Psychology, Psycholinguistics and Cognitive Science: Is There a Place for Linguistics?

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1. Introduction

In a paper now nearly a decade old (Derwing 1979), I outlined some of the early history of modern psychology and linguistics. In brief, the origins of experimental psychology are generally traced back to the last half of the nineteenth century, and particularly to the work of Wilhelm Wundt, whose deep interest in language is widely documented. At that time the field of linguistics did not yet exist as such, but there was a co-existent discipline called ‘philology’ that also concerned itself with language phenomena. Although some of the major German figures in these two fields apparently knew and to some extent even interacted with one another, there was little in the way of overlap in the kind of work that they actually did. Specifically, while the psychologists were concerned “to trace the mental processes that precede, accompany and follow utterances” (in the words of one of Wundt’s commentators [Blumenthal 1970:6]), the philologists were mainly preoccupied with the much narrower, complementary tasks of studying genetic relationships among languages and in clarifying the ways in which language forms changed through time.

The emergence of linguistics as an identifiable academic discipline was more-or-less concurrent with the rise to dominance of the strict behaviorist tradition in psychology, that anomalous era when psychologists generally lost interest in the psyche. There is far more than mere coincidence involved here. For to the extent that psychologists lost interest in internal cognitive processes generally at this time, they also lost concern for the special case of language processes. By default, therefore, the investigation of language phenomena fell for a time almost completely into the hands of the linguists, who naturally carried along with them some of the fundamental ideas they had received as part of their earlier philological heritage. The most pernicious of these old ideas, in my view, was the notion that languages were analogous to “living organisms” (cf. Bierwisch 1971:13 and Robins 1969:181), and the corollary that words and other language forms were “things or natural objects with an existence of their own” (attacked in Jespersen 1924:17). Since these early days, in fact, linguistic theorists have largely persisted in the reified view of language as a kind of ‘natural’ object, analogous to a stone or organism, that has an inherent organization or ‘structure’ that can be discovered and even ‘explained’ without reference to those human beings who produce the forms in the first place.

Eventually, of course, the limits of behaviorism became evident, psychologists renewed their interest in the human mind, and it became respectable once again for them to be interested in such questions as how language forms are learned, stored, retrieved and, in general, used. In fact, it became much more than respectable, as a number of influential figures (just like Wundt two
generations before) came to the opinion that human language was likely to provide one of the best windows (if not the very best) on the human mind in general. As luck would have it, however, because of the long period of benign neglect of language throughout the behaviorist era, the psychology of the day had virtually nothing to say about the subject. (In fact, Boring’s famous history of the field [1950], does not even mention the term “language” in its index.) Psychologists were thus forced to turn to linguistics as virtually the sole repository of what knowledge of language there was at that time, and when the term psycholinguistics was first coined in the 1950’s, it was quite naturally anticipated that linguists would play a major, if not central, role in the development of the emerging new science that this term was intended to denote, viz., “the science of encoding and decoding processes in individual communicators” (cf. Koch 1963:248, citing Osgood).

Moreover, on first inspection, the new cognitive psychologists very much liked what they saw, which was a developing model of generative grammar that seemed to possess the appropriate ‘dynamic’ quality needed for the characterization of speech production and comprehension. As shown in Derwing (1979), however (see especially the ‘cake grammar’ outlined in n. 3, p.133), generative grammars are, just as they have always been, models of static language ‘products,’ not models of language processes or ‘recipes’ for assembling those products (cf. Chomsky 1965 and elsewhere, who has repeatedly and vehemently denied the legitimacy of any such procedural interpretations for grammars).

