

are very grateful to Schmalstieg for having made available his thinking on Old Prussian in the form of a photomechanically reproduced typescript.

M. GROSS, M. HALLE, AND M.-P. SCHÜTZENBERGER, EDS. *The Formal Analysis of Natural Languages; Proceedings of the first international conference.* The Hague, Mouton, 1973. Pp. viii, 388. f80.00.

Reviewed by FRED W. HOUSEHOLDER, *Indiana University.*

There are two things to be noted at the outset about this book.

(1) It's the publication of a set of papers presented at a conference in Rocquencourt (in France) in the spring of 1970. It's difficult to be sure when the mss were sent to the printer; three of the authors cite works of their own dated 1973; but this they could have known a year in advance. Of the seven 1972 items cited, 5 are also the authors' own, but the other two look like genuine references to items already in print. Of the ten items cited from 1971, several bear *also* the notation "to appear." Offhand, it looks as if three or four authors made some additions or corrections in proof, but otherwise the manuscripts were printed as of 1971. So, even when the book was printed, the stuff was two or three years old; now, as I write, it's four or five, and when you read this, it'll probably have aged another year. Nevertheless, several points are raised that are still worth talking about, so I will say something.

(2) The title might conceivably suggest a session devoted mainly to mathematical linguistics (or the mathematics of linguistics), with at least a side glance at computational linguistics. In fact, three papers (constituting Part six) out of 21 are devoted to mathematical linguistics, one of which might have a little link with computer work, though all three authors (as well as at least six of the others) have been in part supported at one time or another by computer projects. Several of the papers in parts 1-5 have something to say about notation, or use words like 'formalism,' but hardly more than any collection of 18 papers in any good linguistic journal.

Several of the papers are thorough, carefully organized, well presented (e.g. Schane's, Emonds', Postal's, Ruwet's, Dixon's, Gross's,

Mehler's, Joshi's); others give the superficial appearance of improvisations (e.g. Halle's, Lightner's, Ross's, Kiefer's) or are summaries of (or extracts from) longer works published elsewhere (e.g. Browne's, Lees', and perhaps Peters'). The two best pieces, in my opinion, are those by Maurice Gross ("Remarques sur la méthodologie de la grammaire générative transformationnelle," pp. 251-64), which is a strong plea to linguists to improve the character of what they offer as evidence (and several of the other contributors should have heeded his plea), in particular by exhaustive checks on the properties of lexical items (check *all* the items for *all* the relevant properties), and Jacques Mehler ("Some remarks on psycholinguistic studies"), which is a very clear and well-balanced survey of research up to 1971, plus a report on two of the author's own experiments (accusateur-accusé and lancer-recevoir).

Since, except for occasional links between pairs of papers (e.g. Postal's style of argument with Gross's criticism, or Dixon and Gross on "voluntary" or "active" subjects and objects, Brown and Rohrer on [$\pm$ specific] noun phrases), there is nothing in this book that calls for a unified discussion of many or all of the papers, I will proceed to comment on them one by one.

The first paper (of Part one: Phonology) is by Morris Halle, and is entitled "A new organon, The formalism of phonological rules." The ostensible point of this paper is that the only explanation why children can "learn the language without difficulty and with astonishing rapidity" is that they are born with the types of rules and schemata, and the general metagrammatical arrangements of *SPE*. In fact, of course, he offers no new evidence in support of this conclusion, but instead proposes some slight modifications of three of the stress rules of *SPE*. This he introduces with the remarks "One of the most surprising discoveries that Chomsky and I made ... was that the stress in a large class of English words was totally predictable from the phonetic form of the word. That is, given the sequence of consonants and vowels that compose the word, the location of the stress in the word can be determined automatically." A similar form of words appears in *SPE*, of course. But neither here nor there is there any discussion of the alternative. The *SPE* procedure starts with a large set of underlying vowels, both Tense and Nontense, and uses the features of these vowels (along with the syllabic structure of the word and the presence of consonant clusters) to predict the accent. Neither in *SPE* nor here, however, is there any discussion of the traditional procedure, to start

with a small set of underlying vowel letters and with the position of the accent or accents, and from them predict the tenseness etc. of the various vowels. It is worth noticing that some quite nice generalizations are possible in the latter case which cannot be extracted with any ease at all from the *SPE* rules.

Halle states the general principles of the *SPE* solution as follows (p. 3, (3)):

- (a) The stress is on the antepenultimate vowel if the last vowel is nontense, and the penultimate vowel is nontense and followed by at most one consonant.
- (b) The stress is on the penultimate vowel if the last vowel is nontense and the penultimate vowel is either (i) tense, or (ii) followed by two or more consonants.
- (c) The stress is on the last vowel if the last vowel is tense.

The traditional view can be roughly cast in the same mold:

- (a) before -CC(-) a stressed vowel is short;
before -(h)V-, a stressed vowel is long. Otherwise
- (b) If stress is on the antepenult, or pre-antepenult, then that vowel is short unless it is *u*, and the penult vowel is reduced. But if the next two syllables are -i/eV (C) (mainly -ea, -ia, -ion, -ious, -eous, -ian) then the stressed vowel is long, unless it is *i*. (The quality of the last vowel involves special rules, as in *SPE*.)
- (c) If stress is on the penult, before a single C it is long, and the last vowel is reduced. (This, of course, does not cover words like *tribute*, *venom*, *medal*, *Paris*, etc.)
- (d) If the stress is on the last (single) vowel, before a C, not followed by "silent e," it is short, otherwise long.
- (e) Double vowels (ee, oo, au, aw, ou, ow, ew, eu, ey, ay, oy, ei, ai, oi and igh) in a stressed syllable represent "long" vowels or diphthongs.

Now these rules do not work perfectly, but on one class of words, *complete* Latin (or Greek through Latin) words—largely names—, it works almost perfectly. ("Complete" meaning that no suffix has been dropped or altered.) And for that class of words we can add these rules for final (always unstressed) syllables:

- (f) A final vowel is always long unless it is *a*, which is reduced.
- (g) Before final *-s*, *e* is always long, *o* is short, the other vowels reduced. Of course there are numerous exceptional cases; but all or nearly all of these cases are exceptions also for the Halle formulation (indeed *SPE* spends a fair number of pages on them).

