

Review article

James D. McCawley, *Thirty Million Theories of Grammar*. The University of Chicago Press, Chicago / Croom Helm, London, 1982. Pp. 223. Hardcover. Price: £ 14,00.

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This book contains four studies by McCawley (McC) and a nine-page introduction. The four papers were written in the 1970s and deal, for the most part critically, with theories and descriptions proposed, in that period by Chomsky or linguists closely associated with him. The first, and by far the largest paper (118 pages) was written in 1973 and published in the relatively inaccessible *Studies in English Linguistics* 3; 209-311 (1975). It is an extensive critique of Chomsky (1972) (surprisingly not included as a separate item in the References). The second paper is a corrected version of an article which appeared in 1976 in *Linguistic Inquiry* under the title 'Notes on Jackendoff's theory of anaphora', but is entitled here 'How to get an interpretive theory of anaphora to work'. Then follows 'Language universals in linguistic argumentation', McC's forum lecture at the 1978 Linguistic Institute held at the University of Illinois at Champaign-Urbana. And lastly, there is 'The nonexistence of syntactic categories', a revised and expanded version of a paper read at the Second Annual Michigan State Linguistic Metatheory Conference, in 1977.

In these four papers McC vindicates, in principle, the major tenets of what has become known as generative semantics against alternatives proposed by Chomsky and associates. He carefully avoids, however, being drawn into artificial camp distinctions or school labellings. His strategy throughout is to concentrate on issues, not on what are considered 'schools of thought'. He is remarkably silent on aspects of professional behaviour. Yet the reader cannot help noticing, on going through McC's text, that not infrequently a theoretical position advanced by linguist A was not accepted in Chomskyan circles, whereas an identical or crucially similar position advanced by linguist B was warmly welcomed there. And he will infer that this had to do with the fact that B belonged to the 'right' school. But McC is hardly ever explicit on this count. An exception is p. 153, where he states explicitly that certain theoretical positions associated with certain linguists have too often been evaluated on the grounds of such associations rather than of their intrinsic merits. Negative value judgments are studiously avoided. If they are found they are never tied to a name, at most to a group, as on p. 160 where McC speaks of 'cheap arguments' developed by linguists on the basis of ad hoc universals of grammar, proposed only to support favourite analyses: "Those argu-

ments have given the illusion of significance only because their alleged role in the war effort against 'excessive power' has obscured important respects in which they are extremely implausible." Positive value judgements, on the other hand, are made with delicate moderation.

Stylistically, McC shows himself, again, a master. Not only does he have an extraordinary command of the English lexicon, he also shows that the old dictum that English does not like long sentences is wrong. His style is thoroughly English and yet abounds with very long sentences rolling themselves out agreeably before the reader's eye. It apparently just takes a better hand to write such sentences in English than in other languages better known for their pliability to lengthy periods. As a result there is a certain highly personal baroque-ness about his style, reflected also in the somewhat extravagant title of this book (as well as of one or two other books by the same author).

More importantly, however, his arguments are generally forceful and lavishly peppered with references to existing literature. They excel in showing common sense and a feeling for well-balanced proportions. Only rarely does an argument fall flat, as on p. 164-5, where McC argues against "the putative universal that no quantifiers need be used in formulating transformational rules". Bach uses this universal to argue for an underlying VSO-order in Amharic, despite the surface SOV-order, in an article (1970) which is otherwise highly praised by McC. McC's objection (p. 165) is that scanning procedures involving the spotting of first, second, ..., last occurrences of certain phenomena in perceptual fields are well-attested in perceptual mechanisms (he refers to Miller & Johnson-Laird (1976)), and he concludes that there is therefore no a priori reason to exclude such procedures from grammars: "I hold that it is implausible to suggest that organizational features that figure widely in perception and in non-linguistic knowledge are systematically excluded from language." This may be so on a priori grounds, but it fits in badly with McC's repeated insistence that rules of grammar are not to be regarded as formulations of psychologically real procedures under any direct process interpretation. Moreover, if in the practice of relatively successful grammatical description it is found that rules tend to conform to a certain (perhaps not yet clearly definable) pattern or format which never involves quantification over nodes or categories, then it is certainly preferable, and thus arguably better, to adopt descriptions that follow the established format. As has been said, however, the great bulk of the arguments developed by McC in this book are not of this nature. I have mentioned the argument against Bach only because of its rarity value.