Thus in time the truth came out: linguists (still philologists at heart) were interested in modeling ‘languages’ (or ‘knowledge of languages’, under some accounts), not at all in modeling the psychological activities of speakers or hearers, and the ‘dynamic’ characteristics of the linguistic models that had once seemed so appealing for psychological purposes were exposed as nothing more than a notational sham. By this date, in fact, the disillusionment of psychologists with the formal linguistic approach seems to have become quite severe, in that linguistic models of grammar have been widely judged to be psychologically unrealistic (cf. Carroll 1986:58). 2

The task of describing and modeling actual language production and comprehension thus, by default, fell largely into the hands of psychologists and AI scientists, with results in the past

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1Cf. the parallel swing in the view of the linguistic philosopher, J.J. Katz, who held in the mid sixties that ‘every aspect of the mentalistic theory [of generative grammar] involves psychological reality’ (1964:133), but by the mid eighties had backed off to a fully Platonist view of grammars as ‘theories of abstract objects’ (1984:21) and of linguistics ‘as different from the psychology of language as number theory is from the psychology of mathematical reasoning’ (p. 27).

2While a small number of diehards and apologists still remain (e.g., Berwick & Weinberg 1983) who still hope to salvage the old ‘derivational theory of complexity’ theory by one means or another, the broad consensus today, among linguists and psychologists alike, seems to be that that theory is as dead as a doornail, and along with it the feasibility of the idea that generative grammars, as traditionally conceived, have any likely role to play in psycholinguistic modeling.
decade or so of such dramatic proportions as to place linguistic research in an almost ludicrous position, by comparison. Thus, for example, while generative grammarians preoccupy themselves with such questions as whether or not certain pronouns and noun phrases can in principle refer to the same person (such as the emphasized words in such marginally grammatical sentences as *Near Dan, he saw a snake* and *To him, I spoke in Ben's office*) and to concoct various purely structural hypotheses to account for their own judgments (such issues, in fact, form the empirical and methodological underpinnings of one of the more popular recent developments within generative syntax, known as GB theory [cf. Chomsky 1981]), cognitive scientists have meanwhile moved on to the experimental exploration of what specific semantic, morphological, syntactic, discourse and pragmatic factors determine what the precise antecedent of a pronoun actually is in a given on-line language use situation, which is, of course, the psycholinguistic question of interest (see Smyth 1986 for a detailed overview and discussion, as well as some of the relevant experiments).

In short, linguistics, as it is normally practiced today, is facing a crisis of relevance. As time goes on, fewer and fewer psychologists look to linguists for ideas and evidence bearing on the psycholinguistic task (much less for guidance), and more and more look to their own resources and models, which they can readily understand, interpret and test. The essence of the crisis, therefore, is not simply that psychologists are beginning to ask whether linguistic models are relevant to their work (i.e., language processing in all of its forms, such as word recognition or lexical access, sentence production, discourse comprehension, etc.), but that they have largely finished with the asking and have decided, for the most part, that the answer is in the negative. They may still make use of a few fundamental ideas that were originally developed in linguistics (such as constituent structure, the morpheme and the phoneme), but they generally find current theorizing to be largely incomprehensible, both in content and motivation. As a result, as Cutler has noted, 'psychological research in the service of linguistics has almost disappeared' (1986:162); thus, if there are any new ideas coming out of the field that might prove useful to the larger psycholinguistic enterprise (as I have argued below), it is the linguists themselves who are going to have to make this evident.

Alternatively, linguists may go on as before, blindly talking of the supposed 'explanatory adequacy' of their ideas, but if they end up as the only ones who accept, believe or even understand what it is they are doing, it will be they who are the losers. There is nothing to be gained by beating around the bush: the hard fact is that psychologists can get along perfectly well without linguistics; at worst, they may reinvent the wheel (albeit in a more useful form) and plod along somewhat longer than necessary with a less than optimum cross-linguistic perspective. But, for them, such a step will not be fatal, as the psycholinguistic enterprise is not in doubt: speakers do produce utterances and hearers do comprehend them, and attempts to model such real processes are on solid ground (as the current explosive growth of programs in 'cognitive science' indicates). On the other hand, linguistics cannot get along without psychology, not, that is, if it wishes to continue to bask in the kind of 'cognitive limelight' that it has long since become
accustomed to. The pay-off for linguistics, if there is to be one at all, is to be found in the contribution it makes to the cognitive or psycholinguistic enterprise. (What else, after all, are linguistic theories good for?) In the dreary non-psychological, go-it-alone scenario of so-called 'autonomous linguistics,' therefore, we can anticipate that the field will surely shrivel back into an isolated, narrow and purely taxonomic (i.e., arbitrary) enterprise that generates obfuscatory descriptions of relatively obscure, dying languages, explores their (generally untestable) historical origins, or otherwise occupies itself with esoterica that are of little concern to anyone else in the outside world.