For the class of complete Greco-Latin words, accent is predictable by the Latin rule, provided that we know the ancient quantity of

penultimate vowels followed by single C's (or certain stop-plus-liquid clusters). No other vowel qualities or quantities need to be known. But a fair number of such words have had "unhistorical" accentuations accepted and established (e.g. *metamórphōsis*, *ábdōmen*, *duódēnum*, *gladiólus*—in fact almost all words ending in -olus, -ola are treated as if the o had been long). Incidentally, even a system with underlying accents can predict some, by a statement like this: "All unmarked words are accented on the penultimate vowel if -CC- follows, otherwise on the antepenult."

However, it must not be thought that all the *SPE* rules have vowel quality on the left (or in the environment) and stress on the right; several work the other way. In at least four rules, the direction is quality to stress (or unstress): (1) the Main Stress Rule, given as (15) in the résumé on pp. 238–45, (2) Laxing Rule I (Auxiliary Reduction I), rule 20, I, (3) Final Weak-stressed [o], rule 21, and (4) Auxiliary Reduction II, rule 24. Six rules go the other way: (1) Auxiliary Reduction I, (rule 20, I), listed above, mentions both stress and quality on both sides, so must be counted here, too; (2) Laxing Rule IV (vowels in antepenult and before certain suffixes), rule 20, IV, (3) Tensing Rule I (the rule for *strange* etc. and *lounge* etc.), 23, I; (4) Tensing IV (for vowels in penults and before -ian etc. as in my rule b above), 23, IV; (5) Vowel Shift, rule 33; (6) Vowel Reduction, rule 43.

One peculiar result of the decision to subject Greco-Latin words and Anglo-French words to the same set of rules is that all instances of traditional "long u" (unlike long a, e, i, o, which agree with *SPE* tense vowels) are considered as either underlying lax, or as laxened by rule, so that underlying tense u's can become [aw]. From a historical point of view, this can happen only (or almost only) before -nd-, -nt- or -ns- in French loan-words. But since this source is French nasalized -õ-, and since this in turn arises either from Latin short u or long or short o, the first question that strikes me, is "How many -ou-/u- ([aw]/[ʌ] or [yu-]) pairs are there?" There seem to be four, plus two uncertain ones: *abound/abundant*, *profound/profundity* (and *found/fundamental*, *foundry/fusion*), *pronounce/pronunciation* (also *de-/an-/e-*), and perhaps *doubt/dubious*, *dubitable*. Conceivably one could drag in also *tower/turret* and *house/husband*. Now if we ask how many -ou-/o- ([aw-]/[ɔ-]/[a-]/[ow-]) pairs there are, the answer seems to be at least six good ones, and maybe another six or seven, several of them good linguistic words: *noun/nominal* (also *pro-*, *ad-*, etc.); *sound/sonant*, *sonorous*, *sonic* (also *re-*); *compound/component*, *com-*

pose, composition (also pro-, ex-); astound/astonish, pound/ponderous (pound is assigned underlying -ū- in *SPE*); mountain, mount/montane, etc. (mountain is also given underlying -ū- in *SPE*); counterpoint/contrapuntal (the mention of the latter word in *SPE* 181 seems to be concerned with the third syllable only), and the prefixes counter- and contra- in general; count/comptometer; fountain, fount/fontanelle, font, and maybe council/reconcile, conciliate. Note that the -pound verbs (ultimately Latin pono) and astound have to get a redundant -d tacked on. Some verbs from -pono also yield -pose or -pone. There are undoubtedly more to be found with the aid of a dictionary. We may wonder why Halle and Chomsky chose [aw] (and later also -au- [-ɔ-]) to be the normal tense mate for u [ʌ], [yu], [yɔ]. (so far as I know, -au- hardly ever alternates with -u-, except in a handful of implausible Latin pairs like cause/accuse, recusant.)

All this may seem as if I'm reviewing *SPE*, not this article by Halle. However, it's almost impossible to separate them; essentially the article is a fulsome eulogy of certain rules of *SPE*, which are here given with little or no change (except to omit various hedges and restrictions): these rules are the Main Stress Rule (given in *SPE* as rule 15 on p. 240), the Stress Retraction Rule (given in *SPE*, I think, as the Alternating Stress Rule, rule 17 on p. 240), two Compound Stress Rules (essentially incorporated in Rule 18 on p. 240). As McCawley says in his comment (11-12), the treatment here, though vague, does seem to indicate a different solution for constructions like Madison Avenue, which are noted in *SPE* on p. 156. The discussion there suggests that these (and John Smith, Senator Jónes, and iron box) should be bracketed as NP's, not N's, in the Smith-Trager manner. Halle's discussion on p. 8 leaves me puzzled, even after McCawley's exegesis, and his listing aluminum utensil on p. 6 with compounds (stressed on the first member) suggests some strange dialect. For normal speakers of English, aluminum behaves like iron, stone (mentioned in *SPE* p. 156) and other material words, as if it were a sort of adjective, hence forms an NP, not a compound. (Indeed "this utensil is aluminum" or "this floor is stone," "this box is iron" are also OK constructions.)

Halle closes (p. 11) with the child language learner, and how "the formal constraints I have sketched above"—(apparently restrictions on the conflation of rules) make it possible for children to learn their native language.

The next article, by T. M. Lightner ("Remarks on universals in phonology") also reads like an oral presentation, with many interest-

ing suggestions scattered all through. He starts out (13) with the suggestion that "there is a set of universal phonological rules, one of which nasalizes vowels in some environment." Here the problem for me is with "there is." Does he mean to suggest that the only satisfactory explanation of widespread tendencies is to assume a special wired-in set of rules transmitted genetically in the human brain? This seems a most unnatural explanation for what are, in the main, rather natural phenomena.

Note 13 on p. 21 says "there seems to be a general tendency to shift e to o before dentals" and gives as one example the Latin shift before l (e.g. in volō). But it is normally assumed, and quite naturally so, that it was the velar quality of this l (i.e. the hollowing of the tongue with raising of the dorsum in its articulation), and not the dental (or perhaps alveolar) placement of the tongue-tip which was responsible. Forms like velle have an l which is every bit as dental, but which is usually supposed to have been 'light' (i.e. with relatively flat tongue).

