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edited by Laurie Bauer

**Department of Linguistics
Victoria University of Wellington
P.O. Box 600
Wellington
New Zealand**

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Is the Morpheme Dead?

Laurie Bauer

Abstract

After a brief consideration of the different ways in which the morpheme was interpreted by the structuralists, three different modern theories which discuss word-structure without any such element are outlined: Word-and-Paradigm (A-morphous) Morphology, Hockett's theory of resonance and Bybee's lexical connection. Various common threads between these three theories are pointed out and discussed, as are problems that these theories give rise to. It is concluded that while these theories do not allow the analyst to conclude that a particular piece of form HAS a particular meaning (rather than that the word-forms in which these forms occur all have meaning in common), the morpheme is unlikely to fall into disuse, and also that morphemic approaches to word-meaning may complement rather than contradicting some of these other approaches.

1 Introduction

Sometime in the distant mists of Linguistics 101 we all learned about the morpheme, and since then we have had the feeling that we have a reasonable idea about what a morpheme is. When we meet discussions of morphemes in books on language change or language acquisition or semantics or generative syntax, we do not clutch our heads in despair and bewail the fact that we don't understand what they're talking about, or dash for one of the many reference books of linguistics to confirm our understanding. Yet perhaps we should, because understandings of the morpheme are very different for different scholars, even (or perhaps especially) those who have considered the notion in detail.

I want to begin this paper by separating out two of these distinct notions, both of which arise within American structuralism. In this section my debt to Matthews (1993) will be very obvious. But I want to go on from making those distinctions to look at some recent developments, because there are at least three people in the field who are — for rather different reasons — suggesting that the morpheme is not a very useful notion to deal with at all. I shall try to outline briefly their competing views of morphology. Finally, I shall ask whether the morpheme has outlived its usefulness, and whether models without morphemes represent real advances in the field.

* A version of this paper was presented at the University of Auckland in September 1994. I should like to thank members of that audience for their feed-back. I should also like to thank Shizuka Torii and Winifred Bauer for their comments on an earlier version.

2 Two structuralist morphemes

2.1 The morpheme as a form

According to the first structuralist viewpoint, morphology is the study of the shapes of words (compare the German *Formenlehre*), and morphemes are correspondingly distinctive units of form. These units of form, it is true, have meaning, but meaning is not part of their definition. Correspondingly, it is perfectly possible for several different morphemes to have the same meaning, so that /ɪz/ on the end of *horses* and /ən/ on the end of *oxen* are two different morphemes that have the same meaning. This is basically the situation as it is put forward by Bloomfield (1935 [1933]).

Although the basic principle is that different forms are different morphemes, it is recognised that some forms are altered by their environments in a process of phonetic modification, so that it is possible to have variants or alternants of morphemes. Thus /s/ and /z/ are alternants of the morpheme /ɪz/.

It is possibly worth noting that this notion of the morpheme is very similar to that propounded by Aronoff (1976). He points out that the *mit* in *remit*, *commit* etc, although it may not have a constant meaning, does have a constant phonological behaviour in that it becomes *mis* before certain affixes, unlike the superficially identical sequence /mit/ in *vomit*, which does not take part in this alternation (*remissory*, **vomissory*). He concludes (1976: 14-5) that

The hypothesis that morphemes are the "minimal meaningful elements of language" cannot be maintained ...

and that

A morpheme is a phonetic string which can be connected to a linguistic entity outside that string.

2.2 The morpheme as a linguistic sign

The post-Bloomfieldians move away from this notion of the morpheme to one in which the morpheme is a linguistic sign. Harris (1966 [1942]: 109) defines the morpheme as follows:

Every sequence of phonemes which has meaning, and which is not composed of smaller sequences having meaning, is a morpheme.

Here the morpheme is still a form, but the meaningfulness of the form is important. In his following paragraphs, Harris takes the meaning to be of prime importance, since he appears to want to call different forms with the same meaning the same morpheme, whether or not the form is identical or even easily derivable by general rules of phonetic modification. Harris talks in terms here of morpheme alternants and morpheme units. The ablaut that relates *take* to *took* is, for Harris, part of the same morpheme unit as

the /ɪd/ on the end of *wanted*. At this point, the morpheme has ceased simply being a sign, and has come to be a set of signs. Matthews (1993) implies that the change in focus comes about because the post-Bloomfieldians did not fully understand what Bloomfield had been saying on this subject. This is as may be. The two theories certainly have different analytical results.

Five years later, Hockett (1966 [1947]: 229) took over Harris's analysis, but introduced the terms morph and morpheme for Harris's morpheme alternant and morpheme unit. Because of the insistence on the inclusion of meaning in the morpheme, Hockett faces problems with portmanteau morphs and empty morphs, problems which have never been appropriately solved. Matthews (1993: 82) suggests that part of the difference between Bloomfield and his followers was that while Bloomfield viewed the morpheme as the basic unit of the lexicon, Harris and Hockett viewed it as the basic unit of grammar. The difference is that for Harris and Hockett /ən/ in *oxen* and /ɪz/ in *horses*, by virtue of their same meaning and their complementary distribution, have the same grammatical function and so must belong to the same fundamental unit. For Bloomfield it was sufficient that they shared meaning without them having to be linked as belonging to the same morpheme.

2.3 Other morphemes

I do not wish to imply, by singling out these two versions of what a morpheme is, that these are the only versions available. Mugdan (1986) goes into more detail and looks at post-structuralist meanings for the morpheme, as well as at the two (fairly grossly distinguished) versions I have considered. Other meanings of the term range from inflectional affix to phonological rule. Spencer (1991: 12-21) spends some time discussing the distinction between morphemes as things (either of the two analyses given above) versus morphemes as rules (linking pairs of words related by apophony, reduplication, conversion, suprasegmental variation, and so on). That is yet another distinction.

The importance of all this is that the morpheme has faced difficulties ever since it was first introduced. There is no agreement as to what a morpheme is, and never has been. However, at this point I would like to leave that for the moment, and go on to consider three recent proposals that do without the morpheme entirely, before trying to see what these various approaches have in common.

3 Three proposals with no morpheme

3.1 The argument from Word-and-Paradigm

The Word-and-Paradigm argument is easy to summarise, though its implications may not be easy to see. The structuralist morpheme, runs the argument, implies a one-to-one relationship between form and meaning. While this is found in many cases, there are also many cases in natural

language where this is not found (I shall give some examples in a moment). Accordingly, we need to account for instances where there is no one-to-one relationship between form and meaning, and this implies doing away with the morpheme as it was in structuralist treatments.

The kinds of example that provide backing for this position are by now well known, and I shall summarise only briefly. Firstly there are cases of cumulative exponence, such as the case/number endings in Latin where a single formative encodes information about both case and number. Secondly, there are cases of extended exponence, where the same meaning is carried by more than one formative in the word-form. For example, in the Latin perfect form *cucurrístis* 'you (pl) have run' the *cu-* indicates that the word is perfect, but so does the presence of the *-is-* following the root; both the *-is-* and the *-tis* indicate that the word is second person. We then have the problems of zero morphs and empty morphs. With zero morphs the meaning associated with a particular morpheme is present, but there is no formal marker of its presence, such as the nominal plural in *The sheep have eaten the grass*. With empty morphs a formative is present, but it does not appear to carry any meaning associated with a morpheme: thematic vowels in Romance verb forms are frequently cited as examples of empty morphs. For example the second *a* in Italian *amavo* 'I have loved' and the second *e* in *temevo* 'I have feared' show the conjugation class of the verb, but have no other meaning of their own. Then there are examples of superfluous morphs, such as the feminine forms of adjectives from which adverbs are derived in Romance. Here we have a formative, and we know what meaning it usually has, but the meaning is missing in the construction concerned: there is no feminine meaning in French *franchement* 'frankly'. Then there are cases of apophony, dealt with in the structuralist literature under headings such as replacement morphs. Here a change from one segment to another (or, we might add, from one suprasegmental pattern to another) has an effect equivalent to that of adding an affix in other words. English examples are *sheathe* from *sheath*, *saw* from *see* and *abstract* from *abstract*. Then there are cases of subtraction, such as the Danish formation of the imperative by deletion of the final [ə] in the infinitive — these are rare, it must be admitted, and it is usually difficult to prove that they are psychologically real, but there are sufficient cases in the literature for it to seem a reasonable morphological process. Equally rare are cases of metathesis used morphologically (as in Klallam), and instances of what Anderson (1982) refers to as inversion. In a language like Georgian, agreement is marked on the verb for subjects, objects and indirect objects. However, in reportive tenses 'the series of markers used elsewhere to mark subjects instead marks the person and number of the direct object ...; and the markers used elsewhere for indirect objects mark subjects' (Anderson 1982: 600). Less rare are what I have elsewhere called synaffixes (Bauer 1988), of which the best known is the circumfix. And finally, though this list is not exhaustive, there are the common cases of reduplication where the form of the morph depends on the form of the base to which it is added, with the result that it is not possible to state a segmental underlier for the morpheme and notions of phonetic modification fail completely.

These types of example are, on the whole, well known and well described. Between them they provide a convincing case that not all of morphology is a sequence of biunique beads on a string. To what extent structuralists has such an image in mind is difficult to decide. It is clear that the types of phenomenon listed above did cause difficulties for structuralist descriptions. It is equally clearly the modern perception that the beads-on-a-string view was prevalent in the structuralist period. Hockett (1987: 83-4) complains that the effect of the structuralist approach was to treat all languages as if they were 'really' agglutinative. Spencer (1991: 39) concurs. 'The very concept of the morpheme', he says, 'tends to presuppose that all morphology is agglutinative, at some level of abstraction'.

What are the alternatives? Anderson (1982: 595) suggests that 'rules substitute for the usual inventory of sound/meaning pairs ("morphemes")'. That is, the features for morphosyntactic categories are on one side of the equation, the form is on the other side of the equation, but the two are not linked in terms of morphemes but in terms of rules creating phonological structure. Since these rules create the link between meaning and form, they appear to have the function that the morpheme as sign had. In a later work, however, Anderson (1992: 1) speaks of a theory that dispenses with morphemes completely.

Matthews (1993: 75), I think, makes this view-point clear. Why, he asks, do we have to associate plurality with the /ɪz/ in *horses* rather than with the whole word? Rather than saying that there is an element /hos/ and another /ɪz/ and together they form a plural noun, might we not say we have a plural noun, which is marked as such by containing the sequence /ɪz/? In other words is the plural noun primary or is it derived from the elements that go to make it up? Matthews opts for the former analysis. In an earlier discussion of the same phenomenon, Matthews (1991: 125) says

consider again a form like *seas*. Qua form it is [si:] plus [z]; that no one will dispute. Nor will anyone dispute that, as a whole, it is syntactically Plural. But it does not follow that it is syntactically a sequence of two elements ('SEA + Plural'). ... In the traditional formula, it is simply 'the Plural of SEA'.