2. On Making Linguistics Relevant

I, for one, however, remain convinced that linguistics (as the only discipline whose practitioners are driven both by inclination and training to look hard at the details of human languages) still has a great deal more to offer than that, but a major reconstruction job is now required to demonstrate this, and, for reasons already discussed, the onus is now on us linguists to make the case.

I therefore have two specific suggestions to offer at this juncture: (1) that linguists make some effort to temper the current free-wheeling theorizing which now occurs in our field by constraints that follow from experimental tests and (2) to demonstrate, once again by experiment, that linguistic theory has some potentially useful raw material as grist for the psycholinguistic mill, and is not just playing arbitrary 'games' with language forms. I will attempt to illustrate each of these possibilities here with examples selected from research recently conducted in our own laboratory, all of which gained impetus from developments in linguistic theories of morphology and phonology.

2.1 Psychological Constraints on Linguistic Theories

One obvious way to improve the case that linguistic theory is relevant for psychology is by demonstrating that linguistic theorizing is itself subject to psychological constraints. This is, in fact, a minimum token of good faith that linguists are willing to participate in the psycholinguistic enterprise, rather than letting their speculations run rampant or even fly in the face of the best evidence of psychological plausibility and accountability. Since linguistic theories invariably arise initially out of attempts to account systematically for regularities exhibited by language forms (i.e., the language product), there is no guarantee, at the outset, that the systems ('grammars') so devised will have any psychological content whatever, or even be readily interpretable in psychological terms, as I have already indicated. Attempts can be made, however, to inject such content and to develop such interpretations, and to test the resulting theories through appropriately designed experiments (see Derwing 1979 and 1980 for an extended morphological example, and the review by Jaeger & Van Valin 1982).

Since linguistic theorizing at base has so little in the way of essential psychological content,
however, a perennial problem is one of selection; that is, if nothing is psychologically motivated or directed at the outset, where does one start in the attempt to find areas of possible psychological relevance? This is by no means a trivial question, since by all accounts (and especially, I dare say, to the eyes of an outsider), the linguistic literature presents an almost bewildering array of detail and formal descriptive machinery, enough to daunt even the most intrepid of curiosity-seekers. Linguistic theory also appears to move at a staggering, even breathtaking, pace, such that just about the time one has managed to pour through enough of the detail to feel she is getting a handle on the situation, the ground rules or direction can suddenly change, leaving one to ponder a theory that even linguists concede is no longer relevant or interesting.

I can offer two guidelines on this score. First of all, given the relatively short half-lives that novelties in linguistics do indeed seem to characteristically exhibit, these are probably best left alone to stew in their own juices for awhile before the attempt is made to build any ambitious program of experimental psycholinguistic research around them.

The fundamental ideas that endure for many years, however, especially through some of the numerous paradigm shifts that must make pure linguistics a particularly frustrating enterprise, are a different story altogether and are probably some very good candidates for a closer look. My second consideration follows from what is undoubtedly the main motivation for experimental research in any discipline, namely, to sort out the wheat from the chaff. Since practical considerations dictate that only a select number of notions can be inspected with the requisite degree of care and perseverance, it follows that attention should be directed first and foremost to those issues which lie closest to the heart of the particular theories under examination, for if things are amiss at the core, the details scarcely matter — and by nipping misdirected theories in the bud, of course, a lot of unnecessary busy work can thereby be saved by all.