In note 16, on p. 24 Latin coquō is phoneticized as [kʷokʷō], and it is alleged that [kokʷō] is wrong. No evidence is given for this, and it's difficult to imagine what evidence there could be. Certainly the word coque ("cook!" as vocative of coquos or imperative of coquō) is virtually always (see *Thesaurus* s.vv. coquo, coquus) distinguished in writing from the word quoque ("also"), (though the variant cocere which L. wants for Spanish appears as early as Priscian and Pseudo-Probus, and once on an inscription). Presumably all c's were somewhat labialized or rounded before o, but apparently they were still distinguished from qu's, much as in English coat vs. quote. Before [u], on the other hand, there is no evidence of such a distinction, either in Latin or English.

An interesting feature of Lightner's article is the words of praise that occur occasionally for earlier linguists, e.g. on p. 29 "insightful remarks of Jakobson" and "Meillet ... his usual perspicacious comments," and, on p. 38 "Beríniste ... the most profoundly original study in Indo-European since <Saussure>."

The remark on p. 30 that "in most dialects word final obstruents are voiceless," citing maze and mace, seems to be made without argument or evidence. Certainly on the hundreds of spectrograms I have examined, of speakers from many parts of the United States (and one or two from England), this is not so. In virtually all examples of final s (or s in cluster with p, t, k, or intervocalic s, for that matter), the voicing is sharply stopped before the noise begins, whereas before final z that is not the case; if voicing is not continued to the end (and it

usually is not in utterance final position), it does go on for at least one or two centiseconds.

The note (29) on p. 37 which remarks "IE or Semitic, which have *NO* underlying vowels" should have offered at least a little explanation. Possibly he means "no inherently syllabic systematic phonemes," since there seem to have been words like *mūs ("mouse") in IE which had a syllabic (here [u]) in every surface form, and there may well have been pairs like Greek *gónos* (*génos* in late IE, where ablaut variants that had once been predictable were so no longer. Of course, with the aid of abstract segments of the right sort, any language can be given an underlying phonology without vowels.

On p. 39 (and earlier on 26) Lightner mentions the Latin rule which changes *nevolumus to nōlumus, amāvisse to amāsse, and conventiō to cōntiō, which he gives as

$$w\tilde{V} \rightarrow \emptyset/V(n) \text{ — } V(\{i^N\})V$$

There seems to be some sweeping kind of misprint in the environment statement here (since none of the examples will suit it), but it's beyond me to determine what L's actual formulation was. Certainly the phenomenon is not restricted to antepenultimate (or earlier) syllables of words of four or more syllables, nor must the deleted short vowel be followed immediately by another vowel, nor must that vowel, in turn, be separated from the next vowel by nothing, nasal or /s/. But whatever the correct formulation, it does not seem to be in the least 'crazy' (as L. calls it on 39), since it consists of two very common processes

$$(1) w \rightarrow \emptyset/V \text{ — } V$$

(Intervocalic w—in fact y should also be included—drops, optionally or obligatorily, under various conditions), and

$$(2) VV \rightarrow \bar{V}$$

(Consecutive vowels contract to one long vowel—or, to adopt another formulation mentioned by L. on p. 16, one assimilates completely to the other—again under a variety of conditions, yielding sometimes the lengthened form of the first vowel, as in amāsse, sometimes the second, as in nōlō, all quite regularly.) I am at a loss to know what is crazy about either of these, both of which are well-established Latin rules, needed anyway, and also quite common rules in languages in general.

The use of VV for $\bar{V}$ is mentioned again on 42, with a reference to Foley, and the remark "several unexpected consequences result." It

is curious that L. does not even allude to the enormous literature of the "structuralist" era dealing with this topic: "Of three possible representations of length (1) single segment, (2) two identical segments, (3) vowel segment followed by non-identical segment (as in, e.g., /ā/, /aa/, and /a:/), which is best?" or "Which is best for language X?" There must be at least 20 or 30 articles which deal with this in part, and four or five devoted entirely to it. He does cite Kenstowicz (1970) and Pyle (1970), but never even mentions the name of Trubetzkoy.

On p. 45 Lightner adds a paragraph which he calls a personal communication from McCawley, mentioning a suggestion of Drachman's about Twana. L. then adds "the proposed explanation ... strikes me as being correct." On p. 49 the same paragraph (plus one introductory sentence at the beginning and a reference to Bantu at the end) is printed as "Discussion," followed by L.'s response: "McCawley's comments, interesting as they are, do not seem to bear on the question. ..."

McCawley's piece "On the role of notation in generative phonology" is also discursive, like the first two, but does bring up a number of genuine defects in *SPE* notation. Once in a while he makes a statement without justification which leaves us a bit dubious, e.g. (54)

"There are hundreds of languages in which all words ending in vowel plus three consonants plus vowel plus two consonants have penultimate stress."

Since, even in English, such words are rather rare (and, of course, don't always have penultimate stress—*dístrict* but *distráct*), and therefore require some searching, one wonders how many languages M. has checked to make him so sure that they number in the hundreds. On the next page he says

"no language has a rule assimilating nasals to the place of articulation of labials and only labials."

This is even more intuitional and anti-empiric, since it must imply that M. has personally checked every human language. If he hasn't, then this is a prediction, which must be based on some evidence which he fails to give.

On p. 58 he says "at least vowel height and pitch height should be represented by single non-binary features, rather than by combinations of binary features," a view which many (including myself) have favored for some time.

In the proposed reformed notation on p. 39 (item (10), last line), where he has said that F and G will range over 0, 1, and 2, he never-

theless writes $F(n) \rightarrow -G(m)$, presumably because he assumes that $-0 = 1$ and $-1 = 0$, and on the next line he says "A rule cannot have an 'if'-part containing $F(n) = -$ and a 'then'-part containing $F(n) = -G(n)$ and $G(n) \rightarrow -$." This must be a slip; $-$ should be replaced by 0 or 1 (it's not clear which from note 4).

I also like the remark (61) "evaluation measures, which in my opinion can never be put on a sound footing" or (62) "the completely barren notion of 'evaluation measure.'" This piece of McCawley's is well worth reading and worth thinking about.