Reading the first paper, McC's extensive critique of Chomsky (1972), is like leafing through an old photograph album. The whole gamut of once familiar issues is gone through again, and one can't help smiling at the thought of those old debates, which for the most part have led to so surprisingly little. One relives the discussions on nominalizations, *respectively*, contrastive stress and focus, on quanti-

fiers, on anaphora, on the present perfect versus the simple past tense, on questions of lexical decomposition and lexical insertion, - to mention but the most outstanding among the issues at hand. And a certain melancholy is unavoidable when one realizes how limited, on the one hand, the terms and insights were within which these discussions took place and how restricted the knowledge of the factors and parameters involved, and, on the other hand, what far-reaching conclusions were drawn. It is remarkable that McC's comments on the issues discussed are still relatively fresh and considerably less faded than the arguments he criticises. His reliance on good common sense and on normal proportions in intellectual discourse made him see quite clearly, even then, the abyss of ignorance and thus the relativity of the arguments and the conclusions.

This does not mean that McC is in any way 'soft' with his opponent, Chomsky. On the contrary, he makes tasty mincemeat of virtually all the arguments proposed in Chomsky (1972). Every single issue is reviewed, and on practically all counts Chomsky is shown to be crucially wrong. McC shows in particular that Chomsky's proposals regarding nominalizations in 'Remarks on nominalizations' do not warrant the distinction as drawn by Chomsky between a 'lexicalist' and a 'transformationalist' approach. As is borne out by later developments, this distinction was artificial and inconsequential.

In Chomsky's own words (1972: 54), "the strongest and most interesting conclusion that follows from the lexicalist hypothesis is that derived nominals should have the form of base sentences, whereas gerundive nominals may in general have the form of transforms". If this claim were correct, it would be an interesting one, but in no way opposed to anything deserving the name of 'transformationalist position'. The claim, however, is multiply falsified. The most pervasive class of counterexamples consists of cases of Equi-deletion: we find subject-to-subject deletion in nominals such as *Harry's refusal to leave*, *your wish to succeed*, and object-to-subject deletion in cases like *Mary's permission to go*, *Monty's order to attack*. Then there are cases like *John's tendency to be rude*, displaying subject-to-subject raising, an example quoted by McC in a note on p. 111. (One should note, however, that the lexicon does not seem to follow the syntax on all points, witness the impossibility of **its tendency to rain in Spain*, despite the normal *It tends to rain in Spain*.) Surprisingly, these obvious problematic cases are not discussed at all by Chomsky. Later, the theory was developed, in his school, that Equi-deletions are not part of the syntax at all, but 'only' of the semantics. The problem of subject-to-subject raising has never been treated seriously. Another important and obvious class of counterexamples is provided by those nominals that incorporate a passive: *the destruction of the city by the enemy*. Here Chomsky resorts to the amazing tactic of proposing that the Passive rule applies both to sentences and to noun phrases containing nominals. It is difficult not to be impressed by this stark violation of what Chomsky calls the lexicalist hypothesis.

What remains of Chomsky's claim is that some transformations

are seen to operate both in sentences and in nominals, whereas others are not. Thus, for example, the so-called rule of *tough-movement* (*Harry is easy to convince*) never seems to be reflected in nominals: * *Harry's ease to convince*. The Chomsky approach is to find ways of showing that those transformational rules that do manifest themselves also in nominals do not, after all, really belong to syntax. The opposite approach would be to try to show that those rules that do not occur in nominals are not, after all, really syntactic. Both approaches are misdirected unless there is powerful independent evidence that either position is fruitful and therefore justified. But such evidence has not been forthcoming. In these circumstances it seems most sensible to leave grammar (syntax) as it stands, and try to find some rationale for the fact that some transformational rules do occur within the lexicon while others do not.

Instead, the Chomsky school insisted on upholding the extreme position labelled 'lexicalist', and it thus became necessary to graft semantic interpretation rules onto surface structures, as well as on deep structures, - the development known as 'Extended Standard Theory' (EST). The two remaining papers in Chomsky (1972) are devoted to an exposé and a defence of EST. McC (p.78) observes correctly:

"Chomsky grossly exaggerates when he indicates that the EST involves a narrow departure from standard theory:

Then the standard theory asserts that the rules include transformations, the base rules, and the output condition noted <the condition that a surface structure may not contain # or Δ >, along with the rules that map deep structures onto semantic representations. EST identifies certain aspects of semantic representation that are determined by deep structure, or others that are determined by surface structure, but otherwise permits no new sorts of rules. (Chomsky (1972: 141))

His use of 'otherwise' brings to my mind the line 'Aside from that unfortunate incident, how did you enjoy your evening at the theater, Mrs. Lincoln?'"

McC then illustrates the enormous changes that are needed if the semantics is grafted on the surface structures as well as on the deep structures.

