially aware of his own acceptance or rejection of certain sentences, their pronunciation or interpretation. Better methods for bringing out the real attitudes of native speakers than those available at present would be most welcome, but not much progress has been made yet. On the whole, the status of evidence in descriptive linguistics is still rather problematic.

Yet it seems that we are justified in drawing the following major distinction among types of linguistic evidence. On the one hand we have data of wellformedness, on the other there is semantic data. The former can be subdivided into phonetic and syntactic data. Phonetic data is provided by

<sup>3</sup> See, for example, Katz (1964), and Ziff (1964).

judgements about the acceptability of certain pronunciations for a sentence. Syntactic data consists of judgements about the correctness of sentences. Judgements concerning possible interpretations for given sentences constitute semantic data. Although all three categories of data have been widely used in descriptive and theoretical work of the past fifteen years, there has been considerable unclarity about the status of and the exact distinction between syntactic and semantic data. It seems useful, therefore, to have a closer look at these two kinds of linguistic facts.

Syntactic evidence, or evidence about syntactic wellformedness, is obtained by eliciting answers to questions of the type: can one say such and such in the language, no matter what meaning would result or how absurd a thing it would be to say? Thus the sentence:

- (1) Every morning I eat the chocolate bar that I showed you yesterday.

will be considered a perfectly well-formed sentence of English, although it can only be true in a world where the same chocolate bar can be eaten more than once. The fact that such a world is different from the particular world in which we happen to live is irrelevant for linguistic description. We do not have to devise or learn a new language in order to speak about a world which is not ours. We take a natural language to be independent of the world in which it is spoken. Likewise, we take the sentence:

- (2) John is taller than he is.

to be fully grammatical, although it can never be true in any world at all since it is contradictory: it is possible to express contradictions in perfect English (see Note 5). In general, as was pointed out by McCawley,<sup>4</sup> so-called phenomena of deviance (syntactically correct co-occurrence of elements that seem incongruous in the light of our world) fall outside the domain of grammar. Whether or not one succeeds in visualizing a situation corresponding to, e.g.:

- (3) Every morning I eat some truth for breakfast.

depends more on one's forces of imagination than on one's knowledge of the language.

An alternative way of collecting data of syntactic wellformedness is to ask: suppose I want to say in good English what is also said by the following expression (English or some other language), can I then say such and such? If the answer is 'yes', the proposed synonym or translation is a grammatical sentence. If the answer is 'no', then the proposed formulation is either ungrammatical or grammatical but with a different meaning. In

<sup>4</sup> McCawley (1968a, p. 129).

general, if a sentence is never judged to be acceptable for the proper expression of any thought in any world at all, it will be marked as ungrammatical. (An asterisk is commonly used for this purpose, and it would be preferable if it were only used for this purpose, and not for deviant or contradictory sentences, or for sentences that are 'ungrammatical on a particular reading'.)

Semantic evidence consists in judgements about possible interpretations, or 'readings', for sentences. Most sentences are ambiguous in a number of, often unexpected, ways. Here again, we do not rule out an interpretation because it requires an impossible world, as with (2) above, or a world which is not ours. Thus:

(4) Every year at Christmas I kill my neighbour.

is taken to be ambiguous in at least two ways. It either says that every year at Christmas I kill the person who happens to be my neighbour at that time, or it says that every year at Christmas I kill the same person, who is my neighbour. In the former interpretation *neighbour* is a variable, in the latter it is a name. The fact that in the latter interpretation the sentence says something that can never be true in our world does not make the interpretation impossible. The possibility of particular interpretations is defined by certain rules of the language, not by the limitations of our world.

Interpretation implies the knowledge of truth conditions. In the actual, immediate understanding of sentences the knowledge of truth conditions is, in some sense, implicit: when we understand a sentence in one of its possible interpretations we know at least under what conditions it will be true, although we are usually unable to formulate these conditions explicitly. An explicit interpretation of a sentence will contain at least a precise formulation, in terms of primitive categories of knowledge, of what must be the case for the sentence (proposition) to be true. If a sentence is contradictory, there will be a contradiction in its truth conditions: something must and must not be the case for the sentence to be true.

The formulation of truth conditions, however, cannot give the complete interpretation of a sentence: there must be more to it. Clearly, if a sentence has two different sets of truth conditions, it is ambiguous in two ways, or has two distinct meanings. For then one can construct a case where all conditions of one set are satisfied, but not of the other. In that case the sentence is true on one reading, but false on the other. If the sentence had only one reading, it would be both true and false on the same reading, which would be logically disastrous.<sup>5</sup> Thus a difference in truth conditions

<sup>5</sup> Compare Quine (1960, p. 129): 'An ambiguous term, such as "light", may be at once clearly true of various objects (such as dark feathers) and clearly false of them.' There is a problem here, however, with sentences that we feel are ambiguous, but each

is sufficient for there to be a difference in meaning. It does not seem to be necessary, however. There are cases where we have a clear intuition of semantic difference, although the truth conditions are identical. Consider the sentences:

(5) I saw him this morning, the bastard.

(6) I saw him this morning.

The addition of *the bastard* in (5) is obviously not semantically indifferent. Yet there seems to be no difference in truth conditions between the two sentences. Then, of course, there is the problem of questions, imperatives, and other non-assertive sentence-types, which have no truth value and therefore no truth conditions. We can speak of the truth conditions of the proposition or propositions which can be semantically isolated from every sentence, but the remaining performative part has no truth conditions, although it plays an essential semantic role. It seems that, in general, where the formulation of truth conditions is insufficient for semantic interpretation, we have to do with performatives in some form or other, or with a focus-comment distinction. Thus, (5) can be analysed semantically as 'I saw him this morning, and I hereby call him a bastard.' What the semantic interpretation of performatives exactly consists of, is far from clear. But this unclarity need not hold us up here. Nor can we go into problems of focus and comment.