Schane's piece (the formalization of exceptions in phonology) is a well-organized and instructive treatment of some rules for French phonology. The heart of it is a proposal (67) "Any deviations are not to be built into the rules themselves, since this will only lead to a proliferation of bad rules, but instead should be separately stated as a *SET OF CONSTRAINTS* on how the rule is to be applied." This essay is the last on phonology.

The four papers of Part Two (by Emonds, Moore, Postal and Ross) deal with English syntax superficially, and syntactic theory indirectly.

Emonds' paper ("Evidence that indirect object movement is a structure-preserving rule") is concerned with defining verbal particles (e.g. up, back, together, etc.) as intransitive prepositions, so that general statements involving both prepositional and particle constructions will be easier. The case is clear and well-argued. I have only a few observations. On p. 78, the examples of 'right' asterisked in (11) are of course all O.K. in American dialects, especially of 50 years ago or so. The word is discussed in Bolinger 1972, pp. 51-2. And it not only modifies prepositions, but also adverbs and other expressions of place and time, in spite of what Emonds attributes to Klima:

- (a) Do it right away/off: (These are formally E's intransitive prepositions, but in this use they are exclusively temporal.)
- (b) I want the money right now.
- (c) It was right here/there a minute ago.
- (d) Finish that right this minute!
- (e) Go right home. (Conceivably "home" counts as an intransitive preposition for E, since it surely patterns like a verbal particle.)

E could still save his generalization by claiming that a preposition has been deleted from examples b, c, d, and e (if he doesn't want 'home' as a preposition itself).

When "in" and "out" are used as particles (intransitively) they seem to be equivalent to "into<the house, the building, the tank, etc.>" and "out of<the house, etc.>", and this continues to be true

when they are used with "objects" like "window" and "door." So "the cat jumped out the window" seems to be underlain by something like "the cat jumped out<of the house through>the window."

I think a good case could be made for saying that most particles are not inherently intransitive, they have just suffered object deletion (under recoverability from situational context, as a rule). Sometimes the object, when present, requires another preposition.

The next paper, Terence Moore's on "Focus, presupposition and deep structure" is not so terrific itself, but elicits an excellent comment from McCawley. Moore's general frame for deep structure is case grammar, plus some unexplained mechanism for assigning "the focus feature" called [F] on p. 93, [+F] on 96. Later (unformulated) rules will use the feature to trigger certain rearrangements as well as mark intonation peaks. (Evidently [+F] resembles other people's [+E] or emphasis feature.) One rearrangement is Dative Movement, which for Moore changes focus; i.e.

Did John give the book to Bill?

and

Did John give Bill the book?

without special stress on any element will differ in focus (here he is, in part, following Chomsky 1971). This seems very implausible to me.

On p. 96 he says "it is not possible for an OBJECTIVE constituent that has been pronominalised to be also marked for focus:

John gave $\begin{cases} \text{Bill it [+F]} \\ \text{it [+F] to Bill} \end{cases}$."

But, of course, this is a special peculiarity of the word "it," not shared by any other pronouns (e.g., here, *them*, *me*, *you*, *us*, *himself*, or whatever you want to consider a pronoun). Probably the most performance-oriented solution would be to have a realization rule which says that if any instance of "it" comes under emphatic stress by any rule, it must be at once changed to "that."

McCawley, in his comments, points out the relevance of tense to the acceptability of certain answers to Chomsky's original questions, and further claims (1) that simple past tense forms in English are anaphoric, and "refer to an event," whereas present perfect forms "assert the existence of an event," and (2) that yes-no questions are derived not merely from pairs of alternative S's of which the second is the nega-

tion of the first, but from pairs which may differ in various ways, yielding the various differences correlated with presupposition, focus, or emphasis. This makes all such "focused" elements essentially contrastive.

The most puzzling piece in the book is Postal's "A global constraint on pronominalization" (100-132). Postal includes a large bunch of sentences, about 35 of which are marked with asterisks. Of those 35, there are perhaps four that ordinary English speakers might object to; the other 31 are quite clear, grammatical sentences. Postal concedes that the constraint he is concerned with is "dialect-particular," but he offers no evidence to suggest (a) that he has correctly represented his own dialect, or (b) that anyone else speaks such a dialect. This is more or less what Bar-Hillel 1971 (and see also Botha 1973) refers to as the "my dialect-your dialect gambit" and which Gross in his contribution to the volume under review castigates with the following words (256, n.4): "entièrement à rejeter, s'il n'a pas été possible de définir ces dialectes de manière indépendante." It may well be that Postal is entirely honest in his judgments, but we have no check. What is to prevent any one of us from making up some elegant constraint, new rule, rule-ordering paradox, or the like, and then constructing a set of sentences with all the right ones starred to support the new proposal? If "my dialect" means anything I want it to mean, nobody, least of all Postal, can possibly object.

Postal never says what his asterisks mean, but they cannot mean (1) "I can't understand this sentence," since he shows us the correct interpretation every time, or (2) "I wouldn't say it like this," since he never offers us any alternative wording that he would accept, or (3) "People don't say this," since he acknowledges that many people do. About the only possibility left is "I would never want to express the idea conveyed by this," which may be O.K. for some of them (since, as in all such arguments, extremely implausible situations must be dreamed up), but will hardly do for all. And, in any case, such a judgment seems to be irrelevant to a linguistic argument, since it is the underlying idea, not the surface expression of it, that is being rejected, obviously for semantic or cultural, not syntactic reasons. And even if we assume that he really means (2) above ("I wouldn't say it like this") and is correct in saying this, we are given no reason to suppose that the basis of his objection is syntactic rather than esthetic or cultural. No doubt his answer is that he can offer a syntactic formulation which will correctly "predict" his asterisks. But the same type of

formulation can readily be given for dozens of esthetic (or "stylistic") preferences. (The sentences whose asterisking Postal is intent upon explaining are sentences like his (2)(a):

Who_i did Mary say his_i mother yelled at? in which the pronoun (here "his") has an interrogative or part of an interrogative phrase preceding it as antecedent, or else a relative, as in (3)(a).