This illustrates a remarkable feature in Chomsky-type linguistics: all the action is taken to be in the syntax, whereas the semantics is considered a sideshow of a sideshow. Commenting on the rush for 'universal' constraints on rules which made Chomskyan linguistics quite feverish during the 1970s, McC notes (p. 72): "There is, incidentally, a striking gap in Chomsky's list of things that must be restricted: he does not mention placing any restriction on the category of 'admissible semantic interpretation rules'." This point is illustrated by McC on p. 58-9, where he comments on Chomsky's criticism of the rule of *can-raising* in English sentences of the type *I can't seem to find my hat*. Since this rule applies only to cases where *can* means 'be able',

and not to cases with the epistemic modal meaning 'possible' (**This can't seem to be true*), it must be sensitive to this semantic difference (Chomsky 1972: 108-9). This is, in Chomsky's words, "an otherwise unmotivated complication". McC reacts (p. 59) by saying: "it cannot be simply the drawing of the distinction that Chomsky is calling an 'unmotivated complication'; evidently he finds it an 'unmotivated complication' only if it appears in a syntactic rule."

A further factor is Chomsky's insistence that rules of semantic interpretation have a "filtering function" (p. 109), in the sense that the rules of syntax will allow for structures which under any reasonable criterion must be considered unwellformed, but which will then be "filtered out" by the interpretation rules. The latter thus have to prop up the syntax in determining wellformedness. Chomsky speaks of "ungrammaticality" when structures are rejected by whatever syntax he is considering at any given moment, but of "deviance" when the rejection is (supposedly) done by the semantic rules. In the absence of any independent criterion for "determining in advance what the factual domains of 'syntax' and of 'semantics' are, any restriction on 'syntax' can be met simply by calling rules that violate it 'semantic', if 'semantic' rules are left unconsidered" (McC p. 72). On p. 89 McC demonstrates a consequence of Chomsky's position in this respect: since for Chomsky the semantic rules filter our violations of gender agreement, there is no reason why they should not do the same for violations of person, case, and number agreement. This would brand as grammatical (but deviant) sentences like:

- (1) a. **Le plume de mon tante sont sur le table de mes oncle.*
- b. **Omnibus Gallia sum in tria partes divisum.*

But even if we take Chomsky literally, and let the semantic rules filter out only violations of gender agreement, it would follow that Italian syntax would be happy to assign to (2a) the structure of (2b) and vice versa:

- (2) a. *Nelle città italiane antichi palazzi abbondano.*
(In Italian cities ancient palaces abound.)
- b. *Nelle città italiane antiche palazzi abbondano.*
(In ancient Italian cities palaces abound.)

It would be easy to carry on in this vein, but we would then do nothing but shadow McC's discussions of all the various points. Suffice it to say that, when judged on their merits, Chomsky's theories and analyses sadly collapse. Under McC's guidance we witness a succession of faulty or defective observations, inconsistencies, misunderstandings of issues, tendentious phraseology, misrepresentations of issues and positions, non-sequiturs.

In the second paper in the book under review McC takes a close and competent look at Jackendoff's (1972) treatment of anaphora and in particular VP-anaphora. McC shows himself as sympathetic as possible, trying to avoid false generalized oppositions, such as the false opposition between a 'generativist' and an 'interpretivist'

approach. McC's argument is that if Jackendoff's semantic interpretation rules are to do the work they are supposed to do, then either they are themselves rules of syntax or the syntax must at least be supplemented with rules that Jackendoff wishes to ban from it. Thus, a VP-deletion rule would have to be reinstated, but under conditions of syntactic *and* semantic identity with the antecedent VP.

The discussion as a whole makes for very pleasant reading. And although I would be inclined to declare McC the winner (but then I am obviously open to a charge of partiality), I have the definite feeling that the debate has lost some (but certainly not all) of its relevance, due to a lack of insight into what lies beyond the rules and the semantic representations. After all, the semantic representations of both McC and Jackendoff are themselves *linguistic* objects, which therefore have their own meaning. It would of course be circular to try and specify *those* meanings again in semantic representations, and one is thus left with the uncomfortable conclusion that all that has been achieved so far is a paraphrase from surface structures into some hopefully analytically significant language of semantic representations. But the phenomenon of meaning itself remains obscure. Here and there, McC makes noncommittal references to notions developed in so-called formal semantics, but the support coming from possible world semantics is clearly insufficient. While defending his VP-deletion rule (and the same goes for rules like Gapping and other forms of coordination reduction), McC appeals to a criterion of 'semantic identity'. Yet it is not clear what this is meant to involve. Even if we take the notion of semantic representation in as wide a sense as Jackendoff does, so that it includes a constituent structure tree with references to the lexicon, a coreference table, a 'modal structure' (specifying scopes of operators), and a topic-comment structure, - even then the semantic identity criterion is not clear.

In the case of VP-deletion, coreferentiality between NP's in the antecedent VP and NP's in the deleted VP is generally not necessary:

- (3) Harry knew the answers, but Bill didn't.
- (4) Harry cut himself, but Bill didn't.