The actual task of formulating truth conditions in terms of primitive categories of knowledge is not usually undertaken by linguists: philosophers are generally more concerned with it. In much the same way, phonetic 'interpretation', or the formulation of physical sound conditions (together with, possibly, actual sound synthesis), is usually left to engineers

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of whose readings is contradictory, such as: *He wasn't alive for the rest of his life* (either: 'for the rest of his life he was dead' or: 'it is not true that for the rest of his life he was alive'). It might seem that here we cannot 'construct a case where all conditions of one set are satisfied', since the truth conditions associated with each reading are an incompatible set. We face the same problem with doubly analytical sentences, such as: *He wasn't dead for the rest of his life* (either: 'for the rest of his life he was alive' or: 'it is not true that for the rest of his life he was dead'). Here we cannot construct a case, it might seem, where not all conditions (barring presuppositions) are satisfied for either reading. Yet, it is a fact of English that the two sentences quoted above are ambiguous in the way indicated, i.e. in the scope of the operators *not* and *for the rest of his life*, and that their truth conditions differ accordingly. But one cannot construct 'cases', or 'worlds', in a straightforward way. One can, however, counterfactually suppose that all conditions of a contradictory set *s* have been fulfilled: 'if all conditions of *s* were satisfied, then the corresponding sentences would be true on that reading'. This amounts to the suggestion made by Kripke (see Hughes and Cresswell (1968, pp. 274-5); they refer to Kripke (1965); I am indebted to Mrs. E. Barth of the University of Amsterdam for the reference) 'that there might be some "worlds" in which every proposition without exception—even one of the form  $p \sim p$ —is possible. Kripke calls such worlds, *non-normal* worlds.'

and phoneticians. The descriptive linguist tends to limit himself to presenting PR's and SR's, that is, providing instructions for pronunciation and semantic interpretation respectively. A semantic representation of a sentence will be a perfectly disambiguated reformulation of the sentence providing precise clues for the formulation of truth conditions. In what 'language' this can be done is still a matter of considerable controversy and unclarity. In particular, it is still almost totally unknown in what terms actual lexical items of the language under description will have to be analysed in SR's. More is known about logical properties of sentences. It seems that, in order to disambiguate, a distinction must be made in SR's between scope-bearing elements, or operators, and a lexical 'nucleus', where the functional relations between verb, subject, object, etc. are expressed. Thus, the sentence:

(7) John doesn't know two languages.

is a clear instance of scope ambiguity. The sentence can be disambiguated, as is well-known from logic, by applying quantificational analysis. (7) can be represented in either of two ways:

- a. Neg— $\exists$ 2 languages—John knows the languages
- b.  $\exists$ 2 languages—Neg—John knows the languages.

Any SR of (7) will have to contain at least the analysis given in either (a) or (b). In general, a SR will have to contain at least the 'logical form' of the sentence it represents. By 'logical form' is meant the analysis of the sentence which is necessary for the construction of a formal logical argument. No alternative is known to logical form for an adequate, unambiguous formulation of truth conditions. Logical form provides the basis for both semantic interpretation and formal argument. It is now generally agreed that the SR of a sentence will be a synonymous expression of that sentence, with the additional property of providing a complete logical analysis and disambiguation. As a statement of principle this will suffice for the moment: further problems do not seem to affect the issue that will be discussed below.

### III

We thus have three categories of linguistic data: phonetic, syntactic, and semantic. A grammar of a language will have to make all and only the correct predictions about proper pronunciation, syntactic wellformedness, and proper interpretation. It does not follow from this that therefore a grammar will contain three different sets of rules, each making the correct predictions about one of the three categories of data, although this may

eventually turn out to be the case. It does follow, however, that there must be a set of rules making the correct predictions about syntactic wellformedness. There must be a syntax, even though there may not be a separate phonology or semantics, since it makes no sense to define proper pronunciation or proper interpretation if the notion 'possible sentence of the language' has not been clearly specified. Clearly, only syntactic observations, i.e. observations of facts of syntactic wellformedness, are valid evidence for proposed theories concerning structures and rules of syntax.

Most work of the past fifteen years has been devoted to syntax. But not always has reliance on non-syntactic data been avoided. In particular, data of ambiguity has often been used in support or criticism of particular syntactic proposals, notably in Chomsky's *Syntactic Structures* (1957), but also in the work of McCawley, Ross, G. Lakoff, Seuren. To use semantic evidence in support of syntactic theories is to make a specific claim of an empirical character about the internal organization of a grammar. As long as this claim has not been verified to a reasonable degree of certainty, one is not justified in building upon it.

In spite of this methodological impurity in Chomsky's early work, however, the theory of Transformational Grammar which he proposed is more than sufficiently supported by purely syntactic evidence. There is abundant syntactic evidence to support the theory that for every sentence of a language there is a finite number of syntactic structures,  $P_1, \dots, P_n$ , related by transformational rules such that each rule transforms  $P_i$  into  $P_{i+1}$  where  $1 \leq i < n$ .  $P_1$  is called the ultimate underlying structure, or deep structure;  $P_n$  is called the surface structure (SS) of the sentence. What is not clear is what must be taken to be the correct specification of the deep structure of a sentence. According to some, including Chomsky, the deep structure representation of sentences specifies functional relations (subject, object, etc.) among lexical items, but does not specify the scope of logical operators.<sup>6</sup> According to others, notably McCawley, G. Lakoff, Postal, and Ross,<sup>7</sup> the deep structure of a sentence will contain all instructions for semantic interpretation: in this theory the deep structure of a sentence is its semantic representation. The former theory is known as *Interpretive Semantics*, the latter as *Generative Semantics*. Since, for reasons that will become clear below, both terms must be regarded as rather infelicitous misnomers, I shall speak of *Autonomous Syntax* and *Semantic Syntax*, respectively.

The controversy over these two theories has become the central issue in the theory of syntax during the last few years. Right from the beginning,

<sup>6</sup> See Chomsky (1971a), (1972b).

<sup>7</sup> McCawley (1968a), (1971a); G. Lakoff (1971, section II is paper VII in this volume); Postal (1970a); Ross (1970a).



however, discussions have been bedevilled by a great deal of notional and methodological confusion. It is the purpose of the present paper to unravel the tangle and state the issue in the clearest possible terms. Some evidence will also be presented that would favour Semantic Syntax.