To make all this theory a bit more concrete, let us consider how this theory would deal with three forms: *oxen*, *horses* and *grumpiness*. The first two are chosen to illustrate cases of irregular and regular inflection, the third to illustrate derivation.

In Word-and-Paradigm *oxen* and *horses* would be dealt with by a single set of ordered rules. We would have the lexemes OX and HORSE, each marked with the feature or morphosyntactic property [+plural]. The form *oxen* would be derived by reference to a lexical entry in some form (possibly as in (1)), while that of *horses* would be ordered much later in a default group whose form is determined purely phonologically (perhaps as in (2)).

- (1) $\left[\begin{array}{c} \text{OX} \\ + \text{plural} \end{array} \right]$

$$X \rightarrow X + \text{ən}$$

$$(2) \left[\begin{array}{c} \text{N} \\ + \text{plural} \end{array} \right]$$

$$X [+ \text{strident}] \rightarrow X [+ \text{strident}] + \text{ɪz}$$

Precisely how *grumpiness* will be dealt with is less clear, though we should expect similar types of rule to build phonological structure, so we might expect rules like those in (3), where Y and Z are random labels for subclasses of adjective and noun:

$$(3) \left[\begin{array}{c} \text{NBASE} \\ + \text{adjectiveZ} \end{array} \right]$$

$$X \rightarrow X + \text{i}$$

$$\left[\begin{array}{c} \text{ABASE} \\ + \text{nounY} \end{array} \right]$$

$$X \rightarrow X + \text{nəs}$$

3.2 Hockett's resonance theory

Hockett (1987: 86) characterises the structuralists as having held the following assumption: 'the meaning of an utterance is a function of the meanings of its constituent morphemes and of their arrangement'. He wishes to reject this assumption, along with the implicitly agglutinating view of language mentioned above. His reasons, however, are different from those of scholars working with Word-and-Paradigm. He wants a theory of word-structure to deal with far more than has traditionally been included within morphology. He wants to be able to include the mechanism that gives rise to blends, he wants to include phonaesthemes, he wants to include reanalyses like *a doggy dog world*, even allusions and puns. And he does not want the possible analyses to be mutually exclusive. Why, he asks (1987: 74), do we have to choose between the analysis of *hamburger* as either *Hamburg + er* or *ham + burger*; why can't we have both simultaneously?

To make these associations (which Hockett 1987: 95 explicitly links to Saussure's 'rapports associatifs'), Hockett suggests a theory of resonance. Utterances trigger in listeners a number of resonances, and meaning is derived from the resonances. Some resonances may be stronger than others: we may actually suppress the resonances that lead us to think of *sham* and *rock* when we hear *shamrock*. Some resonances may themselves suppress others, as when we lose all notion of limbs when told that someone had their *leg pulled*. Some may only work for individual speakers. Many, however, work for all, and 'The "official grammatical structure" of utterances is in the last analysis nothing more than that which correlates with the clearest and most widely shared of the resonances'

(Hockett 1987: 88). Hockett (1987: 97) links this principle of resonance with Bloomfield's (1935 [1933]: 78) statement that 'in every speech community some utterances are alike in form and meaning', though he clearly is interpreting this in a rather different way from the way in which it was interpreted by Bloomfield and the other structuralists at the time.

I shall not consider this hypothesis any further at this point. Hockett's ideas are not worked out in as explicit a fashion as are Matthews's or Anderson's so that the same amount of detail is not possible. Nonetheless, there is sufficient here for us to make comparisons with other expositions about morphological structure, and to have some idea about how *oxen*, *horses* and *grumpiness* would be dealt with.

The strongest resonance from *oxen* would be with *ox* itself, but there would also be a very much weaker one to *brethren* and *children*. Other potential resonances with forms like *drunken*, *ridden*, and *stolen* and even less clear ones to forms such as *coven*, *mutton* or *wanton* would be suppressed because of the incompatibility of the past participle meaning with *ox* and because of the lack of any meaning correlating with the others. There would be other resonances with regular plural nouns, simply by virtue of their plurality, and weaker ones with *cart*, *yoke*, *cattle*, *strong* and possibly also with rhyming words such as *fox* and *box*.

The resonances with *horses* are clearer. On the one side there is strong resonance with *horse*, and on the other with *beaches*, *courses*, *fusses* and so on. The fact that *horses* can be a verb (*he horses about instead of getting on with his tidying up*) is covered by realising that there are also resonances with *watches*, *refuses*, *curses* and the like, which are suppressed if the word is in a nominal function. There are also resonances with *hoarse* 'with a husky voice' to allow puns and the other wordplay. And there are semantic resonances with *ponies* and *stallions* and *mares* and *foals* and so on, and also with *carts*, *cavalry*, *TAB* and many other such collocations.

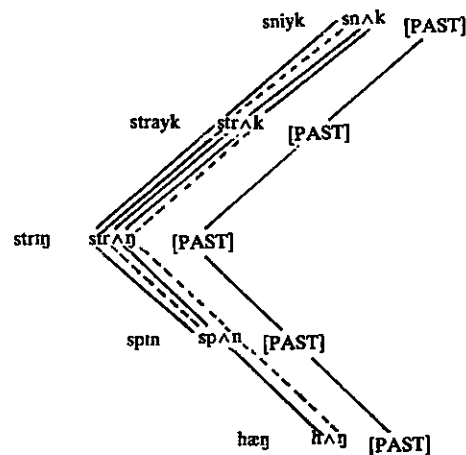
There are at least four major sets of resonances with *grumpiness*. Firstly, there are those that depend on the phonaestheme /gr/ (Marchand 1969: 412): *grim*, *gruff*, *grumble*, *grunt* and so on. Then there is the resonance with *grump*, *grumps*, *grumpish*. Thirdly there is the resonance with words like *sandy*, *lumpy*, *dirty* and *murky*. And finally there are the resonances with words such as *happiness*, *dryness*, *softness*, *cantankerousness*. There may also be resonances with other nouns derived from adjectives, with other nouns, with other words derived from adjectives, although the strength of these is likely to vary from speaker to speaker.

3.3 Bybee's lexical connection

Bybee (1985) works with a model in which word-forms are related to each other in terms of phonological and semantic similarity. Phonological similarity is measured in terms of segmental identity or shared features at the same position in the word-form. Semantic similarity is less well defined, but includes relations such as antonymy, hyponymy, meronymy or

even collocation within a similar domain (e.g. *doctor, nurse, surgery*) (Bybee 1985: 118), and must be assumed to include synonymy. Morphology deals with areas where these two types of similarity coincide: 'if two words are related by both semantic and phonological connections, then a morphological relation exists between them' (Bybee 1985: 118). Lexical connection can be used to account for relationships within or across paradigms, and also to account for morphological classes. Figure 1 illustrates a morphological class of English strong verbs, with solid lines showing identity, dotted lines showing shared features.

Figure 1: A class of English strong verbs (Bybee 1985: 130)



For Bybee, *horses* would be in a paradigm with *horse* on the one hand and with *asses, porpoises, thrushes* and (further away) *dogs, cats* etc., too, on the other. These words would be linked by meaning as well as (diminishing amounts of) form. *Horse* and *hoarse* would not be a morphological link, because only form and not meaning is involved. *Oxen* and *grumpiness* would equally be part of other paradigms, as discussed above with reference to Hockett's approach. The importance of phonological structure in Bybee's approach means (as I read it) that she would be unlikely to want to link *grumpiness* with *pomposity* on the grounds that both *-ness* and *-ity* derive abstract nouns from adjectives, while Hockett might well. In other words, Bybee might limit herself to consideration of the "official grammatical structure", although I see no particular reason why phonaestemes should be excluded from her analysis.

4 Commonalities

These three versions of morphology without morphemes have their differences, but also have things in common. In this section I shall look at some of the common threads that link the three.

The first obvious commonality is that all three take the word-form as a more fundamental unit than any element of the word-form. In the case of Hockett, the fundamental unit may be a lot larger than the word-form (*pull s.o.'s leg, kick the bucket*, etc.), but is not smaller than the word-form. Interestingly, both Hockett (1987: 88-9) and Bybee (1985: 50-1 *et passim*) relate this to first language acquisition, where the child minimally learns word-forms rather than morphs. Only after a number of word-forms have been learned is it possible to analyse the morphs from which the word-forms are constituted. That is just as true in second language acquisition. I know that I could use the 'words' *s'il vous plaît, arriverderci* or *ДО СВИДАНИЯ* long before I could recognise their constituent morphs and work out their syntax.

The second commonality is that the structure of the word-form is, in all three views, purely phonological. Particular parts of that phonological structure might resonate with particular sets of words which have meaning in common (for instance the sequence /ʌŋ/ might resonate with a set of words all of which share the meaning 'past tense'), but that meaning is not associated directly or primarily with that particular piece of phonological form (it is associated with each of the word-forms involved). In the Word-and-Paradigm framework we would have to reformulate this and say that certain parts of the phonological string arise because of the combination of meanings expressed by the word-form, but they are not linked in a one-to-one way. While the theoretical implications of the two formulations are no doubt different, the basic notions seem perfectly compatible.

A third commonality is the importance of paradigmatic structure. With the Word-and-Paradigm approach and Bybee's, both of which are concerned with the structure of inflectional paradigms, this is perhaps not particularly surprising. In Hockett's view, resonances 'hold between a form actually spoken and an indefinite number of more-or-less similar forms in the user's internal storage' (1987: 95). We could reformulate this as 'and indefinite number of more-or-less similar forms *in absentia*', and the notion of paradigm (albeit not inflectional paradigm) would be overt again.

There are also other similarities between the approaches which are perhaps not quite as pervasive. In some cases they are clear parallels between two of the approaches which may then be compatible or incompatible with the third. Let's consider some of these, too.

Both Bybee's and Hockett's approaches are multi-dimensional. In Figure 1, above, we see that segmental phonological structure can provide one dimension of a series of comparisons, but then the feature [PAST] is also added to the comparisons. Strictly this feature is in a different dimension, also linking with words like *recognised, walked, saw* and so on.

Multidimensional objects are notoriously difficult to draw on a two-dimensional page, so that Bybee's figure puts the semantic and the formal connections in the same dimension, but this is surely no more than a matter of convenience. Hockett's approach is overtly multi-dimensional. If we can have resonances working directly from form which in some cases link that form on the basis of lexical meaning, in other cases on the basis of phonaesthemes, in others simply on the basis of phonological similarity (Hockett 1987: 95), then these resonances must be working on different tiers (in the current metaphor) in multi-dimensional space. Although Bybee doesn't discuss such matters, it seems perfectly reasonable in view of her notion of lexical connection that *gleam* and *glimmer* and *glisten* and *glint* etc should be connected formally because of the initial *gl-* just as they are connected semantically because of the connection with light. It is perhaps less clear that the same is true in the Word-and-Paradigm approach. As this is outlined by Anderson and Matthews it has no way of discussing phonaesthemes, since the *gl-* sequences in all of the above words are simply parts of lexical entries. However, every second person form is related to every other by the presence of the feature [2nd person] (however expressed), and every perfect form is related to every other by the presence of the feature [perfect], so that the second person plural perfect active of CURRO is linked to a vast number of other verbs by the place it holds in the various paradigms. The difference is that in the Word-and-Paradigm approach the meaning is used to derive the form, while in the other approaches the meaning is simply another correlate of the form, increasing resonance/lexical connection. This difference may mean no more than that Word-and-Paradigm is basically a generative theory of word-structure, while the other approaches are rather more functionalist in their orientation, but there may be more fundamental differences here.