In (3) the answers Bill did not know may be those Harry did know, but they might just as well have been different answers to different questions. The natural reading of (4) involves Bill not cutting himself, not not cutting Harry. Both Jackendoff's and McC's discussion of, in particular, VP-deletion is based on the assumption that, as a rule, coreferentiality is required, or at least identity of variables (manifesting themselves as pronouns or as zero), as in (4), or:

- (5) Harry wants to leave, but Bill doesn't.

where the subject of *leave* in the antecedent VP is *Harry*, but in the deleted VP *Bill*. This assumption is, however, erroneous, even for cases where the antecedent VP contains an 'ordinary' referring pronoun, as in (6), amply discussed by McC:

- (6) Fred got Sally to kiss him, but Sue refused to.

Both Jackendoff and McC take it as a condition for, respectively, anaphora and VP-deletion that what is involved is Sue refusing to kiss Fred. This, however, only appears to be so, probably for pragmatic reasons. If this were a condition, then a text such as:

- (7) Fred left the party with Sally, and Bill with Sue. Both wanted the girl they left with to kiss them. Now Fred got Sally to kiss him, but Sue refused.¹

would have to be marked as deviant, ungrammatical, unwellformed, or whatever label one wishes to use for rejects of the theory. The same applies to cases like:

- (8) Harry drives a Cadillac, and Bill a Volkswagen. Now Bill takes good care of it but Harry doesn't.

(An appeal to 'sloppy identity' will, of course, not do for such cases.)

In general it seems to be the case that referential identity, though not excluded *per se*, is not at all required for VP-deletion, - except, of course, where referential identity is ensured by independent means, as with proper names (in most cases), or by the express addition of a phrase ensuring strict coreferentiality (as when I add *the same answers, I mean* to (3)). Both Jackendoff and McC, therefore, are in error on this issue. The problem now is that, if we drop their unjustified requirement of coreferentiality (or identity of variables), it is no longer clear what the semantic identity, which is obviously required under some formula, actually involves. Identity expressible in terms of deep structure trees, as in:

- (9) John was amazed at the shooting of the hunters, and so was Bill.

(McC p. 143) is clearly required. But reference relations are unclear. Some 'identity', or at least analogy, seems required, but it is not clear under what formula.

It seems to me that the observational inadequacy we find with both Jackendoff and McC is to be ascribed to a certain myopic pre-occupation with sentence-internal anaphora, characteristic for all discussions on anaphora in linguistics up till very recently. On the rare occasions where external anaphora is mentioned, it is glossed over one way or another. The real reason why external anaphora was never seriously discussed was, of course, the fact that no analytical or descriptive apparatus was available. Nowadays, there is a growing insight that external anaphora is part of discourse semantics, and that any semantic theory of natural language will have to incorporate not only a truth-conditional part but also a part which specifies the role of discourse in semantic interpretation. It is, one would expect, here that sentence-external anaphora would find its place. In any case, the neglect of external anaphora seems to be have blinded linguists, including McC, for observational material where precisely this form of anaphora disturbs the peace, - as in (7) and (8) above, and in those interpretations of (3) and (6) where there is no corefe-

rentiality. It must be said, in all fairness, that McC comes very close to a discussion of discourse factors involved on p. 145-6, where he comments on cases such as:

- (10) a. Frank didn't buy a car, but Shirley did, and she paid \$ 3000 for it.
- b. Frank bought a car, and so did Shirley, and they both cost over \$3000.

He there refers to work by Karttunen, as well as by himself, for further elaboration.

The third paper in this book is on questions of language universals. McC's position in this respect is characterized by the natural point of view that if one speaks of linguistic universals it is useful to look at a variety of languages, including languages or uses of language that make no use of sound as a medium, such as written language use, or American Sign Language. He thus comes to the reasonable conclusion that all linguistic universals are implicational universals, even if some of them may have the trivial implicational form "if x is a language, then ...". He stresses, furthermore, that one should take into consideration quite seriously the possibility that individuals speaking the same language have nevertheless different competences. If universals are genetically determined, as Chomsky has it, then that means that there is room for individual variation within the universal categories, just as humans are genetically determined not only to have two ears but also to differ from individual to individual in the size and other morphological details of those organs. He also puts out a warning against the unwarranted use of proposed universals whose only function is to prop up theories or analyses which are otherwise "extremely implausible" (p. 160). He makes it clear that the simful use of putative but in fact totally unsupported 'universals' is characteristic of the Chomsky school of linguists.

The really interesting aspects of this paper, however, all have to do with specific issues. Among these, some are to do with phonology, others with grammar. I shall concentrate on the latter.