The confusion started when the notion of 'Semantic Component' was proposed by Katz, Fodor, and Postal.<sup>8</sup> It had become clear in the early sixties that a grammar consisting of syntax and phonology alone could never be a full description of a native speaker's linguistic competence. As we have seen, a native speaker not only knows how to distinguish grammatical from ungrammatical sentences and proper from improper pronunciations, he also knows what is a possible interpretation for a sentence; he knows what the sentences of his language mean, what their truth conditions are. Since syntactic (and phonological) rules define structures of symbols in a purely formal, algorithmic way, the structures involved will be meaningless as far as the descriptive apparatus is concerned. If they are taken to be meaningful, then their meaning is not accounted for in any way by the rules that have generated them. Consequently, a full description of a language will have to contain a Semantic Component, since otherwise the structures will remain uninterpreted. So far, the argument stands to reason. They then proposed that a system of 'Projection Rules' should be devised, with syntactic structures as their input and semantic representations as their output. The precise nature of the Projection Rules and the proposed SR's remained, however, rather unclear. In so far as the notion 'semantic representation' has become clearer since then, it appears to be exactly the notion discussed under that name above. But as we have seen, SR's are synonymous, though entirely unambiguous, expressions for the sentences they are related with, with their own syntactic structure and hierarchy of constituents. The so-called Projection Rules can, therefore, only be regarded as transformations mapping one syntactic structure on to another, which happens to be the SR. This is yet another algorithmic operation defining structures.<sup>9</sup>

It is thus clear that a Semantic Component in the sense proposed by Katz, Fodor, and Postal in 1963 and 1964 does not provide an answer to the original problem of semantic interpretation. It creates a level of representation, the SR, which had not been recognized before and which we now reckon to be anyhow necessary in an adequate description of a language. But proper rules of semantic interpretation can never be a mapping between linguistic (syntactic) structures of different levels of representation. They will be a mapping of some sort between SR's and cognitive structures, of a type still to be specified. The problem of

<sup>8</sup> Katz and Fodor (1963); Katz and Postal (1964).

<sup>9</sup> See also Seuren (1969, pp. 84-7).

providing semantic interpretations for algorithmically characterized formal structures is well known in logic and is dealt with in Model Theory. In introducing their notion of Semantic Component into the theory of grammar, Katz, Fodor, and Postal clearly derived inspiration from Model Theory, from which the terms *Projection Rule* and *interpretation* are taken. But the conclusion seems inescapable that their application of model-theoretic notions of projection and interpretation to the theory of syntax was vitiated by some radical misunderstanding.

Yet, leaving model-theoretic interpretation out of account, it is possible to re-interpret the original proposal of a Semantic Component in the following way. Let there be a set of rules making the correct predictions about wellformedness. This set of rules is called syntax. Let there be a distinct set of rules, called Semantic Rules, which make the correct predictions about possible interpretations for sentences. In principle, Semantic Rules will 'scan' syntactic structures (of any level of depth) under topological conditions of analysability, in the way of syntactic transformations, mapping these structures into SR's. The Semantic Rules form together the Semantic Component, which now maps syntactically defined structures on to SR's, instead of interpreting them. In this conception of grammar, SR's have no function in syntax, and there are at least some mapping rules which are irrelevant for the definition of wellformedness but whose sole function is to predict possible interpretations. There is an ultimate underlying structure in syntax, a Syntactic Deep Structure (SDS) for every sentence, which is distinct from its SR and for which syntactic formation rules, the Base, will have to be formulated. This theory of grammar is called *Autonomous Syntax* in this paper: it will be clear from the above that the commonly used term *Interpretive Semantics* tends to perpetuate the old misunderstanding about semantic interpretation.

The question that immediately presents itself is: what level of syntactic structure could be input to the Semantic Rules in a theory of Autonomous Syntax? Semantic Rules may scan syntactic deep structure, surface structure, or any intermediate structure, or a combination of these. Katz and Postal (1964) proposed that all semantic information was contained in the level of SDS. Transformations will then be meaning-invariant. When this theory was proposed, it won wide approval, and for a couple of years it was universally adopted among transformational grammarians. It is known as the *Standard Theory*, and is set forth in Chomsky's *Aspects of the Theory of Syntax* (1965), purified of its original sin of spurious semantic interpretation. The main difficulty with this theory is that it is self-destructive. For if the requirement is upheld that transformations should not affect meaning, then deep structures will have to be set up which are very different from those proposed, for example, in *Aspects*. Deep

structure will become very much more 'abstract', and will in the end be indistinguishable from what SR must be taken to be. This conclusion was reached by G. Lakoff, Ross, McCawley, Postal, and many others.<sup>10</sup> Thus Standard Theory leads inevitably to the elimination of the Semantic Component which it defines.

The main obstacle that Standard Theory puts in the way of the rules and structures proposed in *Aspects* is formed by scope-bearing elements such as quantifiers, negation, modal verbs. The examples are well known. Thus, the following two sentences have different possible meanings:

(8) Nobody here knows two languages.

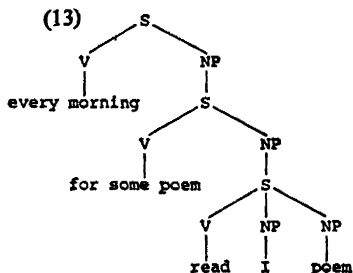
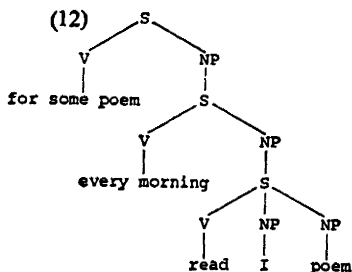
(9) Two languages are known by nobody here.

If the passive is formed by a rule that shifts the whole object noun phrase, including quantifiers and/or negation, to subject position, and makes the whole subject the object of a *by*-phrase, as, in principle, the passive rule presented in *Aspects* does, then clearly, the passive rule may affect the meaning of the sentence involved. But if the passive rule applies only to the lexical heads of subject and object, operators being represented as 'higher verbs', to be incorporated later, then the rule is meaning-invariant. But then also SDS collapses with SR. Likewise:

(10) I read some poem every morning.

(11) Every morning I read some poem.

differ in possible interpretations. (10) is ambiguous in the scope of its quantifiers: it either means 'there is a poem that I read every morning', or 'every morning there is a poem that I read'. But (11) can only mean the latter, not the former. Again, if there is a rule which optionally moves sentence adverbials to the front, it will sometimes have semantic effects. In order to avoid that, one must set up deep structures that bring out the two possible interpretations such as (12) and (13).<sup>11</sup>



<sup>10</sup> e.g. Gruber (1967c); Seuren (1969).

<sup>11</sup> Following McCawley's proposal (1970a, paper III in this volume).

Furthermore, certain restrictions will have to be imposed on the ways in which quantifiers can be lowered, so that (12) will result only in (10), but (13) in either (10) or (11)<sup>12</sup>. But (12) and (13) contain information that will have to be specified anyhow in the SR's of (10) and (11). If a SDS is assumed distinct from SR, there will be a vast amount of unnecessary duplication in any form of the Standard Theory. In fact, no instance is known of semantic information that will not have to figure in SDS, *if transformations are required to be meaning-invariant*.