Both Hockett and Bybee view a morphological relationship as being derived not primary. This is probably true of the Word-and-Paradigm approach, too, though not quite in the same way. For Bybee and Hockett, a morphological relationship is the result of a repetitive association of form and meaning within the word-forms (which are the elements which actually carry the meaning). Within Word-and-Paradigm, a morphological relationship is a relationship between words, and comes about through 'a word's derivational relations to other words' (Anderson 1992: 263). In any of these cases, a morphological relationship is something which derives from comparisons between words, not something which determines the internal structure of a word.

5 Problems

This new (and rather surprising) accord is not necessarily without its problems. The most obvious of these, it seems to me, is how we are supposed to deal with productivity, viewed as the rule-governed creation of new forms. In Aronoff's (1976: 19) words, 'It thus remains the task of a morphology to tell us what sort of words a speaker can form'. It is not clear to what extent this can be done within the three approaches that I have outlined here. On the other hand, it must also be admitted that it is less

clear in 1994 than it was in 1976 that Aronoff's statement about the function of a morphology is true. In the intervening twenty years we have seen a lot of people (including Bybee, of course) being willing to leave a lot more to analogy and worry less about rules in morphology. One position on this question is that there is no distinction to be drawn between rules and analogies in morphology, that everything is an analogy, as was suggested by Becker (1990). My reaction to that book was that Becker had failed to prove the lack of distinction, and that there was a difference in kind between those things we might think of as analogies and those we might think of as rules. For example, *Vaxen* as the plural of the computer a *Vax*, was clearly analogy, while *Vaxes* was equally clearly a matter of rule (Bauer 1992). But it becomes difficult to show this difference of kind in a non-circular way. If the difference is simply in the number of outputs there are from a particular analogy/rule, then it is far from clear that any major matter of principle is involved, and Becker may be right. And most of the crucial examples are in favour of analogy rather than rule: a new word like *muggee* could be formed either by analogy with *murderee* or by a rule which says, in effect, $N \rightarrow V + ee$. A new word like *biographee*, on the other hand, cannot come from a similar rule because it does not have a verbal base, but requires a proportion such as *murderer* : *murderee* :: *biographer* : ?? This is not really the place to go into the details of the rules versus analogy debate (that would take at least another paper). Perhaps all we can say at the moment is that there is ongoing discussion in this area.

Another potential problem with all of these approaches is agglutinative morphology. If it was a difficulty with the structuralist morpheme that it was part of 'the great agglutinative fraud' (Hockett 1987: 82), might the fraud not be just as great if we insist on treating languages like Basque, Kannada, Swahili or Turkish as if they have no agglutinative morphology but are all fusional? Consider, for instance, the fact that there is no language which has inflectional morphology but no derivational morphology (Greenberg 1963) alongside the fact that there is no language (as far as I am aware) that has fusional derivational morphology. This seems to imply some kind of priority for agglutination, not for fusion. Anderson (1992: 260) suggests that there is no necessary problem here.

No one would contest the claim that [a constituent analysis] corresponds to something which is 'true' of the word *discontentedness*, but it does not follow from this that the decomposition of the word is an aspect of its structure, any more than its etymology is. To see that there is an issue here, we can contrast such views, based on morphemes, with a picture of morphology as based on a system of rules which map words (or stems) onto other words. ... On this picture, the structure of *discontentedness* is given by a derivation:

$$(2) [N \text{ content}] \xrightarrow{\mathcal{R}_{\text{dis}}} [N \text{ discontent}] \xrightarrow{\mathcal{R}_{\text{ed}}} [ADJ \text{ discontented}] \xrightarrow{\mathcal{R}_{\text{ness}}} [N \text{ discontentedness}]$$

Each step of such a derivation maps the phonology, the semantics and the syntax of its inputs onto the (corresponding) properties of its outputs. It expresses the same facts as the [constituent structure tree], such as the observation that some of the subparts of the word are themselves words, the relative scope of morphological

operations, etc., but without imposing a distinct structure on derived words to represent their morphological analysis as an aspect of their form...

To me, this looks like sleight of hand, a way of keeping morphemes when you are not having morphemes. Yet it is clearly within the spirit of all these three approaches that the morphological structure is not, in itself, part of the structure of the word, but is a post hoc analysis based on knowledge of associated words. Here Hockett (1987: 88-9) suggests, we as linguists are misled by our own oversophistication, and that ordinary speakers do not deal with elements smaller than the word.

6 Back to the morpheme

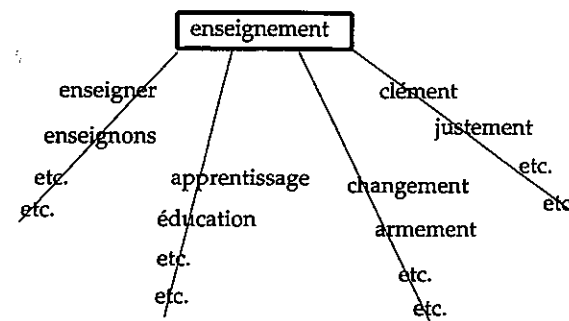
What do these approaches have in common with the morphemic approach and where do they differ? In every case there is a desire to link form and meaning. In the Bloomfieldian approach the morpheme is a unit of form which can be related to a meaning. In the post-Bloomfieldian approach the morpheme is a sign (or a set of signs) with both a form and a meaning side. In the non-morphemic view it is the word-form rather than the morpheme which is the fundamental unit of form, and the meanings associated with the word-form are extra dimensions of lexical connection or resonance.

Ironically enough, this leads us back to earlier views of the morpheme. Meanings were associated with word-forms rather than elements of word-forms from the Classical Greeks right through to the Renaissance (Beard 1994). According to Bloomfield (1935 [1933]: 245), the *gl-* in *glow*, *glare*, *glint* and the *-ash* in *bash*, *clash*, *crash* are 'root-forming morphemes, of vague signification'. He goes on to say that

The analysis of ... the root-forming morphemes is bound to be uncertain and incomplete, because a phonetic similarity, such as, say, the [b-] in *box*, *beat*, *bang*, represents a linguistic form only when it is accompanied by a semantic similarity, and for this last, which belongs to the practical world, we have no standard of measurement. (Bloomfield 1935 [1933]: 246)

Similarly, Saussure (1969 [1915]) deals with morphology without morphemes, and allows 'rapports associatifs' with a given form to be determined on the basis of form, form and meaning or just meaning (1969 [1915]: 175).

Figure 2: 'Rapports associatifs' with *enseignement* (Saussure 1969 [1915]: 175)



Both of these approaches seem to have much in common with the three approaches discussed earlier and to disagree with the more general post-Bloomfieldian structuralist view.

In a similar vein, earlier views of the morpheme view it as a constituent part of a word-form. To the extent that there are morphemes in the Word-and-Paradigm approach at all, the same is true there: there are phonological constituents of the word-form determined by the morphosyntactic make-up of the word. If this is true for Bybee, it seems to be true by accident, rather than being a crucial part of the theory. Constituency is clearly not a part of Hockett's new view of word structure.

One notable difference between the new proposals and the earlier versions of the morpheme relates to the earlier demands for exhaustiveness. In an earlier paper, Hockett (1966 [1947]: 235) speaks of the principle of total accountability, and says

Every morph, and every bit of phonemic material, must be determined by (i.e. predictable from) the morphemes and the tagmemes (if any) of which the utterance is composed.

and later (1966 [1947]: 241) 'every bit of phonemic material belongs to one morph or another'. With Hockett now allowing phonaestemes and the elements of which they are parts to resonate, and the corresponding multi-dimensionality of morphology, there are items which resonate on one tier (as it were) which have no complement within the word-form. In a different way, this is just as clear within Word-and-Paradigm. There a particular piece of phonemic material may be generated only by the presence of two or more morphosyntactic properties, and thus not be uniquely assignable to any particular unit of meaning. The denial of biuniqueness, while it may lead to greater accuracy of description, has made the biggest break between the post-Bloomfieldian tradition and the emergent notions.

The three approaches seem to have rather more in common with the view of the morpheme as a form than the view of the morpheme as a sign. Again, there is irony in the later, more refined position being rejected in favour of the earlier one. In the earlier version of the morpheme, there was meaning associated with the morpheme, but the morpheme did not in itself carry meaning; the form was simply a part of the word-form. Although there are considerable changes to this — the meaning is now being associated with the word-form and the structure of the word-form is purely phonological — this is still rather more like the structures being proposed now than like the post-Bloomfieldian morpheme.

7 Is the morpheme dead?

There is another side to all this that I haven't yet explored. Not having morphemes is consistent with the three new approaches that I have discussed here, and that is the basis on which they are developed. But to a certain extent they are also consistent with having morphemes. There are certain constraints. It should not be the case that all words are to be fully analysed into morphemes. It should not be the case that the only formal and semantic relationships that can be discerned within word-forms should be considered to be due to morphemes. It should not necessarily be the case that morphemes are semantically or formally minimal. However, if we are to accept such modifications to the notion of morpheme, we might be better off thinking up a new term. The term morpheme has already been much abused in the world of linguistics (Mugdan 1986), such extensions might be pushing it rather too far.

Whether or not such a redefinition is possible (and in principle, why should it not be?), it is not clear that is going to solve the problems associated with the structuralist morpheme. So perhaps it should be discarded. But in that case, will the new theories suitably fill its place? Let's consider some of the rapports associatifs / resonances / lexical connections we might wish to consider:

If we consider a word like *grumpiness*, it will clearly have links with other words which have *grump* as a base, words like *grump* itself, *grumpish*. It will have links with words which might be considered to contain the same /gr/ phonaestheme, such as *grouse*, *grudge*, *grunt* and *grumble*, but note that some but not all of these have the extra similarity of the vowel /ʌ/ in the stressed syllable. Then there is the purely phonological similarity of words such as *green*, *grit* and *grunion* on the one hand and *bump*, *lump*, *sump* on the other. Next we find connections with adjectives ending in *-y*, some of which like *bumpy* and *skimpy* share more phonological structure than just the *-y*, others of which like *dreamy* and *lucky* do not. Similarly there are words which end in *-ness*, some of which may also have a preceding *-y* as in *happiness*, and others of which, as with *thankfulness*, will not. More peripherally there are words which are nouns derived from adjectives such as *pomposity*, *importance* and *adjacency*, and yet more peripherally other abstract nouns such as *love* and *music*. Finally, there are words entering into a range of semantic and collocational

relationships with grumpiness, words as diverse as *surliness*, *unsociability*, *cordiality*, and words denoting people of whom grumpiness might be predicated.