The most notable feature in McC's discussion of grammatical universals is his insistence that it makes sense to consider the theory that there is no left-to-right ordering in deep structure. McC does not present this point of view as a clearly formulated and testable theory. Rather, one has the impression that he is toying with the idea. The idea is that the structural relations necessary for a proper functioning of the transformational rules are all expressible in relational terms (subject of, object of, etc.), more or less as proposed in work by Postal and others in the mid-1970s. McC would like to be able to establish that no transformational rule ever makes crucial use of order in its underlying structures, although a complete or partial ordering may be imposed on the output structures. This raises the question of how and when and under what constraints order is introduced into the structure trees, and this question is not answered.

McC realizes that his proposed route is littered with mines. He

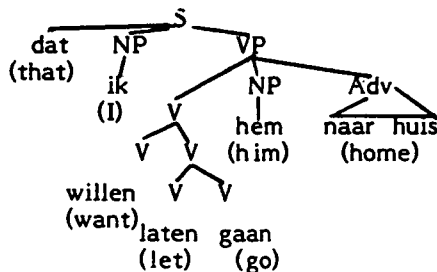
mentions (p. 167-8) Bach's (1970) splendid argument for underlying VSO-order in Amharic, despite the surface SOV-order (the theory McC puts a feeble protest up against on p. 164-5, as I mentioned before). Bach's argument rests on the behaviour of a couple of morphological elements in Amharic which are affixed to whatever happens to be the first word of an NP except when the NP contains a relative clause. In that case the morphological element is affixed to the verb of the relative clause, which comes last in that clause. Since Amharic relative clauses precede the head noun, the hypothesis of underlying verb-first order re-establishes the generalization that the morphological elements in question are simply affixed to whatever happens to be the first word in the NP. If the NP contains a relative clause, that first word is the verb. A later transformation then shifts the verb to the far right of the clause, and it takes its affix with it. This neat theory is lost when we postulate that a transformational rule may never make use of underlying order.

Other cases abound. Thus, the English sentence (11a) has a variant (11b):

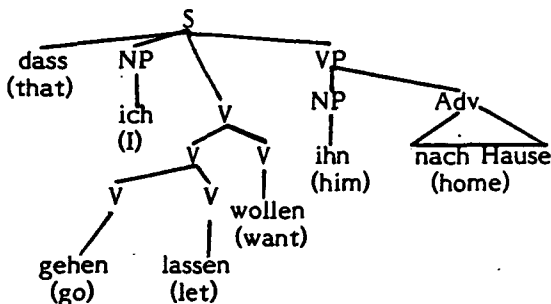
- (11) a. He didn't post the letter until four o'clock.
 b. Not until four o'clock did he post the letter.
 c. *Until four o'clock he didn't post the letter.

If we wish to keep up the reasonable idea that (11a) and (11b) are transformationally related, it cannot be a relation expressible purely in terms of constituents, without an appeal to fixed positions of elements in the underlying structure, since *not until* is not a constituent in (11a). Another case is provided by the by now well-known rule of Predicate Raising (also called Verb Raising) in German and Dutch. This rule, which is most easily demonstrated for subordinate clauses, is associated with certain verbs (which are therefore marked as such in the lexicon) which take an embedded S as one of their arguments. The rule is cyclic and lifts the verb from the lower clause to sister-attach it to the verb of the clause whose cycle is on, under a newly created identical category node (the 'mother'). German and Dutch are virtually identical in this respect, except that Dutch has right-attachment and German has left-attachment, so that repeated application of the rule yields mirrored orderings in both languages. (12a) gives a Dutch derived structure (before V-Final), and (12b) gives the same for German:

(12) a.



(12) b.



Both sentences mean: '... that I wanted to let him go home', assuming past tense. In the final surface form of the Dutch and German sentence, the V-cluster is shifted to the far right:

- (13) a. ... dat ik hem naar huis wilde laten gaan.
- (13) b. ... dass ich ihn nach Hause gehen lassen wollte.

The point here is that the rule of Predicate Raising (one of the neatest and most successful transformational descriptions available) would be badly complicated if the order of the elements involved were to be left out of account, as the careful reader will be quick to detect.

McC rests his case mainly on the ways verbs are placed in Dutch sentences. He refers to Koster (1974, 1975), who defends the theory that Dutch has underlying SOV-order (contrary to what I have assumed in (12a)). Koster argues, to McC's and my mind successfully, against underlying SVO-order, but he fails to take into consideration the hypothesis of underlying VSO-order, which I favour. McC's point of view is that Koster's analysis and his arguments can be fully upheld, even improved, if we assume that Dutch has unordered underlying structures. He even goes so far as to provide a (summary) description of Dutch verb-placement without any appeal to underlying order, using exclusively features of constituent structure (McC p. 168-70). This description is directly based on that given by Koster, and it seems to carry some conviction. I nevertheless believe it to be wrong. Perhaps it is worth our while to take a closer look at this issue.