For example, one of the oldest and most enduring of all the notions from linguistics is the concept of the morpheme. Linguists may engage in apparently endless debate about the details of exactly how words ought to be separated into their meaningful parts (not to mention about how words themselves might best be defined or identified), or about how to represent or describe the resulting pieces, or even about what to call these bits, but the basic idea of the morpheme as a fundamental linguistic unit simply refuses to go away. It makes good sense, therefore, for the experimental linguist to inquire whether or not the morpheme is a valid concept for ordinary, everyday language learners and users — and, if so, to try and find out where these ordinary speakers draw their own lines.

Consequently, I spent more than a decade, off and on, struggling with this issue, yielding the battery of test questions that appears below. This particular question-set is taken from Smith & Derwing (1987), but its parts been used in various combinations in a number of studies to assess a subject's ability (or willingness) to recognize a root morpheme (such as teach) as part of some presumably derived word (such as teacher):

3See Van der Hulst and Smith (1982.2) for the briefest of epitaphs for a school of thought that dominated North American phonological and morphological thinking for the better part of two decades.
Q1. Definition of derived word. (Does the subject use the root? see Berko 1958)
Q2. Berko-type nonsense-word probe. (Is the root used? see also Derwing & Baker 1979)
Q3. (CF-I) Does the derived word ‘come from’ (CF) any other word? (Is the root indicated? see Smith 1987)
Q4. Does the subject know the root word? (confirmed by having the subject define the word.)
Q5. (CF-2) Do you think that the derived word ‘comes from’ the root word? (asked if the root was not identified in Q3) (Smith 1987)4
   Why do you think the derived word ‘comes from’ the word you suggested? (asked if the root was identified in Q3)
Q6. Did the subject ever think of this relationship before? (Derwing 1976)

This research has served to yield a good deal of interesting new data on the morpheme recognition capabilities of linguistically untrained speakers, and especially on the role of the two primary factors of similarity in meaning and similarity in sound that bear on this issue. Various auxiliary factors (such as orthography, construction type, affix productivity and the like) were also identified which can influence subjects’ judgments on these tasks and which therefore need to be controlled and independently assessed (see Derwing & Baker 1986).

Another fundamental and enduring idea from linguistics is the notion of rule, in our case a phonological or morphological rule. In a language like English, of course, morphology is clearly finite and, strictly speaking, rules are not necessary to describe the basic linguistic facts in this domain, which could all be accounted for in principle by simply listing everything in the lexicon. Moreover, even in a morphologically deprived language like English, it is well established that some degree of morphological productivity exists, and that young English-speaking children are prone to create ‘new words’ (like ‘one-ty one’ [meaning ‘eleven’]) as the situation requires.5

This is powerful evidence that children can and do extract some kind of rules, or at least engage in what has aptly been dubbed ‘rule-governed behavior.’ But, assuming for the moment that it is internalized rules of some kind that we are talking about here (I will later briefly discuss an

4 McCawley (1986) has also employed a variant of this question, which elicited judgments on whether the root word ‘was contained in’ the derived word,’ and results are reported in Derwing and Nearay (1986) from a more direct ‘word-cutting’ experiment, as well.

5 This need not imply, as in classical generative phonological accounts, that the function of a rule is to simplify the lexicon, or that a form listed in the lexicon could not also be derived by rule (see Derwing 1988b for some alternative views that are consistent with current psycholinguistic evidence regarding the content and structure of the mental lexicon).
interesting alternative approach), how far does the child actually go? How general, in other words, are the generalizations that the child latches on to? Linguists typically push generality to its very limits, almost as an article of faith (this is the chief symptom of what I like to call ‘the linguists’ disease,’ which is to seek out maximum regularity whether it is there to be found or not; see especially Derwing 1973 and 1974), but what do ordinary speakers, once again, do? And which particular rules, as postulated by linguists, might most profitably be examined from this psychological point of view?