The child_i who_i Mary said his_i mother yelled at was crying. Both of these are asterisked by Postal.)

There are four "appendices" to the paper, which raise a variety of points. One or two merit some comment. In appendix A Postal rejects solutions which depend upon the use of transformations which add syntactic features, syntactic dummy segments, or numerical indices, on the ground (as far as I can see) that these are mere notational variants of Global Derivational Constraints. Then in footnote 25 he cites Lakoff (1970) and Casagrande (1970) for the problem of Ancient Greek case agreement, saying that it is "undescribable in terms of such features" because "there is no bound on the number of relevant subject-participle pairs which have to agree in a Greek sentence," and, he forgets to add, which are separated by extraneous material (since, obviously, no index is needed for an adjacent subject-predicate pair). Now this is partly a restatement of the familiar Chomskyan doctrine that there is no upper bound on the length of sentences, except a "performance" bound. In practice, of course, the actually occurring maximum number of subject-participle or subject-adjective pairs in a single Greek sentence which would need indices is fairly low—two, or perhaps three, at most. (A hasty check through Xenophon's *Memorabilia* I reveals none requiring two, even.) And his rejection of requiring "linguistic theory to provide an unbounded set of integers as features" seems to be based on the argument "The degree to which [this approach] is mistaken thus hardly needs stress." But he hasn't yet told us why it's mistaken, or what the degree of error is, or why it is that bad, or indeed in what way it is bad at all. Certainly it's not difficult in theory to provide an unbounded set of index markers; if you can envisage a sentence of unbounded length, it is clear that the number of indices will be far smaller than the number of words in the sentence. A very simple way of providing necessary markers is by means of a kind of C.S. rule whereby a given subject is assigned a feature [+_____dikai-neuter pl.] say, and that predicate, in turn, gets a feature [+taut-neuter pl._____]. And something can be said for this machinery as corresponding to what goes on during per-

formance. Speakers don't match up subjects with predicates (or heads with modifiers) by means of numerical indices.

The reason why Lakoff, Postal, Casagrande, and the rest feel sure that some innovation is necessary (preferably Global Rules) is that in traditional transformational theory (whether interpretivist or generativist) the relation of subject to predicate can only be defined in terms of contiguity under an S-node, and the (generally or partially) derived relation of modifier to head (in a NP) can also be defined only in that way, whereas languages like Greek are full of 'discontinuous' S's and NP's, held together by agreement instead of contiguity. The discontinuities, as usually interpreted, involve rules like Subject Raising (to subject or to object—raising to object is allowed to remove the NP from under the S-node even if it remains contiguous), Relative Fronting and Interrogative Fronting, and other shifts which are sometimes explained as Scrambling. Familiar beliefs about the ordering of these rules relative to others also affect the picture. A theory, for instance, which said that no order-changing rule could separate subject from predicate or head from modifier until after the last case-assignment rule had applied would, if it could be shown to be workable, solve the problem at once. Closely related would be systems using unordered bases or systems in which subject-predicate and head-modifier were marked otherwise than by contiguity (with bracketing). These two relations (Jespersen's nexus and junction) are of great importance in all languages, and particularly so in languages where form rather than contiguity is the primary surface indication of bracketing. And, as has been noted, a theory which disallows transformations such as Passive, Flip, and Tough Movement would have little trouble with Greek.

The agreement rules involved in steps 1–5 of the Lakoff/Casagrande example (apparently supplied to Lakoff by Avery Andrews) are all straightforward subject-predicate agreement, except for the rule which goes from *toutois ha* at stage 4 to *hois* in stage 5 (the ultimate sentence: *emmenomen hois ismen legomenois dikaios einai* 'We abide by what we know is said to be just'). This is a very special rule, peculiar to Greek and restricted so that it may only (a) change an accusative relative pronoun to genitive or dative, or (b) rarely, change a genitive to dative or a dative to genitive. There is frequently a head or 'antecedent' noun, which is not deleted, but placed after the relative clause, e.g. *emmenomen hois ismen legomenois dikaios einai nomois* 'we abide by the laws which we know are said to be just.' Thus the whole structure is

like a typical Greek NP with *hois* functioning as a definite article, and *nomois*, the head N, at the end. There are many more problems raised by this example, e.g. (1) how are supplementary participles (here *legomena/legomenois*) to be derived? (2) how are circumstantial participles and articular participles (none in this example) to be derived? (3) Which agreements are optional? Cf., for instance, this sentence from Xenophon's *Memorabilia*: *hōn prosthen apeichonto kerdon aischra nomizontes einai, toutōn ouk apechontai* 'They do not refrain from the forms of money-making which they had previously refrained from because they thought them base.' Here one might expect *aischra* to agree with *kerdōn* (or else, like English, to have *auta* 'them' inserted for it to agree with). But it doesn't. Because of a rule that subjects of infinitives after verbs of thinking, saying etc. are in the nominative (or Equi-deleted), a conjoining of such a subject with a non-identical one leads to unlike cases being conjoined: *nomizontsi spheis kai tous epikourous dikaios einai*. 'they think that they (nom. pl.) and their allies (acc. pl.) are just (acc. pl.).' I don't believe the rules implied in Lakoff and in Casagrande are the correct rules. Nor is the familiar formulation of Scrambling correct; certain arrangements are clearly aimed at, not mere randomness. But the weakness in traditional transformational theory to which this example points is genuine, and remains no matter how the rules are formulated, though some tinkering with order relieves much of the problem. A subject and predicate which belong together must always be identifiable (except for cases of genuine ambiguity, which is possible in Greek as well as English). (This topic has been rehashed a number of times; it is perhaps worthwhile looking at Lakoff 1972, Andrews 1971, and Fauconnier 1973. I am indebted to Roger Higgins for calling my attention to these and other items.)

Appendix B refers back to Postal (1970) on 'Crossover' constraints, and contains a reason for preferring a Global Constraint account, namely that the Crossover solution requires 'language and dialect-particular conditions on particular transformations' whereas a Global constraint can be either present or absent without altering the rules. This seems a dubious reason for the preference, particularly when the thing to be accounted for has such a dubious status. Certainly few linguists are afraid of being language-particular and dialect-particular.