It is thus clear that the Standard Theory, despite its name, is self-defeating. At present, its unique defender is Katz.<sup>13</sup> His arguments, however, do not carry much weight. For if all confusion concerning the notion of semantic interpretation is eliminated it becomes clear that the distinction between SDS and SR is vacuous: a deep structure containing all semantic information *is* an SR, since it contains all instructions for proper interpretation.<sup>14</sup>

If the idea of a Semantic Component is to make any sense, its input cannot be just SDS. Can it be surface structure? No linguist will take this possibility seriously these days, although many logicians still separate syntax and semantics in this way. Such a view would be linguistically naïve in the light of what is known about underlying structures. It would imply that all semantic information is to be extracted from SS by means of semantic rules that have no relation to the rules of syntax. There are too many cases, however, where the information required for semantic

<sup>12</sup> G. Lakoff (1971, Section II is paper VII in this volume) makes an attempt at formulating such restrictions. He introduces rules extending over (part of) a derivation, taking transformational history into account. These rules are called *global rules*, or *global constraints*. See also G. Lakoff (1970c, paper VI in this volume). Another attempt was made by Seuren in a paper read for the Società Linguistica Italiana in Rome, December 1969 (1971). Roughly, this proposal amounts to the following. A distinction is made between more or less peripheral, or more or less central, operators: if an operator, after lowering, will be a main constituent in the resulting sentence, it is less central, or more peripheral, than an operator that will occupy a position of greater depth in the tree. Operator lowering is cyclic. If an operator is lowered after another, more peripheral operator, then there is an output constraint requiring that the higher and more central operator precede the less central one which is under its scope. But if a more peripheral operator is lowered after a more central one, surface ordering is less crucial. Thus for (12) there is an output constraint barring (11), but for (13) there is no such constraint.

<sup>13</sup> Katz (1970).

<sup>14</sup> A further confusion lies in Katz's idea that the Semantic Component will, by a process of 'amalgamation', select a, or the, appropriate reading by filtering out selectionally 'incompatible' combinations of readings of lexical items. We have seen, however, in Section 1 that this cannot reasonably be expected of a description of the language. McCawley (1968a, pp. 127-9) deals conclusively with this point. He points out that Katz's rules will select the reading 'chess piece' for *king* in: *The king is made of plastic*, excluding the reading 'monarch'. There is, however, no reason to exclude the latter reading in, e.g. *It is nonsense to speak of a king as made of plastic*, or *How could you possibly think that the king is made of plastic?*

purposes is already given on some level of underlying structure. A clear instance is Subject Deletion in embedded clauses. This is a syntactic rule, as appears from the following (leaving out details irrelevant for the present argument). Consider the sentences:

- (14) Mary told John to help himself.
- (15) Mary told the boys to meet at 5.
- (16) Mary told the children to help each other.

but:

- (17) \*Mary told John to help herself.
- (18) \*Mary told the boy to meet at 5.
- (19) \*Mary told the child to help each other.

Consider also:

- (20) Mary promised John not to contradict herself.
- (21) The boys promised Mary to meet at 5.
- (22) The children promised Mary to help each other.

but:

- (23) \*Mary promised John not to contradict himself.
- (24) \*Mary promised the boys to meet at 5.
- (25) \*Mary promised the children to help each other.

The application of ordinary criteria of descriptive adequacy leads us to posit an underlying subject for the *to*-infinitives in (14)–(16) and (20)–(22), as is, of course, well known. If the higher verb is *tell*, the underlying subject will be identical with the indirect object; if the higher verb is *promise* the underlying subject will be identical with the subject. The ungrammaticality of:

- (26) \*John helped herself.
- (27) \*The boy met at 5.
- (28) \*The child helped each other.
- (29) \*Mary contradicted himself.
- (30) \*Mary met at 5.
- (31) \*Mary helped each other.

which has to be accounted for in the syntax anyway, will now automatically also exclude (17)–(19) and (23)–(25). The rule of Subject Deletion will, under the conditions of identity stated above, delete the embedded subjects.

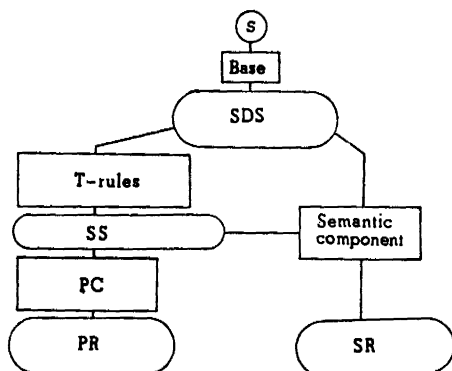
The underlying subject in the sentences discussed above, however, is also

a semantic fact. Every speaker of English knows that, for example, in (14) *John* is supposed to help himself if he does what Mary told him, or that in (20) *Mary* will not contradict herself if she does what she promised. If a semantic, non-syntactic rule extracts this information from the SS, it will only duplicate what the syntax has already done. If there are going to be semantic rules at all, they must not reinstate deleted subjects. Rather, that part of the ultimate underlying structure which contains the underlying subject will be copied in the SR, in Autonomous Syntax.

It is Chomsky's view that some semantic information will be copied from SDS, whereas other semantic information will be derived from SS by means of semantic rules. In particular, he reckons it to be a viable generalization that all functional relations, such as subject, object, main verb, will be defined on the level of SDS, but that the scope of operators is to be scanned from SS, mainly on the basis of their order of occurrence, though by no means exclusively so, as appears from (10). In this way he saves the essentials of the rules and structures proposed in *Aspects*, but he modifies the Standard Theory to include surface structure as an input to semantic rules. Transformations will then be meaning-invariant as far as functional relations are concerned, but they will have irregular semantic effects on the scope of operators. Chomsky speaks of this new version of Autonomous Syntax as of the *Extended Standard Theory*.

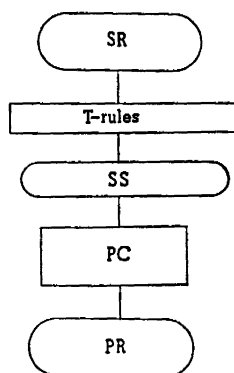
As far as can be seen, this theory is the only version of Autonomous Syntax that does not make vacuous or confused claims and has not already been refuted by the facts (as the theory of surface structure semantic scanning has). It can be diagrammatically represented as in (32). (Square boxes represent sets of rules; round boxes represent sets of structures defined by the rules. PC stands for: Phonological Component.)