McC takes the following observations from Koster:

- (14) a. Jan dacht tijdens de pauze aan zijn vader.
- b. Jan dacht aan zijn vader tijdens de pauze.
- (both: Jan thought of his father during the intermission)
- (15) a. Piet zei dat Jan tijdens de pauze aan zijn vader dacht.
- b. Piet zei dat Jan aan zijn vader dacht tijdens de pauze.
- c. Piet zei dat Jan tijdens de pauze dacht aan zijn vader.
- d. Piet zei dat Jan dacht aan zijn vader tijdens de pauze.
- e. *Piet zei dat Jan aan zijn vader tijdens de pauze dacht.
- f. *Piet zei dat Jan dacht tijdens de pauze aan zijn vader.
- (all: Piet said that Jan thought of his father during the intermission)

Two odd phenomena ask for an explanation: first, allowing for movements of constituents in main and subordinate clauses, why should it be that (14b) is fully grammatical whereas (15e) is clearly ungrammatical? If there is a rule of V-Final in subordinate clauses, this looks anomalous. Secondly, if V-Final is optional, as would appear from (15d), then why should (15f) be so clearly ungrammatical?

Koster's explanation is that the verb comes last in underlying order, and that there is an optional rule moving a prepositional constituent on the left of V to the far right, moving cyclically through the VP. Thus, from the underlying:

- (16) NP[Jan] VP[PP[tijdens de pauze] VP[PP[aan zijn vader]
V[dacht]]]

we may get the variants in (17):

- (17) a. [Jan] [tijdens de pauze] [dacht] [aan zijn vader]
b. [Jan] [dacht] [aan zijn vader] [tijdens de pauze]
c. [Jan] [aan zijn vader] [dacht] [tijdens de pauze]

but not those in (18):

- (18) a. *[Jan] [aan zijn vader] [tijdens de pauze] [dacht]
b. *[Jan] [dacht] [tijdens de pauze] [aan zijn vader]

Koster takes the word order of Dutch subordinate clauses as defined (in part) by this system. In fact, (18) corresponds with (15e, f). In main clauses he assumes an extra rule moving the finite verb form (*dacht*) to second position, which yields (14a, b) via multiple paths.

McC now observes that Koster's explanation is simplified if we assume no ordering at all, except the requirement that in main clauses the finite verb comes in second position and that in subordinate clauses the subject-NP comes first. (Other surface ordering constraints are left out of account.) Within the highest VP of (16) all orderings are admitted, provided the constituent structure given remains unaffected. This solution works well enough for the simple examples given, but, unfortunately, it fails to work generally.

I have three arguments to offer against McC's rephrasing of Koster's description. First, the facts are different when the verb has one or more nominal arguments:

- (19) a. Jan las tijdens de pauze het boek.
b. Jan las het boek tijdens de pauze.
(both: Jan read the book during the intermission)

- (20) a. Piet zei dat Jan tijdens de pauze het boek las.
b. Piet zei dat Jan het boek las tijdens de pauze.
c. *Piet zei dat Jan tijdens de pauze las het boek.
d. *Piet zei dat Jan las het boek tijdens de pauze.
e. Piet zei dat Jan het boek tijdens de pauze las.
f. *Piet zei dat Jan las tijdens de pauze het boek.
(all: Piet said that Jan read the book during the intermission)

Note that the sentences of (19) and (20) are exact counterparts of those of (14) and (15), respectively. Let the underlying structure be as in (16):

(21) NP^[Jan] VP^{[pp[tijdens de pauze] VP^{[NP^[het boek] V^[las]]]}}

If McC's description were correct, (20c, d) should be grammatical, and (20e) should be ungrammatical. (20e), in particular, violates the constituent structure of (21), and should be inadmissible, whereas it is clearly wellformed. Notice that the ungrammatical sentences all have the verb *las* precede the direct object *het boek*. It thus appears that order is relevant when nominal constituents are involved in the VP. The same goes for cases of greater lexical coherence between the verb and some PP, as in *in de rondte draaien* (turn round and round) or *op z'n plaats zetten* (put in its place):

- (22) a. Jan draaide tijdens de pauze in de rondte.
 b. Jan draaide in de rondte tijdens de pauze.
 (both: Jan turned round and round during the intermission)
- (23) a. Piet zei dat Jan tijdens de pauze in de rondte draaide.
 b. Piet zei dat Jan in de rondte draaide tijdens de pauze.
 c. *Piet zei dat Jan tijdens de pauze draaide in de rondte.
 d. *Piet zei dat Jan draaide in de rondte tijdens de pauze.
 e. *Piet zei dat Jan in de rondte tijdens de pauze draaide.
 f. *Piet zei dat Jan draaide tijdens de pauze in de rondte.
 (all: Piet said that Jan turned round and round during the intermission)

(Note that (23e) is ungrammatical.) One of the things that seem to be at work here is a criterion of 'degree of lexical coherence': the greater the degree, the stronger the ban on V preceding the PP (or other constituent category).