Appendix C considers a third possible solution, which is clearly *not* possible for any real or imaginary English dialect, as he in fact concludes.

Appendix D considers the case of a Harlem dialect in which the sentences

(a) I am asking who he punched

and

(b) I know who did he punch

are both ungrammatical. Since I do not believe this is a correct account of the dialect, I don't believe Postal's explanation of it is correct. In any case, it does involve him in what seems to be a dialect-particular constraint on a particular rule (Subject formation), the very thing he objected to in Appendix B.

Ross's article called *Slifting* (133-169) deals with sentences ending in what he calls parentheticals, such as *I feel, we realized, it seems to have been believed, remember, don't you think*—and apparently also *said John, John said*, etc. deriving them by a process of deleting an embedded S (with or without "that") from its position after these expressions and reattaching (by Chomsky-adjunction) to the left, without "that." This account seems plausible, certainly more so than the only other one mentioned by Ross, which derives these parentheticals from some sort of restricted S expansion of an underlying *Adv* which somehow modifies the main S. But in the course of presenting arguments good and bad for this analysis, a few interesting points arise.

Neither Ross nor anyone else (to my knowledge) says anything about the various types of embedded S's without "that" which begin a sentence. In fact, it is often claimed or implied that initial embedded S's obligatorily have "that." A very common exception is the expression "is all" as in

Max is a Martian is all.

The intonation is identical to that in Ross's Slifted sentences, i.e. after the main S, pitch drops very low and then may rise slightly to the end. But this one clearly has the embedded S in its deep structure position, as subject of "is all," so cannot be derived by Slifting. Another common type is reverse pseudo-cleft sentences, i.e. those in which the clause beginning with "what" is last, not first:

Max is a Martian is { what I feel.
 what seems to have been believed.
 what I say.

Some people would feel happier, perhaps, with quotation marks around "Max is a Martian," but the same observation applies to many of Ross's examples (Ross never uses quotation marks in the entire article). This may perhaps have something to do with the sequence-of-tense facts discussed in R's third argument (139-142 and 148) and the pronominal fact represented by example (61) on p. 155. What these show is that almost this entire class of structures belongs to the phenomenon known among literary scholars as the Free Indirect Style—this also applies to Postal's Black English "request for information" indirect questions (which, incidentally, are perfectly grammatical in Standard English, literary English, and most varieties of spoken American or British English known to me), such as

I asked would John come.

(Cases like this are avoided in print because of hesitancy about putting a question mark. In the 19th century and earlier, question marks were the rule; nowadays they are avoided because this seems to be *indirect* discourse.) The primary effect of most uses of Free Indirect Style (including the Slifting use) is to remove a statement from the category of things categorically asserted by the speaker on his own authority. This goal is also accomplished by ordinary indirect discourse. The difference in the case of Slifted sentences seems to lie in the after-thought effect; the speaker starts out boldly, but then decides to pull in his horns. (The characteristics of Free Indirect Style are these, for people who are unfamiliar with the form: (1) secondary sequence is maintained throughout, in general; (2) pronouns are shifted to the point of view of the uppermost speaker (the "I" of the top "I hereby say to you" that Ross assumes); question word-order is as in direct speech; (3) imperatives are replaced by modal constructions—"he was to go," "he should go," etc.; (4) the various "Root Transformations" of Emonds are unaffected; (5) higher verbs, such as "he thought," "he said," etc. may be inserted here and there parenthetically, but are not needed.)

The argument about restrictions on persons (148-50) seems to be not very direct, and (moreover) to be concerned with anomalies which are not really grammatical anomalies. Here one of Grice's rules would seem to be in point (I have to quote this at second hand and from memory): if A asks a question of B, then A must be assumed to have less knowledge of the answer than B. In most of Ross's examples this possibility is either excluded or unlikely ("Am I hungry?"), although

for some it is not ("Is that touching me?" could certainly be asked in cases where "that" can neither be seen nor felt by the speaker, but can be seen by the addressee). Conversely, many second person assertions presuppose that the speaker knows the addressee's internal states and thoughts better than the addressee. Of course such sentences can freely occur in the context of a second person narrative (as, e.g., a director explaining a part to an actor, or a hypnotist building a situation for his subject—"You go to a strange town and meet a man named Aristotle in a café. You talk with him for an hour or two. Aristotle is rich, you realize. etc. . ."). So, again, the oddity cannot be grammatical. The same objection applies to some of the examples in the argument from *so*-pronominalization. The example (no. (56) p. 153) "Rufus is flatulent, and his wife thinks so, too." is odd mainly because there is a difference between the degree of knowledge presupposed by "wife" and by "think," and also because of the relative pointlessness of offering an assertion of less after an assertion of more. With the order reversed, the sentences tend to be O.K. "His wife thinks Rufus is flatulent, and he is, (too)."

Another argument is related to what Ross calls "sloppy identity." In fact, there is nothing sloppy about it, and it is quite regular and often obligatory, and is perhaps not even linguistic. Tell a class "When I raise my right hand, each of you must do the same thing," and you can only mean "raise *your* right hand." But if you simply say "Do what I do," and then hold up your right hand, everyone will either hold up his right hand or else (because of the face-to-face mirror effect) his left. No one will come up to you and lift your hand. In short, reflexivity is in part inherent, and cannot be derived by rules of pronominalization.

Wayles Browne's paper (173-4) on [specific] and [definite] in the interrogatives of various languages partially repeats material published in Browne (1970a and 1970b), which raises an interesting point. If the non-human "what?" can never be [+specific, +definite], this is perhaps linked to the fact that things seldom have unique, individual names; but it is a little hard to see why the human "who?" can never be [-specific, -definite]. It should not be forgotten, either, that, though "who?" normally excludes a [-human] answer, "what?" does not exclude a [+human] one.

Lees' paper (175-9) on the Turkish copula re-raises an old notion of Turcologists, that all verbal forms in Turkic languages are nominal, hence take copula "be" inflection. While it is true that *most* forms are

nominal, several are not: (1) the imperative, e.g. *gel* "come," (2) the definite past, e.g. *geldi* "he came," (3) the conditional, e.g. *gelse* "if he comes." Lees himself mentions the imperative, but any copula derivation of the other two also involves difficulties.