It was argued by Chomsky and Halle (1968) that a particular set of vowel alternations constituted the ‘central problem in the noncyclic phonology of English’ (p. 99) and that the key rule involved (the so-called ‘vowel shift’ rule) was ‘without doubt the pivotal process of modern English phonology’ (p. 187). There is little wonder, therefore, in the face of such strong claims, that this rule (and its adjuncts) would capture the early attention of those experimentalists who were interested in assessing the psychological plausibility of classical generative phonology (GP) and some of its later successors (cf. Halle 1977 and Halle & Mohanan 1985). By this time, therefore, a number of investigators, including two Alberta graduate students (R. Cena and H. S. Wang), have subjected this rule and its variants to a quite extensive bit of psychological scrutiny, using as many as four different experimental techniques: PRODUCTION TESTS (à la Berko 1958; e.g., Ohala 1974; Steinberg & Krohn 1975), PREFERENCE or WELL-FORMEDNESS JUDGMENT TESTS (e.g., Myerson 1976, Armbruster 1978), RECALL TESTS (in which nonsense word-pairs showing these and other, arbitrary alternations were taught to see which pairs were remembered best — and which ways the errors went when the pairs were mis-remembered; e.g., Myerson 1976, Cena 1976, 1978), and, finally, CONCEPT FORMATION TESTS (in which some members of the supposed class were taught in order to see how well subjects spontaneously generalized to the other supposed members; e.g., Moskowitz 1973, Jaeger 1980, Wang 1985). Data collected from all of these sources can now be seen to converge on one clear and quite solid conclusion, namely, that five of these alternations operate together as a (semi-) productive set, but only five, and that these results fit none of the phonological theories proposed. What the data do fit, however, is a theory that typical, literate English speakers have pretty much all learned (or been taught) the familiar long vs. short spelling rule for the five vowel letters of the alphabet (see Wang & Derwing 1986 for full details).

So how can such developments be employed to enhance the psychological plausibility of current linguistic theorizing? For an answer to this question, we cannot look to classical GP, as that particular linguistic model has already died its own natural death (cf. n. 3 above). An approach called lexical phonology (LP), however, is not only very prominent on the contemporary linguistic scene, but is also the closest living descendant of the classical GP tradition and, of all its current competitors, the model closest to it in spirit, as well (for example, it posits very abstract underlying forms, makes liberal use of extrinsic rule ordering, and still invokes the transformational cycle, albeit in a somewhat more constrained form). Its main theoretical distinction, no doubt, is its integration of morphological and phonological processes (with the consequent elimination of boundary symbols) by means of an elaborate system of ordered strata/levels associated with the
lexicon.

LP also incorporates one very important empirical advance, which ought not to be minimized. Specifically, it explicitly specifies a level of representation ('lexical representation,' which is the output of the lexical component and the input to a set of completely general 'post-lexical' rules) as the mental representation or store. This is also claimed to be the representation that serves as the basis of native speaker same/different judgments, that is associated with the assignment of pauses, that is manipulated in secret code languages, and that is even the level of representation involved in speech errors (see, for example, Mohanan 1982:78-94). This is an important empirical advance in two respects: first, it involves the explicit recognition of the essential relevance of these four domains of data; thus any large-scale attempt to describe a language in this framework will yield a large number of very specific representations that can in principle be subjected to testing (see Campbell 1986, Hombert 1986 and Shattuck-Hufnagel 1986, which nicely illustrate some of the experimental possibilities). It is also important to notice that in exposing itself to potential falsification in this way, LP makes a major gain in empirical content over classical GP, which did not go on such a limb as far as its own underlying/lexical representations were concerned. (Fromkin thus makes some questionable assumptions in her analysis of speech error data, concluding, among other things, that these data support the GP account that the English velar nasal derives from an underlying /ng/ [1975: 51]; however, now in LP, where underlying and lexical representations are distinct and only the latter is taken as the level appropriate for speech error manipulations, her results must be explainable in terms of /ŋ/, not the underlying or pre-lexical /ng/. Fortunately for the theory, Smith 1982 has already shown that the speech error data can be accounted for at least as well under the /ŋ/ analysis as the /ng/ analysis.)