(32)



Semantic Syntax, on the contrary, maintains that there is no Semantic Component and no SDS. The ultimate underlying structure is the SR, and the transformational rules map SR's into SS's. In this theory the formation rules for the ultimate underlying structures have a very different status: they define the wellformedness of SR's. Given the great deficiency of our knowledge of SR's as well as of cognitive structures, it would be impractical to attempt to formulate such rules at present. Semantic Syntax can be schematized as in (33).

(33)



The issue between the two theories is now beginning to become clearer. A few years ago, when Chomsky first presented his 'Deep Structure, Surface Structure, and Semantic Interpretation' (1971a), the issue was less clear. Chomsky maintains there that Semantic Syntax is nothing but a notational variant of the Standard Theory (i.e. the diagram of (32) minus the horizontal line connecting SS with the Semantic Component), since, counting out the Base, in both theories there is a well-defined mapping relation between SR and PR. Any 'directionality' in the way grammatical processes are described in terms of input and output, is a matter of formulation: no empirical claim is involved here. What is claimed empirically is that the various levels mentioned are connected through mapping relations in the way indicated.

Yet, although directionality may be irrelevant and both theories imply a well-defined mapping relation between SR and PR, it should be clear that Standard Theory and Semantic Syntax do not make the same empirical claims, and are, therefore, not notational variants of each other. The Standard Theory, as well as the Extended Standard Theory, claim that in so far as the explanation and correct prediction of facts of wellformedness is con-

cerned, a level of ultimate underlying structure, SDS, must be assumed for every sentence, which is distinct from the SR of that sentence. Semantic Syntax, on the contrary, maintains that there is no level of ultimate underlying structure that will account for all facts of wellformedness, which is not identical with SR. These are two different empirical claims about the explanation of the facts of wellformedness. In the case of the Standard Theory the claim that SDS is distinct from SR is, as we have seen, vacuous, or necessarily false, since a level of representation that contains *all* semantic information can be nothing but SR. For the Extended Standard Theory, however, the claim does make sense, and will count as true as long as, for some specific proposal of SDS, no syntactic evidence has been discovered showing it to be inadequate. In this theory the claim *must* be made, since it implies that some semantic information is not present in SDS.

It is Chomsky's present opinion (1972b) that there is no other difference between Semantic Syntax and the Extended Standard Theory than the way in which lexical items are assumed to be inserted. He defines SDS as the level at which all relations between lexical items have been defined: the Base rules do precisely that; they define lexical relations. In most studies in Semantic Syntax, on the contrary, it is assumed that there is no such level. The view is defended that lexical items come to replace sub-trees under certain conditions, during the transformational process.<sup>15</sup> In other words, rules of lexical insertion, though formally distinct from transformations, do not apply in a block but alternate with transformations, albeit in ways that are only partially understood. A variety of arguments is given for this view. A simple and clear argument is provided by the fact that adjectives and verbs with so-called 'negative connotation' allow for the occurrence of items such as *ever* or *any*, which regularly occur in negative contexts, whereas their positive counterparts do not allow for these to occur. Compare, for example:

(34) Charlie *denied* that he had ever been in Paris.

(35) It is *difficult* for Charlie to admit that he has ever been wrong.

with:

(36) \*Charlie *admitted* that he had ever been in Paris.

(37) \*It is *easy* for Charlie to admit that he has ever been wrong.

if there is a *some/any* rule, converting *some* into *any* (*at some time into ever*) when, among other conditions, *some* occurs under the scope of negation, then clearly (34) and (35) ought to fall under this rule. Just to list the verbs and adjectives with negative connotation for the purpose of this and

<sup>15</sup> The first descriptive attempts along these principles were made by Gruber, especially (1967c).



other rules, without explaining the list by means of the negation element, would be the worst kind of taxonomic grammar. But if the ultimate underlying structure of the sentences involved already contains the full lexical items *deny* or *difficult* there is no way of letting the *some/any* rule operate in a non-*ad hoc* manner on these items. It is assumed, therefore, that the negation element, which is somehow 'hidden' in these items, is explicit in the ultimate underlying structure, and that the actual items are the result of later lexical replacement.

The conclusion does not hinge on the existence of the *some/any* rule. Other observations support the same conclusion. Thus it is a well-known fact, noted by Ross,<sup>16</sup> that negation does not occur in the *than*-clause of a comparative:

(38) \*John is taller than Fred is not.

(39) \*John is taller than I didn't think he was.

The same syntactic restriction is operative for verbs and adjectives with negative connotation:

(40) \*John is taller than Charlie *denied* he was.

(41) \*Mary is more liberated than is *difficult* for her mother to understand.

Whatever the explanation is for this restriction in the comparative, it will have to be the same for *not* as for negatively 'coloured' verbs and adjectives.

If Semantic Syntax is right on this point, as I think it is, then *the particular SDS proposal* made by Chomsky is proved to be wrong. This does not mean that no other SDS hypothesis can be formulated: there seems to be no limit to the number of thinkable SDS proposals. One might think of a lexicon with deep and surface items, where SDS defines relations among deep lexical items; the scanning of scope-bearing elements would then still be left to semantic rules operating on SS. On the other hand, if it were the case that all lexical items were the result of a single-block application of lexical rules before all transformations, this would not destroy the case of Semantic Syntax. It would have no other consequence than that a new level of lexical representation would have to be inserted between SR and SS. But SR would still be the ultimate underlying structure for every sentence. It thus appears that unitary lexical insertion, far from being the crucial difference between Autonomous and Semantic Syntax, is compatible with both. It happens to be essential only for the specific form of Autonomous Syntax proposed by Chomsky, not for Autonomous Syntax as a theory of grammar.

<sup>16</sup> Ross (1969a, p. 294); compare also Seuren (1969, p. 129); (1973a).

The essential difference between Autonomous and Semantic Syntax consists in different empirical claims about the explanatory principles, or the causes, of facts of wellformedness on the one hand and semantic facts on the other. For Semantic Syntax all semantic facts will be explained by the rules of syntax: no principle explaining possible interpretations will not be a rule of syntax. For Autonomous Syntax some semantic facts will be explained by syntactic rules, others (facts of scope, in particular) will be explained by purely semantic rules. In this theory syntax is not quite, but partially, 'autonomous and independent of meaning'.<sup>17</sup>

If one concentrates on semantic facts, as a logician would, and sets out to provide rules making the correct predictions about possible interpretations of sentences, then, if Autonomous Syntax is the correct theory, these rules and those of syntax will overlap: some semantic rules will also be syntactic, and some syntactic rules will also be semantic. However, if Semantic Syntax is right, then the semantic rules and those of syntax will be exactly the same (although they would be formulated with opposite 'directionalities'). The issue is not that in the total description of the language (the native speaker's linguistic competence) some rules will occur according to one theory but not to the other. It is rather that what will turn out to be the same rules in either theory (irrespective of 'directionality') will have different explanatory power with respect to the data.