While I would not wish to deny that any of these things might be relevant to the use of a word like *grumpiness* in a poetic context, it is far from clear to me that this is a matter of linguistic structure, far less of morphology as it is usually understood.

By being less specific, by not insisting on unique associations of form and meanings, by allowing different kinds of association, this is a much more flexible view of things. That flexibility is a strength, but it is also a weakness: there is no principled limitation on the kinds of things that might resonate. Accordingly, in the position taken by Hockett, morphology is not distinguished from collocational semantics and purely phonological factors such as rhyme or alliteration. Bybee's position is far more restrictive, since it insists on both form and meaning, but must correspondingly run into trouble with suppletion where the formal relationship is weak (despite the strength of the paradigmatic relationship). The Word-and-Paradigm view is more restrictive still, but is much closer to the traditional view of the morpheme as a result.

Again we can try and look at things from a different angle. Morpheme theory gives priority to resonances of certain kinds — ones that are biunique, minimal, and so on. It starts from the elements, and tells us why the resonances are there. Resonance theory starts from the other end, it looks at the resonances, and one type that it finds (though by no means the only type) is the case of a correlation of form and meaning at a level below the word. In that sense, we could say we are looking at the same phenomenon, but from two different ends. Resonance is the macro approach, morpheme theory is the micro approach. There is no reason why two approaches to the same subject matter should not complement each other. Under this view, morphology is simply a special case of resonance. In particular, it is the view that allows you to say that given a set of words like *grumpiness*, *happiness*, *ugliness*, *forgetfulness*, *thankfulness* and so on that the /nəs/ is the bit that makes the words abstract nouns. Despite the weaknesses which have always been present in the notion (or notions) of the morpheme, and despite the strengths of the approaches which do without the morpheme, they are not likely to kill off the morpheme while there is still a use for the micro approach.

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The Adjacency Condition and the Atom Condition: Compatible Morphological Constraints

Jennifer Hay

Abstract

In this paper it is argued that the Atom Condition and the Adjacency Condition are equally valid constraints, despite the fact that they have generally be seen as alternative accounts for the same phenomenon. The Atom condition specifies the features available to a word formation rule, and the Adjacency Condition describes the specific morphological material available. The two constraints are not contradictory, but rather account for separate phenomena. They can therefore co-exist unproblematically.

1 Introduction

The Atom Condition (Williams 1981) and the Adjacency Condition (Siegel 1977) both impose constraints on the material which can be accessed by a word formation rule (WFR). In a number of constructions, the Atom Condition and the Adjacency Condition make different predictions about what material should be available to a WFR. Morphologists have tended to adopt the condition which best suits their data, and attempts to compare the two have inevitably been inconclusive. The typical conclusion is that 'both the Adjacency Condition and the Atom Condition define interesting sets of facts, but that neither succeeds in accounting for the entire domain of observable phenomena' (Scalise 1984: 177). In this paper I will argue that attempts to establish which Condition is correct or superior have been unsuccessful because both conditions are correct. The Atom Condition specifies which features can be accessed by a WFR, and the Adjacency Condition determines which morphological material is available. The conditions are not contradictory, but rather they account for different sets of phenomena, and can co-exist unproblematically.

2 The Conditions

2.1 The Adjacency Condition

Siegel (1977) proposed the adjacency constraint. The following data provides the basis of her discussion:

- 1 *_Aun[_Adis[_Ahonest]]
*_Aun[_Adis[_Acourteous]]
*_Aun[_Adis[_Aloyal]]

- 2 *_Aun_A[discrete]]
 *_Aun_A[dissonant]]
 *_Aun_A[distinct]]
- 3 *_Aun_A[_Vdistract]ing]]
 [_Aun_A[_Vdistinguish]ed]]
 [_Aun_A[_Vdiscover]able]]
 [_Aun_A[_Vdismay]ed]]
 [_Aun_A[_Vdispute]ed]]
 [_Aun_A[_Vdis[_Vhearten]ed]]

Words in which *un-* and *dis-* are in adjacent cycles are ungrammatical, whereas if there is a cycle which intervenes between them, the word is acceptable. This leads Siegel (1977: 192) to formulate the following filter:

Words in *un* are thrown out if the morpheme *dis* is uniquely contained in the cycle adjacent to *un*.

Noting that recent work has revealed several conditions of a similar structure, Siegel (1977: 192) makes this condition more general:

The Adjacency Constraint: No word filter may involve A and SP unless SP is uniquely contained in the cycle adjacent to A.

Here, A stands for an affix, and SP for a property of the substring of the base. Allen's (1978: 155) reformulation of the Adjacency Constraint is the version which is usually cited:

The Adjacency Condition: No rule of word-formation can involve X and Y, unless Y is uniquely contained in the cycle adjacent to X.

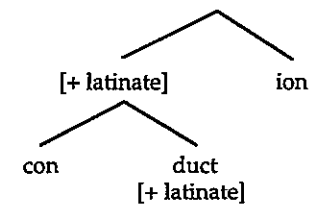
2.2 The Atom Condition

Williams (1981: 253) states the Atom Condition:

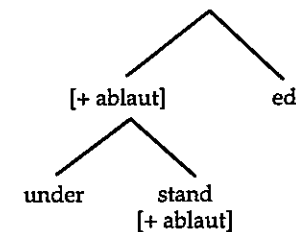
The Atom Condition: A restriction on the attachment of *af_x* to Y can only refer to features realized on Y.

Effectively, this means that the crucial features are those realized on the head. In the same paper, Williams describes the Right-hand Head Rule, which claims that the right-most morpheme in a word is its head. He points out that the Atom condition accounts for a number of phenomena incorrectly predicted by the Adjacency Condition.

The suffixes *-ion* and *-ive* attach productively to [+latinate] verbs. The stems *duct* and *script* are [+latinate], hence the grammaticality of *conduction* and *subscription*. Williams claims that the Adjacency Condition can not account for this because, in the following tree, *duct* is both the stem and the head of *conduct*. According to the Adjacency Condition, *-ion* can only see *con*, and so can't tell that the base is [+latinate].



In current theory, most morphologists would probably claim that *conduct* is not formed by a WFR, and so *conduction* provides a problem for neither condition. The example nevertheless shows how the Atom Condition is intended to work. The second example discussed by Williams is ablaut in English verbs:



The past tense morpheme must have access to the information [+ablaut], otherwise it will predict the incorrect form *understanded*. The Atom Condition correctly predicts that the feature [+ablaut], which is realized on the head, will percolate and be available to the attaching suffix. The Adjacency Condition, however, predicts that the suffix can only see *under*, and therefore does not block the incorrect form.

There are clearly problems with the Atom Condition if it remains coupled with the Right-hand Head Rule. For the condition to be effective, we need a coherent account of headedness. Progress toward such an account now looks promising, particularly with the work published in Lieber (1992). In the discussion that follows, I will assume Lieber's notions of headedness and percolation. I am assuming that, for each category in each language there is a *Categorial Signature* — a set of relevant features which percolate from the head. I also assume that inflectional affixes cannot be heads, but can contribute features through 'back-up percolation' (Lieber 1992: 92).

3 A Synthesis

Note that the wording of the Atom Condition specifically refers to *features* realized on the base. Both of the examples given by Williams are examples of a WFR being able to access features realized on the head of a word. The feature [+ablaut] or [+latinate] percolates from the head to become a feature of the base as a whole. The example offered by Siegel in defence of the Adjacency Constraint illustrates a restriction on the cooccurrence of two

prefixes: *un-* and *dis-*. When *un-* attaches to a word, it can see the features realized on the base as a whole, but the only *specific morphological material* available to it, is the material contained in the immediately adjacent cycle. This is intuitively logical. Morphological material doesn't percolate, so it would be nonsensical to expect the Atom Condition to deal with such cases.

In the remainder of this paper I will present data given by various morphologists which has been used to argue for or against one of these conditions. I will show that the data used to disprove the Adjacency Constraint involves features and can adequately be dealt with by the Atom Condition. Data used against the Atom Condition is data which involves *specific morphological material*, and is dealt with satisfactorily by the Adjacency Condition. The domains of these conditions do not cross. Both are important and necessary.

4 The Domain of the Adjacency Constraint

There has been less evidence presented against the Atom Condition than against the Adjacency Condition. This is because the constraint on availability of features is more widely utilized in word formation than the constraint on morphological material. Evidence used against the Atom Condition falls into three main areas: negative constraints such as that on *[un...dis..] discussed earlier, potentiation and reduplication.

4.1 Potentiation

4.1.1 EN...MENT

Williams (1981:256) acknowledges that, when coupled with the right-hand head rule, the fact that *-ment* is potentiated by *en-*, provides a problem for the Atom Condition. It is now generally accepted that a class-changing prefix can be the head of a word (Zwicky 1985: 2, Bauer 1990: 13), so the Atom Condition can deal satisfactorily with the situation. If we restrict the domain of the Atom Condition to features, then it no longer explains the facts. There is no feature realized on the head which the suffix *-ment* requires. Rather, *-ment* preferentially joins to words headed by *en-*. It must be able to see that particular affix, and recognize it as being the prefix *en-*. It can do this because they are in adjacent cycles, not because *en-* is the head.

4.1.2 a C_α...MENTO

Scalise (1984: 176) discusses a case of potentiation in Italian which he considers problematic for the Atom Condition. The data set in question is given in (4).

4. cogliere	→	*coglimento	'gather'
accogliere	→	accoglimento	'welcome — (a) welcome'
crescere	→	*crescimento	'grow'
accrescere	→	accrescimento	'increase — growth'

battere	→	*battimento	'beat'
abbattere	→	abbattimento	'knock down — knocking down'

This example is similar to the previous one. a C_α potentiates the suffix *-mento*. In this case however, the prefix is clearly not the head of the word. It isn't a category-changing prefix like *en-*. But this is not, as Scalise claims, an exception to the Atom Condition. Potentiation requires the WFR to have access to specific morphological information. In this example, *-mento* is added in the cycle adjacent to a C_α so it can recognise the morpheme. The Adjacency Condition applies here, the Atom Condition is irrelevant.

4.2 Reduplication

Broselow (1983: 342) claims that the Atom Condition cannot describe the facts of reduplication in Lushootseed, and that the Adjacency Condition is more effective in this respect.

The two most common types of reduplication in Lushootseed are distributive reduplication and diminutive reduplication.