The unfortunate thing in all this is, however, that the true 'heart' of LP is not these lexical representations, which are the output of the lexical component, but rather the formal machinery inside, together with the vast array of semi- or non-productive processes postulated as being linked to morphological operations. Without gaining experimental access to these, therefore, any model could claim empirical equivalence to LP by the mere positing of the same set of lexical representations. It is in this area, therefore, that the procedures and findings discussed earlier in this section can be employed to beneficial effect.

In LP, much as in its GP progenitor, morphological analyses and operations are predicated upon what professional linguists find intuitively satisfying or plausible (e.g., that such words as fable and fabulous [a 'vowel shift' pair] are morphologically related, and that some version of the vowel-shift rule is a central part of the phonology of English [cf. Kiparsky 1982; Halle & Mohanan 1985]). There is no longer any good reason for linguists to base their theories on such an informal body of evidence. Using the procedures outlined above for morpheme recognition (supplemented, perhaps, by other, more 'subliminal' approaches, as illustrated in Fowler, Napps & Feldman 1985), data can now be readily obtained for the empirical evaluation of any number of proposed morphological relationships (see Derwing 1976, for example, for data on the specific fable-fabulous pair); by the same token, proposed phonological or morphological rules can
similarly be evaluated by means of the techniques outlined above that have successfully been used for them (showing, as indicated, that vowel shift is not a viable phonological process at all, but rather something quite different).

If LP responds to this challenge of empirical accountability, its status as a plausible psychological theory will correspondingly be greatly enhanced. On the other hand, if linguistic theorists of this and other schools persist in their narrow-minded commitment to so-called 'internal' (i.e., familiar) evidence only (cf. Ohala 1987), they risk even further alienation from the very field that they most need to court. The relationship between theory and data is a reciprocal one: if experimental evidence from real-time language processing is irrelevant to linguistic theory, then linguistic theory is, by the same account, irrelevant to real-time language processing. One cannot have one's cake and eat it, too.

2.2 The Psychological Utility of Linguistic Concepts

Another way for linguistics to enhance its status in the eyes of psychology is to show that at least some of the ideas that have emerged from purely linguistic research (that is, from the close examination of so-called 'primary linguistic' data) have some potential utility for contemporary language user models. One such model, called Parallel Distributed Processing (PDP) (or, less formally, 'connectionist theory'), has recently generated a great deal of discussion and enthusiasm and has been specifically applied to the investigation of morphological issues (see especially Rumelhart & McClelland 1986, which deals with the description and acquisition of the English past tense inflection). In a recent review, in fact, Sampson sees in PDP not only an empiricist model that is likely to give Chomskian rationalism a good run for its money, but even 'an intellectual paradigm fully as revolutionary as the generative paradigm ever was' (1987:871). The PDP analyses illustrated, however, are all cast in the framework of holistic segments (e.g., regular allomorphs of the past tense inflection), without regard for their further analysis into bundles of phonological features.6

There is good experimental evidence to indicate, however, that a feature analysis is required (or some formal equivalent to it) in order to account for the developmental sequences (or 'stages') observed. This is particularly evident in the case of the regular English plural inflection. The study involved was a cross-sectional one that used the Berko (1958) nonsense-word probe technique as a test for productive knowledge of the regular plural endings (Innes 1974). Although an age-based analysis did not yield any definitive developmental patterns, a new analytic technique, called 'response coincidence analysis' yielded some quite remarkable results, which could only be seen once the subjects were pooled into homogeneous performance groups (see Baker & Derwing 1982 and Woods, Fletcher & Hughes 1986, Chapter 14, for details).