One will therefore have to disagree with Chomsky when he states that there is no essential difference between Lakoff's global rules and surface structure semantic rules. Global rules, as was said in footnote 12, are rules extending over transformational derivations. They can be viewed as rules specifying sequences of transformations for particular underlying structures, or as sets of rule features. They are necessary in Semantic Syntax, among other things, for a correct surface ordering of operators given their position in SR. (According to G. Lakoff (1970c, paper VI in this volume), there are many other cases, of an indisputably syntactic nature, where global constraints must be assumed to be rules of syntax.) They are, as Chomsky points out, equally necessary in a linguistic description conforming to the theory of Autonomous Syntax. Here they will specify sequences of semantic transformations, converting SS's into SR's, while ensuring that operators occupy their proper positions in SR's given their occurrence in surface structures. They will still be rules extending over sequences of (semantic) transformations. But this is beside the issue. The issue is that if Semantic Syntax is correct, there must be purely syntactic facts of wellformedness calling for the rules in question, whereas for Autonomous Syntax there must be no such facts.

In principle, the issue between Autonomous and Semantic Syntax

<sup>17</sup> Cf. Chomsky (1957, p. 17).

cannot be decided conclusively on the basis of linguistic facts alone: there is no proof that either theory is right or wrong unless outside evidence is provided. This is because counterexamples to either theory will themselves embody universal claims about infinite sets of linguistic facts. If it could be shown that there is at least one rule required for the correct description of semantic facts which has no place in syntax, or, in other words, that all syntactic facts can be accounted for adequately without this particular semantic rule, then Autonomous Syntax would have proved itself. But this cannot be shown. The counterexample, i.e. the rule in question, can be shown to be wrong, but not right. In general, a particular SDS hypothesis, or a particular Semantic Syntax description, can be shown to be wrong by providing linguistic facts as counterexamples or by showing that it fails in simplicity (generality), but it cannot be proved to be correct. The best that can be achieved is reasonable certainty. If we feel reasonably certain about the adequacy of a description which crucially involves Autonomous Syntax, we have reasonable certainty that this theory is correct. Given this situation one can do no better than go by the balance of the available evidence. If one SDS proposal after another is shown to be inadequate for the purpose of syntax, the case of Semantic Syntax will be strengthened. If, on the other hand, some SDS hypothesis continues being resistant to counterexamples, or if no syntactic argument can be constructed in support of a particular rule formulated for semantic reasons, the balance will swing in favour of Autonomous Syntax. The strict undecidability of the issue, however, does not make it unempirical. It would be settled if we could make the structures and processes of the brain visible in some way or other. Then we would be able to inspect linguistic competence directly so that the issue could be settled decisively. Or the issue might be settled on the basis of other, external, evidence about brain structures and processes. But no such evidence is available at present, and we have, therefore, to rely on what provisional conclusion can be drawn from linguistic evidence.

Since in Semantic Syntax one sweeping generalization is made for all facts of syntax and semantic interpretation, it is incumbent upon the defender of Semantic Syntax to present observed facts as counterexamples to any new proposal made within the theory of Autonomous Syntax. According to Chomsky there is some merit in the position of Autonomous Syntax, from a methodological point of view. The merit would consist in the fact that more, and more specific, restrictions are imposed on an Autonomous Syntax grammar than on one within the theory of Semantic Syntax. He writes:<sup>18</sup>

The fundamental problem of linguistic theory, as I see it at least, is to account for the choice of a particular grammar, given the data available to the language

<sup>18</sup> Chomsky (1972b, p. 67).

learner. To account for this inductive leap, linguistic theory must try to characterize a fairly narrow class of grammars that are available to the language learner; it must, in other words, specify the notion 'human language' in a narrow and restrictive fashion. A 'better theory', then, is one that specifies the class of possible grammars so narrowly that some procedure of choice or evaluation can select a descriptively adequate grammar for each language from this class, within reasonable conditions of time and access to data. . . . For the moment, the problem is to construct a general theory of language that is so richly structured and so restricted in the conditions it imposes that, while meeting the condition of descriptive adequacy, it can sufficiently narrow the class of possible grammars so that the problem of choice of grammar (and explanation, in some serious sense) can be approached.

The theory of Autonomous Syntax is precisely such an attempt at specifying a form of grammar that is sufficiently restricted to make the theory interesting.<sup>19</sup> It looks, however, as though restrictions of the type implied by Autonomous Syntax will be counterproductive in accounting for the 'inductive leap' made by the language learner. It will be remembered that the quarrel between the two theories is not necessarily about particular rules, but about the explanatory power of the rules with respect to the data of the language. The child will, therefore, have to select a particular set of rules, about the form of which there need be no quarrel between defendants of either theory. It is not clear that an account of this selective, or inductive, process is in any way facilitated by imposing restrictions on the explanatory power of the rules the child has to acquire anyway. On the contrary, the wider the range of data falling under the domain of a particular set of rules, the greater the explanatory power of these rules, and the more highly valued the account of the inductive process leading to their acquisition. The restrictions that will help to explain the inductive process will have to be restrictions on the form and ordering of rules, not on their range of applicability. In general, we search for a theory that is as highly restricted as possible as regards the form of the rules and internal organization, but which covers as wide a range of data as we can find for it.

It is clear that Semantic Syntax will bring the problem of linguistic induction nearer to its solution. In this theory, both the syntactic and the semantic properties of the same objects, sentences, can be adequately predicted by one single set of rules, the syntax. For the theory to have any chance of success this set of rules must be restricted in highly specific ways. And Semantic Syntax leaves the possibility of formulating such restrictions as open as does Autonomous Syntax. In the latter, the rules that have to be acquired in order to make the correct predictions about the syntactic properties of sentences will also account for *some* semantic

<sup>19</sup> The same wish to make grammars more highly restrictive underlies the work referred to by Peters and Ritchie (1969).

properties; other semantic properties will not be accounted for by the rules of syntax but by a separate set of semantic rules filling in the semantic gaps left by the syntax. The two sets of rules may or may not be subject to the same set of restrictions of form and internal organization. This theory makes more assumptions than is logically necessary in order to give a rational account of the inductive process. We will accept it if we are forced by the facts, but not for any reason of its being a 'better theory'. The 'better theory' of the two, i.e. the one providing the simplest explanation of the process of linguistic induction, is Semantic Syntax.

