
**Introduction**

There is no comprehensive handbook of psycholinguistics. Morton and Marshall's apparent aim is to rectify this situation with their 'Psycholinguistics series', in which they intend eventually "to cover the whole range of psycholinguistics and associated topics with leisurely overviews". As it is in the nature of academic disciplines to develop ever more sub-areas and to expand their contacts with other fields, it is clear that the series could go on for a very long time indeed. This volume is the second, and like the first it contains four very disparate essays. Since many of the essays in the series are certain to prove useful for teaching — advanced undergraduates and graduate students thrive on review articles - it would be of great service to the field if the series were to appear in paperback. The first two volumes are nicely produced, and the second seems to have suffered less serious a publication lag than the first. The more efficient a production line Morton and Marshall can set up, of course, the more grateful psycholinguistics will be.

**Syntax**

The first essay in this volume, Neil Smith's 'Syntax for psychologists', is extremely good value. The psychologists for whom Smith is primarily writing are, one assumes, advanced members of the field who want to talk to linguists. The article is not simplified to the point of undergraduate digestibility, although it is simple in all the right ways. More complicated points (such as a brief account of the rationale of trace theory) are relegated to footnotes, and bibliographical notes provide potential amplification without interrupting the continuity of the text. Smith outlines the development of transformational grammar, the emergence of the "standard theory" and the major challenges (generative semantics) and extensions it has motivated, and adds a cursory consideration of three alternative systems: case grammar, systemic grammar and relational grammar. Case grammar is handled in masterly style: its superficial advantages are displayed and just as smartly exploded. With the other two Smith seems to have pulled his punches a little, although the reader obtains a fair impression of them, including, for instance, the undeniable fact that relational grammar is not easily accessible for outsiders.

In the concluding sections of his essay Smith discusses perceptual strategies and why they do not constitute in themselves an adequate basis for a (performance) grammar and the abandonment of the competence-performance distinction. This section is somewhat skimpy. The most recent attempts to account for acceptability in performance terms — *e.g.* Frazier and Fodor (1978) — were presumably not avail-
able to Smith at the time he was writing, but it is unclear why he failed to mention their predecessor Kimball (1973).

Smith presumably included the perceptual strategies work because it should already be familiar to psychologist readers. But it was not strictly necessary; the strengths of his article lie in its linguistic explanations. Where he discusses psychological research directly, he is on less sure ground — for instance, when he claims (p. 35) that comprehension of sentences containing negatively marked words involves added complexity. In fact none of the published experiments on this topic have used tasks which directly tap the comprehension process, and some have failed to control for length and frequency of occurrence of the critical words. In on-line comprehension experiments with frequency and word length controlled the present reviewer has failed to find any effect of greater complexity due to either implicit or explicit negatives.

In fact the title of Smith's paper is unnecessarily restrictive — subtract a page or so of conclusion and two earlier perfunctory bows to psychological reality, and it is hard to see that 'Syntax for educationalists' or 'Syntax for fishmongers' would have looked any different. Indeed it would be a great pity if educators, fishmongers or other members of the motivated laity in search of a summary introduction to current linguistics were to be deterred by the title, because it is difficult to imagine that they could find themselves better served elsewhere. It is unfortunate that recent work on performance constraints on grammar has already rendered part of the essay obsolete. I suspect that some current linguistic work on alternatives to transformational grammar, particularly the exploration of context-free grammars, will soon have the same effect on other parts. However it is to be hoped that when 'Syntax for non-linguists part II' is warranted, Morton and Marshall call upon Smith to write it.

**Infant speech perception**

Mehler and Bertoncini's essay on this topic is somewhat disappointing in that it is unnecessarily difficult to read. This is because the style is disjointed and also because it is badly proof-read. The most charitable — and of course the most likely — interpretation of this is that it has been written by some very busy people. What, for instance, is the untutored reader to make of "the view that is generally upheld by Haskins" (p. 77)? Who is this fellow Haskins and how did he get into a discussion of an experiment by Fodor, Garrett and Brill? Further enlightenment for the uninitiated comes only ten pages later. Among other annoying errors is a mismatch of /pa/s and /ba/s with VOT on p. 72.

However it would be a pity if such faults led readers to give up on this paper, because it touches upon most of the relevant literature and reports some interesting experimentation by the authors themselves which was previously unavailable to an English-speaking audience. In these studies infants (a) showed evidence of perceiving
a change in a rhythmic series of beats only if the change spanned a certain crucial interval which happens to be the same interval that sets off categories on the VOT continuum; (b) exhibited awareness of a change in a CVC stimulus (i.e. a syllable) but not of the corresponding change in a CCC string; and (c) preferred their own mother’s voice to that of a strange woman but only if their mother was talking normally. Mehler and Bertoncini interpret these results as signifying (a) that categorical perception of stop consonants may follow from our possession (from birth?) of a feature analyzer with a threshold equivalent to the crucial interval involved; (b) that the syllable is the characteristic unit of the infant’s perception of speech; and (c) that the infant may use prosodic cues to speaker identification. This series of experiments is stimulating and could well have been discussed in greater detail, displacing some of the rest of the paper, for instance the rather cavalier treatment of studies of speech perception in animals.