Secondly, McC's description fails to account for the fact that not all grammatical sentences are equally acceptable or unmarked. In (15), for example, there is a descending degree of preferability from (a) to (d).

Thirdly, some orderings are topic-sensitive. If instead of the PP *tijdens de pauze* in (14) and (15) we take the adverb *gisteren* (yesterday), we get the same grammaticalities and ungrammaticalities, but in all cases where *gisteren* stands in final position it must be entirely unaccented, i.e., pronounced with a flat low tone, indicating that what happened yesterday is (part of) the topic under discussion. If we assume definite movement rules (which must involve underlying word order), we can make these rules sensitive to topic-comment distinctions, at least in principle (since no formal account of this distinction has yet been made available). But if no such rules are formulated in the grammar, the topic-sensitivity of certain word orders requires the formulation of surface patterns or filters specifying under what conditions sentence-final adverbs must be (part of) the topic. Such a procedure would not only be cumbersome, it would in all probability result in a taxonomy of the facts that would be captured much more

elegantly and with much greater explanatory power in terms of a transformational movement rule system.

It should be noted, in this connection, that Koster's description itself is susceptible of serious criticism. Without going into the numerous and intricate details of Dutch word order, it may be observed that any system which takes the final position of the verb as basic will have to account for the possible alternative positions (in subordinate clauses) by the invoking of movement rules of one sort or another. This, however, creates the anomalous situation that sentence forms resulting from minimal rule applications would also rank highest on the scale of stylistic preferability, whereas application of the available rules would result in a deterioration of the sentence's quality. No matter how the rules of grammar are to be interpreted or considered to be implemented in brain systems, a grammatical description with this property must be considered counterproductive. This is easily seen when we go back to the sentences (15a-d). As has been said, there is a descending degree of preferability ranging from (a) to (d). The most preferable form is the one where (1) the verb is final in its immediate VP, and (2) that VP stands to the right in the higher VP. Violation of condition (2) results in a small decrease in preferability; violation of condition (1) is more serious. In (d), both conditions are violated. A system with verb-first in underlying structure would avoid this anomaly: there application of optional rules will improve the quality of the sentence.

Furthermore, there is the behaviour of Dutch clitical pronouns. In main clauses they are seen to gravitate towards the verb form, whereby the neuter object pronoun *het* (it) stands closest to the verb, the neuter prepositional object pronoun *er* and the other pronouns take later positions:

- (24) a. Ik gaf Karel het boek. (I gave Karel the book)
 b. Ik gaf *het hem*. (I gave it him)
- (25) a. Ik liet het boek altijd graag op de tafel liggen.
 (I always preferred to let the book lie on the table)
 b. Ik liet *het er* altijd graag op liggen.
 (I always preferred to let it lie on it)

The phenomenon of the verb attracting weak (clitic) pronouns is, of course, very widespread in European and other languages. The verb seems to act, in these languages, as some sort of structural pivot or rallying point for weaker elements to attach to. If we now accept the theory that underlying form in Dutch has verb-final word-order, as in the subordinate clauses:

- (26) a. ... dat ik het boek altijd graag op de tafel liet liggen.
 b. ... dat ik *het er* altijd graag op liet liggen.

we face the undesirable consequence that Dutch clitics cannot now be said to gravitate towards the verb, since the verb stands at the far right in (26), and, according to this theory, has never been moved. Yet the clitics (*het*, *er*) stand between the subject (*ik*) and the time ad-

verbial (*altijd*). A theory which has the verb moved from its end-cyclic position after the subject, leaving behind the orphaned clitics, would avoid this awkward consequence.

Finally, there is the point that Dutch and German do not behave at all like verb-last languages. In particular, both languages are essentially prepositional, and not postpositional languages. They also have normal question word fronting, unlike SOV-languages.

It seems to me, therefore, that the best theory is one that gives Dutch a deep structure VSO-order, which is transformed during the cycle to end-cyclic SVO (NP - VP), whereupon postcyclic rules move the verb (in main clauses only the non-finite parts of the verb) to the right under a variety of conditions.² It would be presuming too far upon the reader's patience to try and specify all the manifold and in part unexplored complications of the conditions under which the rule or rules in question are optional or obligatory.³ But it is clear that the underlying word-order for the verb-movement rules is too important to be dispensed with. It is clear, furthermore, that constituent structure can be 'broken into', as we see in (20e),⁴ so that the, in itself very elegant, proposal made by McC must, unfortunately, be considered inadequate.