It cannot be argued that Autonomous Syntax is the 'better theory' because it assumes one level of representation, SDS, over and above those assumed by Semantic Syntax. This would be an incorrect argument since, as we have seen, the crucial controversy is not about the number of levels of representation, but about the explanatory power of the rules of syntax. There is nothing in Semantic Syntax which would make it incompatible with the assumption of further levels intermediate between SR and SS. There might be a level of lexical representation, although, as we have seen, that is not very plausible. There might be a level of 'shallow structure' defined by the end of the cycle and the beginning of the post-cyclic rules. Any distinction of levels of representation *within syntax* will be welcomed as a further step towards the solution of the inductive problem, since it implies a restriction of ordering among different types of rule: one type of rule applies in a block before another. But the distinction between SDS and SR in Autonomous Syntax is not of this nature. It does not imply a restriction on rules accounting for a unified category of data. It limits the applicability of the rules of syntax to all syntactic and some semantic properties of sentences and lets the rules of the Semantic Component apply to the remaining semantic properties.

It might be objected that all data of linguistic competence form a unified category. This is true, in so far as we have to do with attitudes about or properties of the same objects: sentences. But within the category of linguistic data there are distinctions which raise different empirical problems. There is no denying that a child, when learning its language, acquires three kinds of knowledge: about properties of syntactic wellformedness, about possible interpretations, and possible pronunciations of the sentences of the language. If one set of rules can be made to account adequately for all the different properties of the sentences, the explanation is more satisfactory than if two or three distinct sets of rules are required. In other words, if the rules of syntax define structures which contain all necessary information about proper interpretation and proper pronunciation, then there is no need for any phonological or semantic component. If, however, further rules, of a non-syntactic nature, are needed to specify possible

pronunciations or interpretations in appropriate universal terms of semantics and phonetics, then the addition of a phonological or semantic component to the grammar will be warranted. Unfortunately, the facts force us to admit a separate phonological component: no structure defined in syntax contains all information required for proper pronunciation. Surface structure comes closest to it but is essentially insufficient. (It might be sufficient if the problem were not to specify possible pronunciations but rather possible spellings.) Whether or not the same is true for a semantic component is exactly the issue.

## IV

As has been said above, the issue cannot be settled conclusively on the basis of linguistic facts alone; and no other relevant evidence is available. As it is, the only way of proceeding is to try and find syntactic facts that will serve as counterexamples to any particular proposal of a purely semantic, non-syntactic, rule or of a syntactic description in terms of some SDS distinct from SR. To show that the same universal restrictions hold for rules that are indisputably syntactic as for those that are the object of controversy between the two theories (such as the rules relating quantifiers in SS to those in SR), can have no more than suggestive value. Chomsky has recently proposed some universal restrictions on rules, both semantic and syntactic (1971b). If his proposals are correct, they will not destroy his own theory, as some might be inclined to think. It is possible for there to be two distinct sets of rules, each doing a different job of explanation, but both falling under the same universal constraints. On the other hand, if different general constraints turn out to apply to the well-established rules of syntax and, for example, rules handling quantifiers, this will not establish the case of Chomsky's Extended Standard Theory. What will be established, in that case, is that there is a distinction in types of rule. But the crucial issue, i.e. whether this is a distinction within syntax or between syntactic and semantic rules, will still be moot.

Ross's thesis (1967) is a splendid example of an attempt at formulating universal constraints on rules of syntax. McCawley has recently pointed out<sup>20</sup> that two of Ross's constraints, the Complex-NP-Constraint (CNPC) and the Coordinate-Structure-Constraint (CSC) seem to apply to rules handling quantifiers.

A complex NP is a noun phrase containing an embedded S and a lexical head, such as *the claim that S*, *the rumour that S*, or a relative clause plus

<sup>20</sup> At the LSA Winter Meeting, Washington, D.C., 30 December 1970.

its lexical antecedent. CNPC restricts movement of material into or out of the S of a complex NP. It prevents from occurring formations such as:

- (42) \*Where is the path that leads to 100 yards long?

which would have been the result of preposing of the element *wh-some-where* in the underlying:

- (43) The path that leads to *wh-somewhere* is 100 yards long?

If preposing does not apply, the result will be the correct sentence:

- (44) The path that leads to where is 100 yards long?

Similarly, CSC restricts movement into or out of a member of a coordinate structure, thus preventing formations such as:

- (45) \*What do you like wine and?

McCawley pointed out that these constraints can be made to explain certain semantic facts of scope ambiguity. As is well known, the sentence:

- (46) Tom believes that Fred knows many girls.

is ambiguous between the two readings 'Tom believes that there are many girls that Fred knows' and 'There are many girls that Tom believes that Fred knows'.

However, the sentences:

- (47) Tom believes the rumour that Fred knows many girls.

or

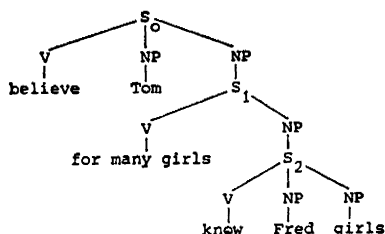
- (48) Tom believes that Fred is charming and knows many girls.

are not ambiguous in that way. They do not allow for the interpretations:

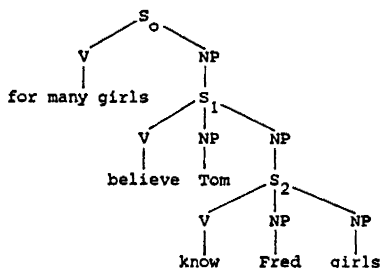
- (49) \*There are many girls that Tom believes the rumour that Fred knows.  
 (50) \*There are many girls that Tom believes that Fred is charming and knows.

In fact, (49) and (50) are ungrammatical because of their violation of CNPC and CSC respectively. Let us assume that the SR's of (46) differ in at least the ways shown in (51) and (52).