6Some of the other problems with PDP-based linguistic accounts, such as those raised by Pinker & Prince in their review (1988), are discussed in Derwing 1988b.
The point of particular relevance to the issue at hand, however, is the fact that, at each stage of this developmental picture, performance on the nonsense /ð/-stem was comparable with that on the other sibilant-final stems.\textsuperscript{7} What is remarkable about this, of course, is that there are no plausible /ð/-stem plurals that might serve as the real-word analogs here, at least none that the small children involved were likely to know.\textsuperscript{8} It looks very much, therefore, that the children have learned to pluralize this type of nonsense stem not by analogy to other /ð/-stem words, but rather by analogy to other (i.e., non-/ð/) sibilant stems. In other words, to accommodate such data within a PDP account, it would seem that nodes for phonetic features would also have to be appended, with vast further extensions to the connectionist networks involved.

Another place where linguistics might contribute to psychology is in the development of models of word recognition and/or lexical access. So far as I know, all such models have heretofore assumed that lexical items were both represented and primarily accessed in terms of their constituent phonemes, i.e., individual segments (see, for example, Morton 1969, Forster 1976, Marslen-Wilson & Welsh 1978, Elman & McClelland 1984, etc.), though syllable strategies might also be sometimes employed (Cutler et al., 1986). We have done a considerable amount of work in our laboratory on the psychological status of the phoneme, as well as a number of other phonological units that have proposed by linguists as potential ‘building blocks’ for the mental lexicon.

The phoneme issue, in particular, has been explored here and elsewhere by means of quite a number of different experimental techniques, including CONCEPT FORMATION (Jaeger 1980), DISCRIMINATION TESTS (Derwing & Nearey 1981), STRING SIMILARITY JUDGMENTS (Vitz & Winkler 1973; Derwing & Nearey 1986) and even simple SEGMENT COUNTS (e.g., Dow 1981; Derwing, Nearey & Dow 1986). In general, the results of all of these studies tend to confirm a traditional taxonomic phonemic analysis.

This superficially very neat picture is much complicated, however, by a rather large body of literature which suggests that children and illiterates are not very good at counting or otherwise manipulating individual segments or phonemes. (Some new data are reported in Dow 1987 and Dow & Derwing 1987, in fact, indicating that English-speaking children are better even at manipulating whole onsets than they are at manipulating any of their constituents parts.) Some recent cross-linguistic work also suggests that educated adult speakers with non-alphabetic orthographic traditions (such as Mandarin Chinese, as reported by Read, Zhang, Nie & Ding 1986) can’t manipulate segments very well, either. (To illustrate with an anecdote, one of our own Chinese graduate students once confessed to me — in perfect English — that before he had

\textsuperscript{7}Except the /ð/-stem, whose special status can be explained (see Derwing & Baker 1979:212).

\textsuperscript{8}Linguists commonly use the word rouges to illustrate this pattern in introductory courses, but this is a highly marked plural of a mass noun - and even the root seems to be losing ground rapidly to blush these days. (There used to be a word rouge, I understand, to refer to a one-point conversion in Canadian football, but this usage has long disappeared from the local scene in Edmonton.)
started to study 'Western linguistics,' he had no idea that words like can and not had anything at all in common. The potential theoretical implications of these preliminary findings are enormous, for they suggest the strong possibility that, contrary to the long-standing tradition of western linguistics, the segment (or phoneme) may not be the natural, universal unit of speech segmentation, after all, and that the orthographic norms of a given speech community may play a large role in fixing the scope of the 'basic' phonological units that the members of such a community actually perceive.

Finally, metrical phonology (MP) presents what is undoubtedly the most radically different of the currently popular approaches to phonological theory. As is well known, it grew out of consideration of certain suprasegmental phenomena (e.g., stress, and in autosegmental phonology, tone) that could not be conveniently handled under the purely segmental approach of SPE. The result has been the creation of a vast theoretical framework of new levels (tiers) and units. As suggested by the preceding discussion, all of this theoretical apparatus must be empirically justified, if anything of psychological import is to be made of them.