Diminutive reduplication has four allomorphs:

- The first CV of the stem
- A copy of the first stem consonant, followed by an [i]
- either of these followed by [ʔ]

The choice of allomorph is lexically governed. The diminutive of the form *bədaʔ* is *bibədaʔ*. So this stem elects the second of the allomorphs listed above. Distributive reduplication copies the first CVC of a nominal or verbal stem. When distributives are formed from diminutives however, the resulting form is not what one would expect.

5. Distributive-Diminutive (Broselow 1983: 325)

		dist.	dim.	stem	
		CVC	CV	CVCVC	
a.	* bədbibədaʔ	bəd	bi	bədaʔ	
b.	* bibbibədaʔ	bib	bi	bədaʔ	
c.	bibibədaʔ	bi	bi	bədaʔ	<i>small children</i>

In this example, the diminutive form is *bibədaʔ*. The distributive affix would usually copy the first CVC of the base, but in this example it only copies the diminutive affix. The only material available for the affix to copy is the material added in the adjacent cycle. Reduplication is clearly within the domain of the Adjacency Constraint and not the Atom Condition.

Reduplicative affixation requires direct access to morphological information.

5 The Domain of the Atom Condition

The Atom Condition applies in all cases where a WFR requires access to features realized on the base.

5.1 Deponent Verbs in Latin

Carstairs-McCarthy (1992: 68) discusses deponent verbs using the following example:

utor = use
utebantur = they were using

Utor is a verb which is passive in form, but active in meaning. This type of verb is labelled 'deponent'. If *utor* were an ordinary, non-deponent verb, then the form for 'they were using' would be **utebant*. The bracketing Carstairs-McCarthy suggests for *utebantur* is:

[[[[[[ut] eba] nt] ur]
use imp. past 3rd pl pass.

The suffix *-ur* is only required to attach because the root is marked [+deponent]. Carstairs-McCarthy considers this a problem for the Adjacency Condition. He doesn't involve the Atom Condition in his discussion, but instead tries to combine the Adjacency Constraint with feature percolation — allowing the feature to percolate out to the brackets enclosing [utebant], and thus be adjacent to the suffix when it joins. To make the Adjacency Condition work in this case, Carstairs-McCarthy must adapt it to resemble the Atom Condition. This is because [+deponent] is a feature, and so the Adjacency Constraint is not the relevant condition to employ.

5.2 Contracted Verbs in Attic Greek

Carstairs (1984: 84) discusses an example from Attic Greek. Most verbs form the present optative with the suffix *-oi-*. But the so-called 'contracted verbs' form the optative differently. These verbs end in a vowel *-a-*, *-e-*, or *-o-* with which the optative *-oi-* merges. The form *timōi* in example (6), is formed by the merging of the stem *tīma* ('honour'), with the optative *-oi-*. In the singular, contracted verbs select a different set of Person-Number affixes than the non-contracted verbs.

6	Non-contracted:	Contracted:
Sg 1	<i>lū-oi-mi</i> , not ' <i>lū-oi-ēn</i> '	<i>timōi-ēn</i> preferred to <i>timōi-mi</i>
2	<i>lū-oi-s</i> , not ' <i>lū-oi-ēs</i> '	<i>timōi-ēs</i> preferred to <i>timōi-s</i>
3	<i>lū-oi</i> , not ' <i>lū-oi-ē</i> '	<i>timōi-ē</i> preferred to <i>timōi</i>

Carstairs considers this example problematic for both the Adjacency Condition and the Atom Condition. The Adjacency Condition cannot explain the data, but we should not expect it to. The root is marked with a feature indicating the conjugation class of the verb. The Atom Condition should therefore apply. The example is only problematic for the Atom Condition if one accepts the strict version of the right-hand head rule, according to which inflectional suffixes are also heads. The system described in Lieber (1992) would clearly interpret the root as the head in this case, as inflectional affixes can never be heads. Information regarding the conjugational class of the verb is therefore available to the adjoining person-number suffix.

5.3 Italian Nominalizations

In his comparison of the two conditions, Scalise (1984:176) points out that Italian nominalizations seem to confirm the Atom Condition. The relevant data is shown as example (7).

7	Scalise (1984: 176)	
	a(c)-centrare → accentramento	'centralize — centralization'
	de-centrare → decentramento	'decentralize — decentralization'
	a(c)-clamare → acclamazione	'acclaim — acclamation'
	de-clamare → declamazione	'declaim — declamation'
	a(p)-prezzare → apprezzamento	'appreciate — appreciation'
	de-prezzare → deprezzamento	'depreciate — depreciation'
	a(d)-durre → adduzione	'adduce — adduction'
	de-durre → deduzione	'deduce — deduction'

The choice of suffix doesn't appear to be phonologically, semantically or morphologically determined. The root is determining which suffix will adjoin. The difference between the examples in (7) and those in (4) is that in the examples in (4) are instances of potentiation. The adjoining affix looks for the presence of a specific morpheme in the base. It seems unlikely that the suffix *-mento* is marked with all the bases it can attach to. Rather, the verbs will each be marked with a feature indicating the endings they can take. These features will percolate and be available to the adjoining suffix. The Atom Condition is the relevant one here, and explains the data perfectly.

6 Problem Data

6.1 Constraints on the Italian Suffix *-mente*¹

A phenomenon discussed in Scalise (1990: 92) seems to contradict both conditions. The suffix *-mente* may be added to the adjective suffixes *-oso*, *-ale*, *-ario*, and *-ico*, but only when the *noun* of the base is [+abstract]:

- 8]_N + suf]_A + mente]
(+abstr)

This example is clearly the domain of the Atom Condition, but the adjectival suffix changes the category of the base, and so must be the head, while the abstract feature is realized on the noun. Scalise describes this phenomenon, but gives very few examples of it at work. The examples given for the suffixes *-oso* and *-ico* are shown in (9).

- 9 Scalise (1990: 90-91)
- | | | |
|------------|-------------------|----------------|
| desertico | → *deserticamente | '*desertly' |
| barbarico | → barbaricamente | 'barbarically' |
| erboso | → *erbosamente | '*grassily' |
| coraggioso | → coraggiosamente | 'boldly' |

It is clear that in the above examples, the adjectives with the [-abstract] base are the ones which cannot take the suffix *-mente*. But there seem to be good semantic reasons why these examples are ungrammatical. How exactly would one do something grassily or deserty? In another section of his paper, Scalise (1990: 91-92) comments:

-mente 'avoids' entire semantic sets: in fact it is not added to adjectives denoting provenance or to adjectives formed by evaluative suffixes or to material adjectives.

Examples he gives in illustration of this last constraint are **cartaceamente* ('paperly'), **argenteamente* ('silverly'), and **salinamente* ('saltily'). This seems to be the same semantic constraint which explains the non-existence of **deserticamente* and **erbosamente*, but without recourse to notions of abstractness.

In fact, one doesn't have to look too hard at all to find counterexamples to Scalise's proposed constraint.

- | | | |
|-------------|---------------|-------------------------------|
| 10 magnete | magnetico | magneticamente |
| 'magnet' | 'magnetic' | 'magnetically' |
| telegrafo | telegrafico | telegraficamente |
| 'telegraph' | 'telegraphic' | 'telegraphically/by telegram' |
| acrobata | acrobatica | acrobaticamente |
| 'acrobat' | 'acrobatic' | 'acrobatically' |

¹ I am grateful to Gary Johnson for his invaluable help with the examples in this section.

Italia	italiano	italianamente
'Italy'	'Italian'	'in an Italian manner'

The adjectives in (10) all have [-abstract] bases, and yet allow affixation of *-mente*. Scalise's suggested constraint therefore cannot hold. While it is true that there are a large number of words which do behave in the way he describes, this is for semantic reasons, rather than because of features realised on the base.

6.2 *Un-* Revisited

Allen (1978) discusses *un-* prefixation. She amends Siegel's (1977) restraint on *un-* and *dis-* to a more general constraint:

Condition on *un-* prefixation (Allen 1978:46):

Un- may not attach to a word which has negative content in the cycle adjacent to *un-*.

In order for *un-* to know whether the adjacent cycle has negative content, the relevant morphemes must carry a feature to that effect. This constraint, therefore, is an example of the Adjacency Constraint used to restrict features available to a WFR — which is clearly at odds with the hypothesis under discussion. Allen deems this condition necessary because of the non-occurrence of *un-* in cycles adjacent to *dis-*, *mal-* and *-less*, and because of the ungrammaticality of the following words:

- 11 (Allen 1978: 46)
- * unbad
 - * unhorrible
 - * unevil
 - * unghastly
 - * unugly

These examples show that there are clearly constraints on *un-* prefixation. Opposites of these examples, however, are also unacceptable — **ungood*, **unwonderful*, **unnice*, **unfantastic*, **unbeautiful* — so the non-existence of the examples in (11) are hardly proof of a negative constraint. Moreover Zimmer (1964: 36) lists a number of examples which do have negative content in the base. These include *uncorrupt*, *unselfish*, *unsordid*, *unvicious* and *unvulgar*. As Zimmer points out, there are huge constraints on *un-* affixation to simplex forms. One reason for this is that simplex forms often have antonyms, and so there is a blocking effect on *un-*. The affix is far more productive with derived bases. Zimmer (1964: 85) summarizes the situation as follows:

...we must differentiate between two kinds of acceptability: acceptability in terms of a particular form (e.g. *unkind*) and acceptability in terms of a particular pattern (e.g. *un-x-able*). In terms of the linguistic behaviour of individual speakers it is clearly only the second kind of acceptability which implies productivity.

Baayen and Lieber (1991) used corpus data to produce a quantitative analysis of the productivity of a number of English affixes. The formula used to

calculate productivity is $P = n_i/N$, where n_i is the number of words with the relevant affix occurring exactly once in the sample, and N is the total number of tokens of all words with that affix.

Table 1² shows the number of different words, and their frequency of appearance in the corpus. Baayen and Lieber's formula for productivity, gives *un-* prefixation with simplex forms a productivity score of 0.0000. *Un-* prefixation to bases in *-ed*, *-ing* or *-ful*, however, has a productivity score of 0.0019.³

Table 1:

Summary of frequency distributions of *un-* when added to simplex bases and bases in *-ed/-ing/-ful*. r = frequency. (Baayen & Lieber 1991: 825)

	SIMPLEX	-ED/-ING/-FUL
$r = 1$	0	6
$r = 2$	0	7
$2 < r \leq 10$	1	31
$10 < r \leq 100$	9	62
$r > 100$	8	5
Total	18	111

Lieber (1992: 9) points out that a theory of word-formation is only responsible for accounting for those processes which are productive. This being the case, we must only account for *un-* prefixation to derived bases, and not to simplex ones. Which brings me, finally, to the point. Simple adjectives which are prefixed with *un-* are listed in the lexicon. These forms are analyzable, but not formed by a WFR. We don't need, therefore, to account for the presence or absence of negative content in their base. Words such as *undisturbing*, *unencumbered* and *unlawful* are formed by WFRs. The only restriction on such forms is that *un-* does not attach to words beginning in *dis-*, *mal-* or ending in *-less*. This can be dealt with by an expansion of the constraint in Siegel (1977) and in keeping with the Adjacency Constraint.