The paper by N. Ruwet (180-202) dealing with French pronominal constructions, particularly reflexives, strikes me as an admirable piece of work, though I am not sure that he makes a perfect case against deriving various manner adverbials from higher predicates.

Dixon's discussion of "The Semantics of Giving" (205-223) presents a number of good ideas for semantics in general, of which the main one is the notion of "nuclear" and "non-nuclear," nuclear words being those of generic meaning in terms of which other, more specialized words are invariably explained. (A related notion of semantic reductionism appears in Householder 1971.) McCawley's comment at the end, that sometimes the "nuclear" term is missing, well fits the case in Japanese, where (according to Kumiko Takahara-unpublished and Kuno 1973) there are four verbs corresponding to *give* (*ageru*, *yaru*, *kudasaru*, *kureru*), differing in one dimension as [$\pm$ honorific], and in the other as [$\pm$ in-group] or the like. One also wonders a little at the omission of Bendix (1966) from Dixon's Bibliography, since Bendix does (on pp. 61-76) treat "give" along with *find*, *get*, *lend*, *borrow*, *take*, *lose*, *keep*, *get rid of*, two of which are also treated by Dixon.

The major piece in the book, at least for my money, is Maurice Gross's "Remarques sur la méthodologie de la grammaire générative transformationnelle" (251-264). One of the minor disputes of recent years has been between linguists who are accused of being "data-oriented" and those who are said to be "theory-oriented." Obviously, of course, this is the ancient anomalist (=data-oriented) vs. analogist (=theory-oriented) dispute, and equally obvious, now as in antiquity, is the fact that no valid grammars can be written without the help of both data and theory. But there are times when it is salutary to have our attention called to the value or importance of a particular class of data. This is what Gross has done primarily; secondarily he calls our attention to the fundamental worthlessness of a certain kind of evidence (as was remarked above in connection with Postal's paper). Everyone should read this whole article with great care; I will here content myself with quoting a few passages. From p. 255: Une étude faite au Laboratoire d'Automatique Documentaire et Linguistique et portant sur 5,000 verbes (soit la totalité des verbes français) a permis de recenser les verbes qui avaient la propriété (α)-(β) [namely "ac-

tive" and "voluntary"], avec une fidélité remarquable; l'accord entre locuteurs est quasi unanime (à quelques verbes près, peu utilisés et dont le sens précis est mal connu). As an old hand at collecting classificatory judgments about vocabulary items (see Householder et al., 1964 etc.), I confess I am a little surprised at the reported unanimity, although perhaps semantic judgments of this sort tend to be more consistent than purely syntactic ones. But I have to admire anyone who is not willing to be satisfied with less than "la totalité des verbes français." That is right. As Gross says on 256, with regard to Kiparsky and Kiparsky (1970), "il est en effet possible de dresser une liste complète des verbes anglais qui acceptent une complétive [indeed that's what we tried to do in Householder et al. 1965]. . . . Il serait donc très facile de donner une base empirique sérieuse à cette étude, au lieu de la laisser ainsi à la merci d'un contre-exemple etc. . . ."

From p. 258: "Une telle présentation laisse supposer que la propriété est très générale, ce qui est presque toujours faux. . . . les présentations que l'on trouve habituellement dans la littérature donnent à croire que les propriétés (e.g. le passif) sont très générales; en fait les distributions des propriétés sur le lexique sont souvent très asymétriques."

After a reference to Chomsky's lexicalist hypothesis: "Notre position va plus loin: considérant que les transformations ont toutes des 'exceptions,' nous pensons que cet état de fait est dans la nature même des transformations, et qu'une de leurs fonctions consiste à différencier les éléments de lexique, c'est-à-dire à les classer en éléments subissant une transformation donnée et en éléments ne la subissant pas."

From p. 259: "Nous sommes en mesure de formuler des assertions du type suivant: Les verbes qui ont la propriété *P* sont $V_1, V_2, \dots, V_m$, et *IL N'EN EXISTE PAS D'AUTRES*. Ce type d'assertion qui nous semble indispensable en linguistique est nouveau en syntaxe; il est pourtant souvent possible de le formuler."

"La matrice [this is the score sheet with a line for each of 3000 verbs and a column for each of 110 syntactic properties] ne possèdera pas deux lignes identiques. Autrement dit, deux verbes quelconques n'ont jamais leurs propriétés syntaxiques identiques, sauf peut-être pour quelques classes productives." [These turn out to parallel similar productive classes in English, e.g. *whisper, groan, shout, grunt, moan*, etc.]

P. 260: "Alors *M* [a set of synonyms grouped together] considéré comme morphème (abstrait) unique possède un comportement très régulier." In note 10 he gives as an example the French "verbe ab-

strait *M* appelé *TO KNOW*, et nous montrons qu'il possède les deux réalisations morphémiques $M_1 = \textit{savoir}$, $M_2 = \textit{connaître}$." This obviously resembles Dixon's proposal with McCawley's revision (the abstract verb *TO KNOW* is "nuclear") which we discussed above, and almost implies that *savoir* and *connaître* constitute a single suppletive verb. P. 263: "Par contre, si l'apparition de certaines propriétés syntaxiques est une conséquence mécanique de l'apprentissage de certains éléments de sens, le problème de l'acquisition du langage devient moins mystérieux."

Kiefer's paper "Morphology in generative grammar" (265-280) takes up a problem rarely dealt with by transformationalists (though Peter Matthews 1972 is an excellent non-transformationalist attempt to cope). He adds (266) another inadequate effort to a long line of inadequate attempts to distinguish inflection from derivation: "Y will be considered an *INFLECTIONAL ENDING* if it does not affect the semantic content of X, and a *DERIVATIONAL ENDING* if it does." So in Greek the stem *Pele-* (meaning Peleus, a man's name) adds the suffix *-ides* (meaning "son of") to yield *Peleides* "son of Peleus." Has the semantic content of *Pele-* been affected? I cannot see how. But *-ides* is surely a derivational ending.

Note 5 (271) makes two interesting points: one is a reversal of more conventional derivational directions to derive "the book was read by John" from "the book was read John," and "the book which John bought" from "the book John bought." The other is to say that "morphology" should cover the treatment of all sorts of minor morphemes, not just affixes, including "rules that determine the order of certain morphemes" and "various types of readjustment rules."