To date, in fact, the results have been rather encouraging on this score. One important feature of MP has been the re-emergence of the syllable as a viable structural unit; moreover, no longer is the syllable viewed as a mere sequence of phonemes (as in SPE and in Hooper 1972). Formal (and presumably universal) procedures are proposed both for determining syllable boundaries (cf. Selkirk's Maximal Onset Principle, 1982) and for analyzing the syllable into its integral constituent parts, as in the typical right-branching structure for the English word blind, according to which the syllable is first broken down into the onset /bl-/ and the rhyme (or rime) /-aynd/, then the latter into a nucleus (or peak) /ay/ and a coda /nd/. (Other theorists, such as Iverson & Wheeler 1987, argue for a left-branching structure, at least for some languages, which links the onset with the nucleus).

The experimental investigation of these ideas was begun by Treiman in the early eighties (Treiman 1982, 1984) and my student, Maureen Dow, continued the effort in her dissertation, completed in 1987. We can now confirm, therefore, based on a variety of different experimental techniques (unit counting, global sound similarity judgments, and especially string manipulation techniques of various kinds), that the integrity of onset and coda units has been established for English, as well as the superiority of a right- over a left-branching model of the English syllable (see Derwing, Dow & Nearey 1987 for a summary account). On the other hand, more recent work by Treiman & Danis (1988) indicates that, while English speakers are highly consistent in counting the number of syllables in words, they are at the same time quite uncertain, under some circumstances, about the location of the syllable boundary in VCV environments. This result challenges not only the universal applicability of such influential proposals as Selkirk's Maximal Onset Principle (not to mention the associated notion of ambisyllabicity), but the very idea of the well-definedness of the syllable in general. (Note that Cutler et al. 1986 also explain differences in the speech perception strategies used by English and French speakers in terms of the relative ease of establishing precise syllable boundaries in the two languages.) The results for MP in general have thus been mixed so far; nonetheless, the results have been encouraging enough
to persuade me that this is the most promising model for the immediate future of phonological investigation, which is precisely why my own experimental efforts have taken a sharp turn in its direction.

At the very heart of MP, for example, is the venerable notion of the ‘sonority hierarchy’, i.e., that all segments have their natural place on a specific sonority scale (with open or low vowels at the high end and voiceless stop consonants at the other); this has served as the foundation stone upon which syllable structures have been hypothesized in the first place. Recent suggestions have also been made to the effect that there may be no sharp division between the nucleus (sonority peak) and coda, but rather that segments are bound successively closer to the vowel or syllable nucleus as a function of their place on the sonority scale. Such claims are also specific enough to lend themselves to experimental test and we had some encouraging preliminary results to report on this matter at the LSA in December (Derwing, Nearey & Dow 1987). No single experiment is sufficient to conclusively resolve such issues, however. Since so many of the experimental procedures we have been using are new and open to the challenge of validity, we must take pains to insure that our key findings can be replicated under varied experimental conditions (cf. Derwing 1979). Perhaps an equally limiting aspect of our research effort to date is its preoccupation with English; clearly, since universal claims are often involved, it is vital that these experiments be extended to other languages (cf. Derwing 1988a).

3. Prospectus and Conclusions

Either linguistics of the next century will forsake its autonomy and participate in the cognitive enterprise as a full experimental partner or, if it persists in holding doggedly to its own set of ‘purely linguistic’ methods and models, it will likely wither back into its former role as a minor branch of anthropology, roughly on a par with archeology, leaving to others the systematic exploration of the unique ‘window’ that language provides into the human mind.

In sum, today’s linguist has come face to face with a question that I posed only in hypothetical terms in Derwing (1973:333): cognitive science, with the study of language processing as its heart, is alive and well and linguists are going to have to decide whether they want to participate in its future development, or rather to continue to languish in their own little world of uninterpreted models of language, where major decisions are made on the basis of ‘thought experimentation’ and arbitrary principles of theory evaluation. I have no doubt which of these choices linguists will opt for in the long run, but in the short run they are letting others run far ahead with the ball. It would be a shame if the first speaker-hearer models were built with scarcely a linguist on hand to celebrate the event.
References


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