² These figures have been extrapolated from a bar graph given in Baayen and Lieber.

³ As an indication of how this score compares with other affixes, *-ity* and agentive *-er* each scored 0.0007, *-ish* (from adjectives) scored 0.0034 and *-ness* scored 0.0040.

Condition on UN- affixation (Siegel 1977: 192) — modified
Words in *un* are thrown out if the morphemes *dis*, *mal* or *less* are uniquely contained in the cycle adjacent to *un*.

This rule is compatible with the current hypothesis. It requires specific morphological material to be available in an Adjacent Cycle.

6.3 Zulu

Carstairs (1984: 84) discusses data from Zulu which he deems problematic for both conditions. The Zulu passive is formed by the suffix *-wa*:

12 Active:		Passive:	
uyabona	'he sees'	uyabonwa	'he is seen'

In Zulu *-w-* can never follow a labial consonant. Thus, when the passive *-wa* suffix is added, a preceding labial is dissimilated:

13 Active:		Passive:	
uyahlaba	'he stabs'	uyahlatshwa	'he is stabbed'
uyabamba	'he catches'	uyabanjwa	'he is caught'

Doke (1973: 136) states the rule as follows:

Verbs, of which the final syllable of the simple stem begins with a bi-labial consonant, change that consonant to the corresponding prepalatal, according to the Zulu rules of palatization, before suffixing *wa*.

What Carstairs considers problematic is the fact that this dissimilation occurs even when the causative suffix *-isa-* intervenes between the root and the passive:

14 Active:		Passive:	
uya-hlab-is-a	'he causes to stab'	uya-hlatsh-is-wa	'he is caused to stab'
uya-bamb-is-a	'he causes to catch'	uya-banj-is-wa	'he is caused to catch'
uya-khumbul-is-a	'he causes to remember'	uya-khunjul-is-wa	'he is caused to remember'

Doke (1973:137) summarizes the phenomenon neatly:

Palatization persists in the formation of passives even when the bi-labial is no longer in the final syllable of that word, if that word is a derivative of a simple stem which contains the bi-labial in its final syllable.

There is therefore a consistently defined environment in which the change takes place, and so the process could be dealt with by a phonological rule. This rule would have to include simple morpheme boundaries. Herbert (1977: 23) suggests the phenomenon is non-phonological. Even if non-phonological, this process does not provide counter-evidence to the hypothesis under discussion. The Adjacency Condition and the Atom Condition restrict the information available to a WFR. In this example, it is

the root containing the bilabial which requires the information, and not the suffix *-wa*. Whether a morpheme can access information added in later cycles is a separate issue. As this example is restricted to a particular grammatical category, perhaps Lieber's 'back-up percolation' holds a clue.

7 Conclusion

It has been shown that, far from being contradictory conditions, the Atom Condition and the Adjacency Condition account for different data, and are both necessary. Scalise (1984: 177) has already pointed out this difference:

It should be noted that there is a basic difference between the AdC and AtC which makes the two difficult to compare. Specifically, the AdC establishes which morphological material can be referred to by an affix, while the AtC establishes which features can be referred to by an affix. As a result, the AtC cannot make reference to specific morphemes, while the AdC allows this to be done.

Somehow, Scalise misses the significance of this comment. It follows a lengthy discussion, and several examples intended to illustrate the 'superiority' of one or other of the conditions. The conditions each make some correct and some 'false' predictions, and so Scalise's conclusion is that 'at present, therefore, it is difficult to choose one condition over the other' (Scalise 1984: 177). This paper has shown that this is a choice we do not need to make. Most counter-evidence for each of the conditions comes from outside of its domain. Other examples which have been considered problematic for both conditions, have been shown to be compatible with the relevant constraint.

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V-movement and Optionality

Elizabeth Pearce

Abstract

In Pollock (1989) a number of cases of differing surface positions of adverbs in relation to a verb are accounted for in terms of optionality of V-movement. In the Minimalist Programme (MP) of Chomsky (1993) optionality of head-movement is ruled out by the principle of economy. Furthermore, the MP requires that head-movement be morphologically motivated.

This paper attempts to bring these notions to bear on a reassessment of the use of optional V-movement in the description of surface orderings in French infinitives in Pollock (1989). It will show (i) that $V_{\text{Infinitive}}$ -Adverb versus Adverb- $V_{\text{Infinitive}}$ ordering is derived by semantically motivated Adverb shift rather than by optionality of V-movement; and (ii) that $Aux_{\text{Infinitive}}$ -*pas* versus *pas*- $Aux_{\text{Infinitive}}$ ordering is an effect of diachronic change in the properties of the negative head realized as *ne*.

1. Optionality

Since Pollock (1989), it has been a working assumption that the relative positions of verb and adverb in the clause can provide an essential clue as to whether V-movement has taken place or not.

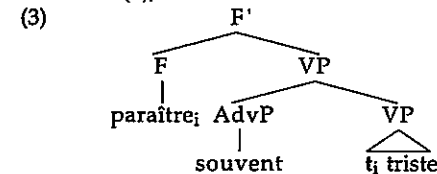
Thus, in the contrast between English and French shown in (1) the French *embrasse* in (1b) has moved up to a functional head position in the Syntax, whereas in (1a) *kisses* remains in the VP and the inflectional elements are either lowered to the verb or, alternatively, they are inserted with the verb directly in the D-structure (with subsequent LF matching as in Chomsky (1993)).

- (1) a. Mary often kisses John.
 b. Marie embrasse; souvent t_i ; Jean.

Certain other cases are said to indicate optionality of V-movement in French:

- (2) a. [Paraître_i souvent t_i triste] n'est pas bon pour la santé.
 b. [Souvent paraître triste] n'est pas bon pour la santé.
 [to appear often/often appear sad] is not good for the health

For Pollock (1989), the alternative verb-adverb ordering in (2a) and (2b) is attributed to optionality in the movement of the verb to a functional head above the VP in the nonfinite clause. The relevant part of the structure is shown in (3), in which F is a functional head:



Such use of an optional rule application in the syntax would appear to run counter to the proposal of Chomsky (1993) (henceforth the 'Minimalist' view), that all rule applications must be semantically or morphologically motivated. Thus, whilst the possibility of head-to-head movement is universally available in the terms of the framework, we must be able to provide a motivation as to whether or not such movement applies in a given case.

In the particular case of French, the motivation for obligatory V-movement in the finite clause, as in (1b), is defined as a function of the morphological properties of the inflectional element or elements. The extension of the same motivational criteria for V-movement in the nonfinite clause suggests that: either a [-finite] F has the properties that induce V-raising and the V raises, or that the [-finite] F does not have the properties that induce V-raising and the V does not raise.

A third possibility, however, is that the morphological properties of the [-finite] F are not uniquely determinable, that speakers may vary in their conclusions as to the properties of the inflectional category, and that the alternations in the surface forms reflect the differing analyses which speakers apply to the data that confronts them. Such a view as to the interpretation of surface alternations is perfectly consistent with the account that the grammar provides of the kind of reanalyses which must occur in diachronic syntactic change: within a given period of time, it is possible that the data of a single speaker will reflect differing resolutions as to the properties of a particular construction.

A possible approach to the treatment of the data in (2) is to say that such is the case for the optionality exhibited in the contrast between (2a) and (2b): the alternations in the surface forms would reflect a degree of indeterminacy in the properties of the French [-finite] F shown in (3).

*For their help in sharing with me their intuitions about French, I wish to thank France Klijn, Jacqueline Ferry and the students in the Maîtrise d'anglais (1993-94) at the Université Française du Pacifique, Tahiti. An extended version of this paper was presented at the meeting of the Australian Linguistics Society at La Trobe University, July 1994. My thanks to the participants for their comments. Thanks also to Paul Warren for helpful comments on an earlier draft. Any errors of fact or interpretation are my own.

However, in what follows I will show that the data in (2) is not about optionality of V-movement, but rather it is about an 'optionality' in the surface position of the adverb.

In the terms of the Minimalist requirement that applications of syntactic processes must be morphologically or semantically motivated, I will propose that it is the semantics of the adverb which is the motivating force behind the differing positions of adverbs relative to the verb. I will also show that the optional V-movement account for (2a,b) is untenable when the placement of clitic pronouns is taken into consideration. Finally, for yet another case in French, I propose a morphologically driven account for the apparent optionality of auxiliary verb movement in the presence of *ne* in nonfinite clauses.

2. V-movement

The contrasts in (5) below lead to an alternative account of the differing positions of the nonfinite verb and the adverb in (4):

(4) a. (?) Complètement résoudre ces problèmes, c'est difficile.

b. Résoudre complètement ces problèmes, c'est difficile.

'To resolve these problems completely is difficult.'

(5) Ces problèmes,

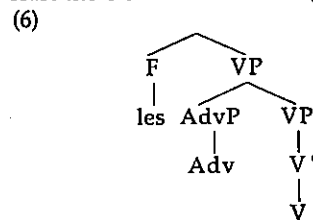
a. (?) complètement les résoudre, c'est difficile.

b. *les complètement résoudre, c'est difficile.

c. les résoudre complètement, c'est difficile.

'These problems, to solve them completely, is difficult.'

A generally accepted view (Kayne (1989), for example) is that the clitic pronoun in the French clause must attach to a functional projection above the VP. That is, we must assume that the structure of the clause includes at least the elements shown in (6):



Given a structure like (6), (5c) is derived if V-movement to F applies. The ill-formed (5b) can be derived if no V-movement takes place. (5a) can

be derived only if we allow for the possibility of a repositioning of the adverb, whether or not V-movement takes place.

In Pollock's account of the nonfinite clause in French, F is the lowest functional projection, and, under Pollock's schema, (5b) and (5c) would both be derived and (5a) is not accounted for. If on the other hand, we view V-movement to F as obligatory, then we derive (5c), we cannot derive (5b), and, once again, we can derive (5a) only if we allow for the possibility that the adverb can move.

Thus, under both accounts, a repositioning of the adverb is required in the derivation of (5a), but the obligatory V-movement approach does not derive (5b), whereas the optional V-movement account does derive (5b).

The conclusion at this point, therefore, is that obligatory V-movement gives the optimal account of the data and that the available alternations in surface ordering should thus be attributed to differing possibilities for adverb placement. I will next show that the variable adverb placement approach is supported by aspects of the syntax and semantics of adverbs.

3. Adverb placement

First there are a variety of positions that adverbs can occupy in the sentence which cannot be accounted for through V-movement alone. Second, there are reasons to suppose that differing adverb positions match with differing semantics, at least in some cases.