(51)

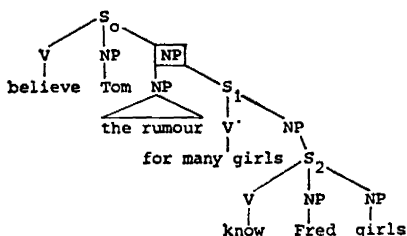


(52)

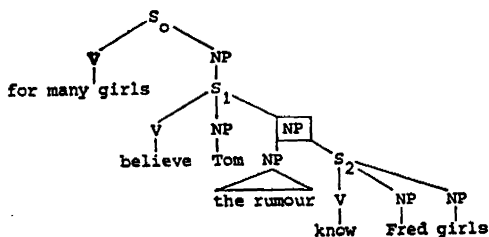


Neither CNPC nor CSC prevents the lowering of the quantifier *for many girls* into  $S_2$ , or, in terms of semantic rules, the raising of *many* in *Fred knows many girls* of (46) to the position V in either  $S_1$  or  $S_0$ . For (47), however, the SR can only be (53), and not (54).

(53)



(54)





This is explained by CNPC, since in (54) the quantifier *for many girls* is outside the boxed NP, which is complex, so that it cannot be moved into, or have been moved out of (according to different directionalities of the description) that NP. An analogous analysis will exclude one reading for (48) on the basis of CSC.

Although this is a highly interesting, and also somewhat suggestive, generalization, it does not prove anything concerning the present controversy. What is at issue is the status of the rule relating the quantifier in SR to its counterpart in SS. Is this a syntactic or only a semantic rule? If we wish to establish that it is a rule of syntax we must be able to show that in some cases violation of some constraint will result in an ungrammatical, i.e. syntactically ill-formed, formation. This is not the case for (54). If we lower the quantifier into  $S_2$ , thereby violating CNPC, the resulting sentence is still grammatical, but it does not mean what (54) means. There are cases, however, as McCawley did not fail to point out, following an observation made by Ross,<sup>21</sup> where violation of some constraint by a rule handling quantifiers or other operators does result in an ungrammatical formation:

(55) \*John and nobody are similar.

(56) \*John and somebody are similar.

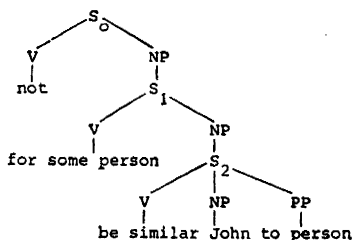
(57) \*John and many people are similar.

Here, CSC has been violated, as appears from:

(58) John is similar to nobody (somebody/many people).

Let the supposed SR of (55) contain at least the structure shown in (59).<sup>22</sup>

(59)



If, on the  $S_2$ -cycle, an optional transformation of NP-Conjunction has converted  $S_2$  into: *be similar*<sub>NP</sub>(*John and person*)<sub>NP</sub>, CSC will prevent *not* and *for some person* from being lowered into  $S_2$ . The ungrammaticality of (55)

<sup>21</sup> Ross (1967, 6.4.2.1, pp. 458-9).

<sup>22</sup> It is immaterial whether *similar to* in  $S_2$  is the ultimate underlying form, or *John and person*, as G. Lakoff and Peters (1969) have it. The point is that operators are not lowered into a coordinate structure, whether it is 'original' or not.

is a syntactic fact, and thus supports the view that Quantifier Lowering is a rule of syntax.

A similar argument applies to the proposed rule of Negative Raising,<sup>23</sup> which raises the negation of a *that*-clause into the higher clause under certain conditions, particularly the condition that the higher verb belong to the class of negative raisers. *Think, believe, suppose* are such verbs. Accordingly, the sentence:

(60) I don't think you are right.

is ambiguous: it means either 'It is not the case that I think you are right' or, and preferably, 'I think that you are not right'. Since the sentence is ambiguous, it must have two SR's, which will differ in the scope of *think* and *not*. The question is, do we have semantic Negative Lowering or syntactic Negative Raising? The fact that all so-called negative polarity items<sup>24</sup> (such as *any, ever, can possibly, at all*) occur freely in the *that*-clause when the higher negative-raising verb is denied:

(61) I don't think anybody will understand you.

(62) John didn't believe I had ever been in Paris.

(63) I didn't think John's answer could possibly be correct.

(64) Fred didn't think the show was at all funny.

might lead one to suspect that there is a syntactic rule of Negative Raising. The evidence is not sufficient, however, since some verbs that are not negative raisers allow for the same items to occur in the *that*-clause when they are denied:

(65) I didn't realize that anybody had understood you.

(66) John didn't realize that I had ever been in Paris.

(67) I didn't foresee that John's answer could possibly be correct.

(68) Fred didn't know the show was at all funny.

Apparently, the negative polarity items mentioned do not require the negation element to occur in the same clause. It is sufficient for there to be a 'negative context', the precise character of which is not very well understood. There are at least two negative polarity items, however, that require the negation in the same clause, punctual *until* and *yet*:

(69) Tom won't get here until tomorrow.

(70) Tom hasn't found the solution yet.

<sup>23</sup> See, for example, R. Lakoff (1969, page r VIII in this volume).

<sup>24</sup> See, for example, Baker (1970b).

These occur in non-negated *that*-clauses only after negated negative raisers:

(71) I don't think Tom will get here until tomorrow.

(72) I don't think Tom has found the solution yet.

but

(73) \*I didn't realize that Tom would get here until tomorrow.

(74) \*I didn't realize that Tom had found the solution yet.

So the conclusion seems justified that at least for (71) and (72) we must assume a syntactic rule of Negative Raising. This conclusion finds further support in the fact that both CNPC and CSC apply to this rule, blocking it in the case of complex NP's or coordinate structures:

(75) \*I don't believe the rumour that Tom will get here until tomorrow.

(76) \*I don't think Tom will get here until tomorrow and is a reliable chap.

(77) \*I don't believe the rumour that Tom has found the solution yet.

(78) \*I don't think Tom has found the solution yet and is an intelligent chap.

If there were only a semantic rule of Negative Lowering from SS to SR, the syntax would have to specify that punctual *until* and *yet* may occur in non-negated object-clauses of negated higher verbs belonging to exactly the class that the semantic rule will specify as Negative Lowerers, except when the clause is part of a complex NP object or a constituent in a coordinate structure. It is clear that some significant generalization would thus be lost.

Although the evidence presented is insufficient to force a decision between Autonomous and Semantic Syntax, it shows at least some fundamental flaws in Chomsky's version of Autonomous Syntax, the Extended Standard Theory. It is not clear what other version of Autonomous Syntax could be invented which would not fall victim to the objections raised against the Extended Theory or against previous versions of Autonomous Syntax.