Jacques Mehler's "Some Remarks on Psycholinguistic Studies" (296) is a good account of the state of the art in 1970 or so, with a report on several experiments undertaken by Mehler himself with various collaborators.

One or two minor points. On p. 301 M. says "it is easy to find another [sc. honest psycholinguist] who claims that ambiguous sentences are easier to understand than nonambiguous sentences, irrespective of awareness." This I cannot understand, and would like to know the name of one such psycholinguist. Similarly on 302: "the knowledge of a speaker about his language might have no relationship at all with the processes that account for language behavior." If he said "no influence on his language behavior," I could agree, or if he said "the ideas of a speaker about his language" I could agree, but that such

knowledge, which must by definition be true, should be totally unrelated to facts about anyone's language behavior (not just the knower's own behavior) seems impossible. However the statement on the next page (303) seems more reasonable: "there is no sensible manner in which one can state that mental grammar is in some way a set of operations that follows closely the steps that are indicated in linguistic grammar to derive sentences." Somewhat similar is this on 304 "it is ... natural that linguists ... will tend to find more than one manner of formally characterizing sentences, and therefore alternative ways in which the generative component of the model can be conceived." (And he means also "equally valid" manners and ways.) Bravo! I've been saying this for years, but getting laughed at for it. A little less controversial is the remark on 309: "the position holding that in performance one and only one procedure is available and universal is incorrect. That this is so can be demonstrated by the following few arguments."

The last three papers in the book are concerned with what one might have (from the title) expected the whole book to deal with, mathematical linguistics in the narrower sense. Joshi's paper (329-61) deals with a number of interesting points about Mixed Adjunct Grammars (which are very similar to Dependency Grammars) with Transformational components, where an entity called a Trace is made use of. Since nobody, at least no linguist, who lives very far from Philadelphia has any interest at all in Adjunct Grammars (or Dependency Grammars), which are generally (and regrettably, I think) looked upon by linguists much as Manicheanism was regarded by the early Christians, there is not much point in discussing this paper here, even if I were competent to do so.

Kuroda's paper (362-71) is concerned with supplementing the Chomskyan notion of weak and strong equivalence of grammars with the ideas of hierarchy-homeomorphism, hierarchy-continuity, and hierarchy-equivalence. Again, I doubt if most linguists give a damn.

Peters' "On restricting deletion transformations" (372-84) is almost as bold as Postal's paper in making decisions about grammaticality, but in the opposite sense. Where Postal asterisks a bunch of ordinary English sentences, Peters accepts as normal sentences like his (18) on p. 370:

"Their sitting down's promising to steady the canoe's threatening to spoil the joke purports to follow the script." I am not sure what the restrictions are on Poss-Ing (to use the familiar nomenclature), but I

do know that by and large only persons appear in the Poss slot here, and that even a simple example like "The house's having collapsed was distressing" is totally unacceptable to many people, and that I have never even encountered one example in all the billions and billions of English words I have read (and heard) where a whole Poss-Ing nominalization is made the Poss of another. If Peters can show me some examples antedating 1969, I'll believe that they exist, but frankly I doubt it. There are always better, clearer and easier ways to say whatever such a construction might be intended to mean.

However, Peters defines the term SURVIVOR PROPERTY as follows (382)

"A transformational grammar with base component B and transformational component T is said to possess the *survivor property* if every transformational derivation $t_1, \dots, t_n$ with respect to T such that B generates t_1 satisfies the following condition: if ϕ is the input domain of any cycle to $t_1, \dots, t_n$ and ψ is the output from that cycle, then ψ contains more terminal nodes than any subpart of ϕ on which the transformational cycle operated earlier in the derivation." I'm not sure exactly what that means, but perhaps it is of some interest.

In conclusion, I must say that except for Gross' article I would hesitate to recommend that anyone buy this book. But, if you buy it for that, then there are a few other things worth at least a passing glance.

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4. Punctuation. (a) Use only single quotation marks—never double except for quotes within quotes. This applies to all uses of quotation marks without exception. If the second of a pair of quotes stands at the same point as another mark of punctuation, the quote precedes unless the other mark is itself part of the quoted matter: The word means 'cart', not 'horse'. He writes, 'This is false.' Does that mean 'You heard me'? It means 'Did you hear me?' (b) Never use quotes to enclose a word or phrase cited as a linguistic example; see §6. (c) Words containing pre-fixes are written solid, without hyphens, when no misreading will result: antimentalism, contradistinction, extra-systemic, prevocalic, semivowel, subdialect, superstock. The prefix is followed by a hyphen when the next element begins with a capital: non-Germanic, pre-Greek. (d) Ellipsis is indicated by three periods, close set, with a blank space before and after, like ... this. Do not add a fourth period even if the ellipsis precedes or follows the end of sentence. (e) Use no comma after the expressions e.g. and i.e., and do not underscore them. (f) Use a comma before the conjunction that joins the last of a series of three or more coordinate items: A, B, and C; X, Y, or Z.

5. Footnotes. (a) Footnotes are numbered serially through the article or review, or through one chapter of longer work. (b) The footnote reference number is a raised numeral following the word or passage to which it applies; it is not enclosed in parentheses, and is not followed by a parenthesis or a period. Reference numbers follow marks of punctuation. (c) All footnotes must be typed (with double spacing) on a sheet or series of sheets following the main text. They must never appear on the same sheet with the text. (d) Each footnote is typed as a separate paragraph, with the first line indented. It begins with its reference number, not enclosed in parentheses and not followed by a parenthesis or a period.

6. Cited forms. (a) A letter, word, phrase, or sentence cited as a linguistic example or subject of discussion appears in italics: the suffix -s, the word *like*, the construction *nich frient*. Do not use quotation marks for this purpose. (b) But cited forms may also appear in phonetic or phonemic transcription, enclosed in square brackets or in slant lines: the suffix [s], the word /layk/. Symbols between brackets or slants are never underscored. (c) Forms in language not written with the Latin alphabet must be transliterated (or transcribed), unless there is a cogent reason for citing them in the original characters. This provision applies to Greek as to other languages. (d) Cited form