Laenzlinger (1993) gives the following examples indicating different possible locations for the adverbs they contain:

(7) a. (Souvent,) Jean a (souvent) frappé (souvent) ses ennemis (souvent).

'Jean has often struck his enemies.'

b. (*Entièrement,) Jean a (entièrement) lu (entièrement) le livre (entièrement).

'Jean has read the book entirely.'

Although it is conceivable that the immediately pre- and post-verbal positions for the adverbs, could be explained in terms of V-movement, the phrase initial and final positions require another device. So, in the finite clause, we need to allow for variable positions for adverbs.

In the presentation of example (5a) above, the use of the rating '(?)' reflects a degree of variability in speakers' reactions to sentences like (5a). Reactions to (5a) are intermediate between those for (5b) and (5c). At the same time, speakers report that when the adverb precedes the infinitive, as in (5a) and in (4a), it is more strongly focused than it is in the other positions. What these two points suggest is that the intermediate rating in

the relevant cases should therefore be interpreted as indicating a more marked construction in which the adverb is focused.

Some further evidence in support of the view that adverbs receive differing interpretations according to their positions is provided by the analysis of Laenzlinger who shows the following contrasts in scope interpretation (Laenzlinger 1993: (85) and (86)):

(8) a. L'électricien souhaite [lentement tester toutes les ampoules].

b. L'électricien souhaite [tester lentement toutes les ampoules].

'The electrician wishes to slowly test all the bulbs/test all the bulbs slowly.'

Following Laenzlinger's interpretation of (8a,b), in (8a) it is the testing of all the bulbs that is intended to be slow, whereas in (8b) it is the testing of each bulb in turn which is intended to be slow.

(9) a. Jean aimerait, selon moi, [souvent parler à quelqu'un].

b. Jean aimerait, selon moi, [parler souvent à quelqu'un].

'Jean would like, according to me, to often speak/speak often to someone.'

In (9a) *quelqu'un* can have wide scope over *often*, but in (9b) *quelqu'un* only has narrow scope.

In Laenzlinger's analysis of these particular cases the alternative positions of the adverbs reflects differences in their D-structure positioning. In other words, the scopal interpretation reflects the adverb function and, as a consequence, its position, rather than it being a function of whether or not V-movement has applied.¹

To summarize: first, the optionality account of V-movement in French infinitives does not work when it is applied to constructions including clitic pronouns. Second, (i) adverbs have a freedom of positioning that cannot in all cases be related to V-movement effects, and (ii) there are factors of semantic interpretation that relate to differing adverb positions. On the basis of these two main aspects of the analysis, I conclude that there is no good evidence for optionality of V-movement and that differing relative orderings of Verb and adverb must be attributed to the syntax of the adverb positioning.

¹ Note also that there are limits as to the functions of different types of adverbs as evidenced by contrasting constraints on the positions available (shown, for instance, by (7a) versus (7b)).

4. V-movement and Auxiliary Verbs

Consider now a second case which has been proposed as involving optionality of V-movement in French infinitives, and for which, I propose that what is involved is indeed optionality of V-movement, but that, here, the optionality can be seen as morphologically driven. The case to be considered is that illustrated by the alternations shown in (10) and (11) from Pollock (1989):

(10)a. Ne pas avoir de voiture en banlieue rend la vie difficile.

'Not to have a car in the suburbs makes life difficult.'

b. N'avoir_i pas t_i de voiture en banlieue rend la vie difficile.

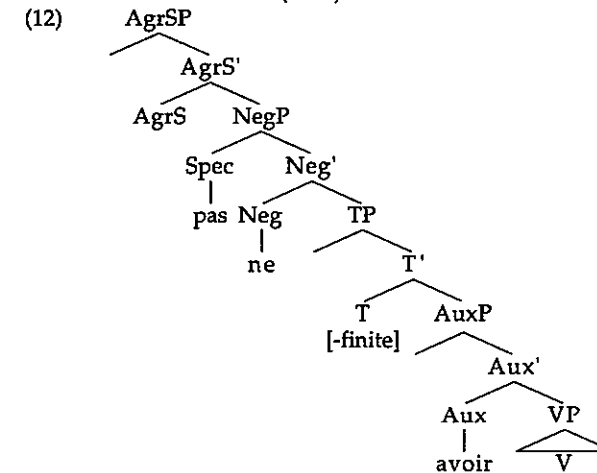
(11)a. Ne pas posséder de voiture en banlieue rend la vie difficile.

'Not to possess a car in the suburbs makes life difficult.'

b. *Ne posséder_i pas t_i de voiture en banlieue rend la vie difficile.

In (10) we see that the auxiliary verb *avoir* may either follow or precede the negative *pas*, in contrast to the non-auxiliary verb *posséder* in (11) which may only follow *pas*.

In order to consider the nature of the contrast between (10a) and (10b), we need to see what exactly are the relevant assumptions as to the structure of the clause, in particular with regard to the position of the negative elements. The structure shown in (12) is in accordance with the essentials of the treatment in Belletti (1990):



The structure in (12) gives the details for the 'top' part of the IP. AgrSP is for Subject agreement and we will assume here that [-finite] under T is the

location for the infinitive morphology. In other words T is the functional projection labelled as 'F' in (3), in the sense that it is T that is the landing site of the movement of the infinitive verb. In NegP, *ne* is the head and *pas* occupies the Specifier position.

In the derivation of the sentence, *ne* raises to the AgrS head. Without attempting a precise account of the reasons for this movement, we can say that it is a general characteristic of clitics that they must raise to a functional head position. When *avoir* raises to T, we thus obtain the *ne pas avoir* ordering in (10a), and similarly for (11a) the *ne pas posséder* ordering.

Now, in (10b) we have the additional ordering with *n'avoir pas*. In (10b) the auxiliary verb has also raised up to the AgrS head. In other words, the contrast between (10a) and (10b) indicates an optional implementation of V-movement.

My analysis of the optionality of V-movement for the auxiliary as indicated by (10) is based on an interpretation of the morphological properties of the *ne* head: there are good reasons to suppose that the properties of *ne* have been and continue to be subject to reinterpretation.

In contemporary spoken French *ne* is usually omitted in finite clauses:

(13)a. Ils ne savent pas la réponse. [written]

b. Ils savent pas la réponse. [spoken]

'They don't know the answer.'

For Moritz (1989) the possibility of omission of *ne* in infinitives is subject to conditions relating to the syntactic role of the infinitive phrase. The sentences and grammaticality ratings for (14a) and (14b) are taken from Moritz (1989):

(14)a. [?(Ne) pas se révolter dans sa jeunesse] témoigne d'un esprit triste.

'Not to revolt in one's youth is evidence of a sorry mind.'

b. Il est triste de [?(ne) pas se révolter dans sa jeunesse].

'It is regrettable not to revolt in one's youth.'

The ability to omit *ne* appears to have extended even further in Québécois (see Muller 1991: 262-3).

These facts provide us with clear evidence of variability in the morphological properties of *ne*. The register and speaker variability are such that the alternations in the surface orderings can be seen as an effect of

differing speaker analyses as to the role of *ne* in promoting or inhibiting the raising of the auxiliary.²

To conclude, I have shown that two cases of supposed optionality in V-movement in French infinitives can be seen as conditioned by two distinct factors. On the one hand, the alternations in ordering of adverbs and nonfinite lexical verbs, as in (2), is attributed to adverb placement rather than to alternative possibilities for V-movement. On the other hand, the alternative orderings for the negative adverb *pas* and auxiliary verbs is seen as a function of the morphological properties of the negative head *ne*.

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² For analyses of the historical evidence with respect to changes in the forms of negated infinitives in French, see Hirschbühler and Labelle (1992), Martineau (1994), and Pearce (1993). The syntactic/morphological properties of *ne* are also treated more extensively in Pearce (1993).

The sound of *desert trains*: delay strategies and constraints in spoken sentence processing¹

Paul Warren

Abstract

Data from an eye-movement study (Frazier & Rayner 1987) were put forward as evidence for a delay strategy in the processing of syntactic category ambiguities (as in ...*the desert trains*...). These results have since been reassessed (MacDonald 1993) in the context of an approach to sentence processing which assumes that language users develop probabilistic constraints on the possible interpretations of sentences. This paper examines critically some of the theoretical linguistic assumptions underlying the original study, as well as the interpretation of some of the eye-movement data. I then discuss the possible disambiguation of such ambiguities in the spoken domain, drawing on both acoustic-phonetic data and phonological theory in support of an interactive, and potentially constraint-based approach to spoken sentence processing.

Introduction

In a study of sentence processing in reading, Frazier and Rayner (1987; henceforward F&R) argue that strings such as *the desert trains* contain a category ambiguity in the interpretation of *desert* as either a noun or an adjective². Since the processor is sensitive to this category ambiguity, it delays making any structural interpretation of this word and awaits further input information. The following word *trains* is also category ambiguous, between verb and noun, though the selection here is not independent of that of *desert* as noun or adjective. Because of the continuing ambiguity, interpretation is delayed further, until the next word, which — in F&R's materials at least — resolves the ambiguity.

Such materials are therefore processed in a different way from many of the other sentence types investigated by Frazier, Rayner and colleagues (e.g. Frazier & Fodor 1978; Frazier & Rayner 1982; Rayner, Carlson & Frazier 1983; Rayner & Frazier 1987), most of which have been studied within the general framework of a parser that commits itself early on to a 'default' structural analysis, i.e. to one that either minimises the structural complexity of the

¹ My thanks go to Esther Grabe for comments on the phonological prosodic analysis adopted in this paper.

² MacDonald (1993) assumes, without discussion, that the ambiguity of *desert* in this phrase involves usage of this word as modifying vs head noun, rather than as derivative adjective vs noun. Further motivation for this analysis is given below. Note that while this does not remove the ambiguity at *desert*, it does change its nature, which has clear implications for F&R's distinction between lexical and syntactic ambiguity, as also discussed below.

current parse (following the principle of Minimal Attachment) or adds new input into an existing constituent rather than opening a new one (following the related strategy of Late Closure). F&R argue that the processing delay claimed for *desert trains* and other similar materials is an example of a general principle that applies whenever the processor has to operate on ambiguities that reside in prestored analyses rather than resulting from structural analysis. Thus the category ambiguity in *desert* is a lexical rather than a structural ambiguity. A similar explanation is proffered for semantically ambiguous lexical forms and for lexical (thematic) frame ambiguities.

In this paper I argue against F&R's interpretation on two counts: first there is an alternative explanation of their reading-time data, as presented in MacDonald (1993) and expanded on here; second, their linguistic analysis of sequences such as *desert trains* is neither unproblematic nor parsimonious. I then present a consideration of the spoken forms of such ambiguities, which has a bearing on both the linguistic status and the parsing of such sequences.