In the final paper of the book under review, McC argues not so much for the nonexistence of syntactic categories (this is just one of McC's titular quirks) as for the thesis that deep structures are constrained by being built up from a very limited number of categories (probably just S, N, V), and that the multiplicity of surface categories and semicategories is the result of the rules of grammar transforming the deep structures into surface structures. That is, it is McC's view that transformations not only change tree structures from the point of view of constituent structure, but also from the point of view of node labellings. He considers (p. 199-200) the possibility that syntactic categories are not 'basic' in any sense but manifestations of bundles of more basic factors. He admits, however, that this view remains speculative.

The article latches on to the discussions that have taken place in the past about the identity, at some level of analysis, of the categories NP and PP, AP and NP, auxiliary verb and main verb, etc. He then passes on to a consideration of what makes a syntactic category. Here he mentions the function of a constituent in logical structure as one criterion (p. 185), and lexical category as another (p. 186). This latter categorization is probably constrained by structural factors. Thus, McC observes that "while a V in English can have up to two NPs as sisters, only a highly restricted set of As (for example *like* and *worth*) allow even a single NP sister, and combinations of an N with even one NP sister are excluded altogether" (p. 186). Morphological features, as well as grammatical relations count in determining categoryhood, and also the rules of grammar that are lexically associated with particular lexical items.

My impression of this paper is that it falls somewhat short of the usual standards of vigorous and systematic presentation McC's readers have come to expect. This is particularly regrettable since the question

of syntactic categories changing systematically through a transformational derivation is of great interest and badly in need of further competent research. It would have been to the point and also very enlightening if McC had pointed to cross-linguistic category differences, as he did in McCawley (1973: 282), where he mentions the interesting fact that in Finnish negation is a surface verb. Such facts can be multiplied at will. English adverbs, for example, find greatly divergent expressions in other languages. Sometimes a verb:

- (27) a. Harry has just left.
 b. Harry vient de partir. (French)
 (lit: Harry comes from leaving)

sometimes a separate clause, or some other 'creative' rephrasing:

- (28) a. I have been reliably told that the earth is flat.
 b. Ik heb uit betrouwbare bron dat de aarde plat is. (Dutch)
 (lit: I have (it) from (a) reliable source ...)

We also find the opposite, where English uses a verb but another language an adverb:

- (29) a. Gerald likes to get up early.
 b. Gerald staat graag vroeg op. (Dutch)
 (lit: Gerald willingly gets up early)

Adjectives and verbs, as is well-known, often serve as recipient categories for semantically identical material in different languages, and sometimes even within the same language (cp. English *squint* and *be cross-eyed*, or Italian *zoppicare* and *zoppo*, both meaning 'limp', or, for that matter, the English word *limp*).

Historically, there is a great deal of evidence that words change category. Usually they move from verbal to adverbial or prepositional status. This evidence is strongest in the case of Creole languages (see, e.g., Seuren 1983) and languages with serial verb constructions.

This whole fascinating area of category shift and reanalysis is rather badly underexplored at the moment. Universalist linguistics will be of great help here, and we may expect some useful results given the recent tide of universalist linguistic studies. But we would also like to have some insight into the universal constraints that can be formulated within the terms of a theory of grammar whereby deep structures containing only the categories 'noun', 'verb' and 'sentence' correspond with surface structures containing a multiplicity of categories and semicategories ('squishes'). A great deal of research is still needed here, at least if it is agreed that such a theory of grammar is worth developing on independent grounds. For if it were not, the research proposed would be a shot in the dark, and the chances of hitting a good one among the thirty million theories of grammar floating around in dark grammatical space would be very slight indeed.

Notes

- 1 I must say that I prefer to leave out the final *to* in this case, although I have no explicit account of this preference. However, the presence or absence of *to* in VP-deletions is not an issue in the discussion between McC and Jackendoff.
- 2 For an exposé of the cyclic rules mapping underlying VSO on end-cyclic SVO (NP - VP), see Seuren (1983), in particular p. 240-248.
- 3 For one thing, the rules are clearly topic-sensitive, as has been noted. This also appears from the fact that (20e) is possible only if the book in question is (part of) the topic under discussion. In (20e) the topic is rather what happened during the intermission (or what Piet said that happened during the intermission). If the book in question is (part of) the topic in (20b), then the word *las* (read) must be heavily (almost contrastively) accented, carrying the burden of being the comment expressed in the sentence.
- 4 It is possible, and as far as I can see by no means absurd, to propose that there is a difference in underlying structure for the verb-movement rules according to whether *het boek* is a topic constituent or belongs to the comment expressed in the sentence. We might think of a structure where *het boek* is a fixed VP-constituent when it expresses the topic, and that it takes part in the game of verbal musical chairs when it is part of the comment.

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