Logogens

The remaining two papers in the book are in a sense related. One is Morton’s definitive description of his logogen model of word recognition, and the other is an attempt by Ellis to bring together data from speech production and from short term memory studies within the framework of Morton’s model. Morton’s description of his model is, without any doubt, timely. In fact it would have been timely at any time in the past few years. Previous published accounts of the logogen model are now many years old, the model has been extended, and a mass of relevant new evidence has accumulated. Instead of referring students to secondary sources for the most up-to-date account of the model, one can now refer them to this paper.

The account which Morton gives of the gradual development of his model accurately represents the process as one of successive addition of boxes and connections to the original simple diagram. However it is disappointing that Morton compares the logogen model only with those models — guessing and analysis-by-synthesis — to which he contrasted his own model when he first conceived it, whereas today its chief rival is surely a search model as proposed by Rubenstein et al. (1970) and most stoutly defended in recent years by Forster (e.g., 1976). Morton confines his response to Forster’s work to a contemptuous aside about the task — lexical decision — employed in much of Forster’s experimentation. For comparisons of the predictions of logogen and search models, one will still have to look elsewhere (e.g. to Coltheart’s useful papers in this area — Coltheart et al. 1977; Coltheart 1978). The reader also looks in vain for a response from Morton to some of the recent findings that are directly at odds with the predictions of the logogen model. For instance, the logogen model explains the effects of context and of word frequency in the same way — less evidence is required to make the response available for a word in context than for a word out of context, and analogously for a high frequency than for a low frequency word. This suggests that the well-known
interaction of context effects with stimulus quality (Meyer et al. 1974) should be paralleled by an interaction between frequency effects and stimulus quality. However, it seems that frequency effects do not interact at all with stimulus degradation (Stanners et al. 1975; Becker and Killion 1977). Morton could well have taken the opportunity to defend his model against this potentially damaging result, but alas, he did not. The note of humility on which he ends his essay is justified.

Speech errors and short-term store

Ellis takes from Morton's model the concept of the response buffer, a pre-output store of words about to be uttered, one of the functions of which is to account for the phenomenon of silent rehearsal. He argues that both the short-term store for phonemically encoded material in memory experiments and the pre-output buffer which figures in most models of speech production (the level at which such speech errors as anticipation, perseveration and exchange of sounds are assumed to occur) have the characteristics of Morton's response buffer, and furthermore that they are the same store. This claim he then proceeds to test by the "error equivalence hypothesis" — the same kinds of error should occur in both spontaneous production and short-term recall. There is a great deal wrong with this procedure. Firstly, it is circular — the "response buffers" in models of speech production (e.g. the phonological string level in Fromkin's (1971) Utterance Generator, or the sound level of representation in Garrett (1975)) were postulated specifically in order to account for the error data, so that no error data can be independent evidence in their favour. Secondly, the establishment of error equivalence does not indicate that the two storage systems are one and the same, since the characteristics of errors in spontaneous speech and in recall may be due to more general properties of human cognition which would be operative in any storage system. Finally, since postulation of a response buffer in no way entails acceptance of the rest of the logogen model, there appears to be little reason for Ellis to have chosen to pursue his hypothesis within Morton's system in particular.

Ellis in fact succeeds in finding support for the error equivalence hypothesis. Many types of phonological error in spontaneous speech (but speech error researchers will note that he has by no means included the whole range of these) prove to have parallels in errors of short-term recall. But the arguments are not very strong. Take for example his claim that the Ranschburg Effect in serial list learning (impaired recall of lists containing repeated items) is analogous to the tendency to omit or replace one of two repeated sounds in spontaneous speech. (The example Ellis gives at this point is in fact a syllable omission error and not a phoneme omission error!) The Ranschburg effect, however, is greater for the second of the two repeated items than for the first, which is not true of the tendency to commit errors on such items. Furthermore, the Ranschburg effect occurs only when there is some distance between the two identical items — when the two
are adjacent, recall is facilitated rather than inhibited. Exactly the reverse is true of speech errors — the likelihood of error increases as distance between the identical sounds decreases; and the most common type of speech error involving repeated items is a haplology, in which the speaker skips straight from the first occurrence of the repeated sound to the material following the second, omitting everything in between, as in the following example from my own error collection:

(1) A large number of words seem to become unique after two syllables. (Target utterance: . . . after two syllables.)

The analogous effect in serial recall would be for a subject to report the digit list 964762 as 962, which does not occur. Finally, linguists since Paul (1880) have observed that speech errors involving omission or replacement of repeated items have an analogy in language change in the phenomenon of dissimilation, in which one of two identical or similar sounds in a word is lost or altered (e.g. the process by which the second occurrence of /r/ was changed in the evolution of English *marble* from Old French *marbre*). One hesitates to speculate on the possibility that this established equivalence might lead Ellis to suggest that language change occurs in the response buffer.

I do not wish, though, to criticise the spirit of Ellis’ enterprise. Indeed, we need many more such attempts to tie speech error data to the results of laboratory investigations of language performance.

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References


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