Delay vs constraint

The evidence cited by F&R for a delay strategy operating in the case of their category ambiguous items comes from reading times, in an eye-movement study, for ambiguous sequences such as *desert trains* and the following disambiguating region, compared with reading times for the same sequences when already disambiguated by a preceding determiner (*this*, forcing the noun-verb reading, and *these*, forcing the adjective-noun reading). Example materials are given in Table 1.

	precursor	ambiguous region	disambiguating region
<i>ambiguous</i>			
a) N-V	I know that the	desert trains	young people to be especially tough
b) A-N	I know that the	desert trains	are especially tough on young people
<i>disambiguated</i>			
c) N-V	I know that this	desert trains	young people to be especially tough
d) A-N	I know that these	desert trains	are especially tough on young people

Table 1: Example set of experimental materials used by F&R.

If the ambiguities were structural rather than lexical, it is argued, then in a comparison of ambiguous with disambiguated versions, we should expect longer reading times in the disambiguating region of b) and not a). This is because the 'default' structural analysis expected by F&R on other grounds would be of *desert* as a noun and the string *desert trains* as noun+verb, which would lead to a 'garden path' or misanalysis in b), as becomes clear once the disambiguating region is read. At this point re-analysis is required,

resulting in longer reading times. The misanalysis would only be found in b), since c) and d) are disambiguated by prior information, and the 'disambiguation region' in a) confirms the default structural interpretation.

However, F&R found that reading times in the disambiguating region were longer for both a) and b). Also, reading times in the ambiguous region were longer for the disambiguated items (c, d) than for the ambiguous items (a, b). Their interpretation of this pattern of results is that readers are aware of the category ambiguity in *desert trains* in a) and b) and therefore do not make a structural analysis of this phrase until disambiguating information is available, and hence the shorter reading times in the ambiguous region (where structural analysis is not going on) and longer reading times in the disambiguating region (where analysis can proceed), when compared with c) and d).

MacDonald (1993) explores an alternative explanation of F&R's result, pointing out that the use of *this* and *these* is deictically marked in isolated sentences such as c) and d), as no previous reference to *desert* has been made. The phrases *this desert* and *these deserts* are therefore infelicitous and attract longer reading times, causing the reading-time difference between ambiguous and unambiguous items in the ambiguous region. In support, MacDonald presents data from her own experiment based on a selection of F&R's materials, in which she finds that the determiner effect remains even when the phrase is made inherently unambiguous by changing the morphological form of *desert* or *train*, as in *I know that the/these deserted trains...* and *I know that the/this desert trained...*

The immediacy of this infelicity would appear to be supported by a significant interaction reported by F&R between ambiguity and what they call 'word order' (i.e. first or second word in the ambiguous region), as shown in Table 2.

		word1	word2	mean in region
	e.g.:	<i>desert</i>	<i>trains</i>	
ambiguous	<i>the</i>	35.25	46.00	40.63
disambiguated	<i>this / these</i>	40.25	46.75	43.50

Table 2: reading times (ms/char) for words in the ambiguous region of ambiguous and disambiguated versions of sentence materials used by F&R. This table pools data from their experiments 1 and 2. In this pooled data both main effects and the interaction are significant, as reported by F&R.

It is clear that the effect of longer reading times for disambiguated materials can be attributed almost exclusively to the first word of the ambiguous region, e.g. to *desert*, suggesting that the effect of determiner infelicity is immediate and short-lived. However, MacDonald does not explore this interaction, nor does she find such an interaction in her own

experiment (which may be in part a consequence of the different set of conditions used by her). F&R find a further, three-way interaction involving also the A-N/N-V distinction, which is discussed further below.

In further experimentation, MacDonald subsequently demonstrates a range of semantic effects in the processing of similar category ambiguities, effects which she argues are best accounted for by a set of probabilistic constraints on interpretation. These effects include the relative frequency of words like *desert* and *trains* in different category forms, the frequency of these forms in combination, and the combinatorial semantics of such phrases. All of these (and other) sources of information affect the likelihoods of potential interpretations of the sentences and consequently affect reading times. Unlike F&R, MacDonald does not see these ambiguities as requiring a different processing strategy from previously studied structural ambiguities, since lexical, pragmatic, structural and other information sources all act as probabilistic constraints on interpretation. Further below I argue that prosodic and intonational distinctions should be added to this list of constraints when dealing with spoken language comprehension.

The linguistic analysis

As noted above, F&R's linguistic treatment of items like *desert trains* assumes that the analysis of *desert* involves a category ambiguity. While admitting that the linguistic analysis of such items is not clear, F&R maintain that a sequence like *desert trains* must be either adjective-noun or noun-verb, and cannot be treated as a compound, since 'English does not permit novel compounds to be generated without compound stress (stress on the left-hand member of the compound) [... and ...] compound stress is not obligatory on these phrases.' (F&R, fn.1). F&R's position is threatened by two considerations — one concerns their insistence that stress must be on the left element of compounds, and the other has to do with the interpretation of *desert* as an adjective rather than as a modifying noun in the phrasal interpretation.

The assumption that sequences like *desert trains* cannot be compounds because of their stress pattern is a false one — for example, Bauer (1983a, 1983b) has argued that the notion of compound stress is not as clear as is often maintained, and that there are many factors that influence the assignment of stress in compounds. These can include contextual effects, such as prosodic environment (Bolinger 1955), but even in citation form, compounds may be right-stressed, if lexically conditioned by the head noun (Bauer 1983b — citing Lees 1968 — compares *apple pie* and *apple cake*), and some may have 'double stress', such as *bánk hóliday* (Bauer 1983a: 104ff). Sampson (1980) also notes a tendency, in RP at least, for stress to be on the second item. This trend clearly goes against F&R's assumption of left-hand compound stress. As evidence for variation, both between different items and between (expert) native speakers, Bauer (1983b) presents data from 10 subjects, who were asked to decide whether collocations (including 43 noun+noun) should receive initial, final or level stress. Overall, the data

for the noun+noun items (extrapolated from Bauer's Appendix) show that subjects assign initial and final stress in about equal proportion (27% and 24% respectively), but more often favour level stress (48%). For any single item, there is rarely clear agreement between the 10 subjects. F&R's criterion of stress position as a determinant of whether noun-noun sequences are compounds thus seems rather insecure. It is also not based on fact, since they provide no evidence that their items cannot be regarded as compounds, nor that they are consistently right-stressed.

The second problem with F&R's linguistic analysis of their ambiguous sequences is their insistence that the first word is category ambiguous. In part this relates to the discussion of compound stress, since if some of the test sequences can be compounds (with or without left-stress), then the first word can be a noun in both readings of the ambiguity. Additionally however, there is the possibility of an alternative, modifying-noun interpretation of the first word, as assumed by MacDonald (1993), who presents the contrast in *desert trains* as one between [modifying noun + head noun] and [head noun + verb]. It follows that there is a category ambiguity involved in these materials, but it affects the second element (*trains*), and not the first. The ambiguity in the first element then follows from the fact that any noun can be either the head of a noun phrase or a modifier of a following head noun.

Of course, such observations do not necessarily affect the hypothesis of a delay strategy in processing the ambiguities, but if *desert* and other words in this position in F&R's experiments are nouns in both constructions, then the ambiguity is no longer one of syntactic category, but involves structure. That is, readers process *desert* as a noun, but do not know whether it is a single noun or the first element of a compound. If this is the case, then it undermines the distinction made by F&R on the basis of these ambiguities between first-pass analysis of structural ambiguities and delayed analysis of lexical ambiguities.³

Word-by-word analysis

In the preceding discussion I have assumed that readers process the textual input word-by-word as it is read, and therefore start to make judgements about the interpretation of *desert* before they read the following word. These judgements may subsequently influence or be influenced by following material, in developing a sentence- and utterance-level interpretation. Much the same approach is taken by MacDonald (1993), who in a further experiment involving corpus data assesses the contribution to ambiguity resolution of the frequency of occurrence of items like *desert* as head nouns and as modifying nouns, of items like *trains* as verbs or nouns, and also their relative co-occurrence. She also measures the relative

plausability of the combination of the two words as [modifying noun+head noun] or [head noun+verb] by using a completion task, in which subjects wrote down a completion for sentence fragments ending in the ambiguous phrase. A similar task was used by F&R to assess the likelihood of interpreting *desert* and equivalent words as adjectives or nouns in the ambiguous versions. For the purpose, however, of interpreting the reading time data and developing an understanding of what subjects are doing as they read the text word-by-word, we need also to assess the plausability of *desert* as a modifier or as a head noun before the next word is read.

Let us assume that the contribution of each word to the interpretation is indeed assessed as that word is read, and consider now the process of interpreting the four versions of F&R's materials, at the point at which *desert* is encountered. In the ambiguous cases in a) and b), the interpretation of *desert* is unclear — it can be either a modifying noun or a head noun, as discussed above. Interpretation here may be influenced by the relative frequency of *desert* in these two roles, as well as by its plausability in these roles in the context in which it is read. At this point, however, it is unlikely to be influenced by the frequency of *trains* as verb or noun, and presumably not greatly by combinatorial factors to do with the sequence of *desert trains*. It is therefore surprising that F&R assess the adjective / noun interpretation of *desert* by presenting subjects with the complete ambiguous sequence, but perhaps less surprising that they subsequently find no effect, in a reanalysis of reading times, of the adjective / noun bias measured in this way.

Overall, F&R found a 52% bias for the noun (vs adjective) reading in their first two experiments (data extrapolated from their Appendixes 1 & 2). To test whether readers' preferences for the two interpretations of the first word in the ambiguous sequence might initially be very different, I asked 12 naive subjects to perform a continuation task on the materials from F&R's experiments 1 and 2, presented just to the first word of the ambiguous region (*desert*). One item was not used here, namely *pants suit*, which is not a usual collocation for the British English subjects used. The results were very different from F&R's: there was a bias for the noun reading of 86%, compared with 53% for the same set of 31 items in their data (for details, see Appendix 1). Clearly, subjects' preference, when reading a phrase such as *I know that the desert...*, is to interpret *desert* as a head noun. There is a clear implication of this result for their re-analysis of the reading time data.

Consider now the disambiguated versions of F&R's materials. In c), just as in a) and b), the interpretation of this first word of the ambiguous region remains unclear — *this desert* could continue *this desert trains young people* or *this desert oasis is very welcome*. Clearly the disambiguation achieved by F&R in using *this* here is of the whole phrase (*this desert trains*), and not of the first word alone. I argue that the only version of F&R's materials in which readers will in fact know the correct structural assignment on reading the first word of the ambiguous phrase is the 'disambiguated adjective-noun' version in d) in Table 1, where the

³ Of course, certain frameworks, e.g. categorial grammar, treat all ambiguities as category ambiguities. Thus *desert trains* will require a choice between N and N/N interpretations of *desert*, just as, say a transitivity ambiguity in a closure context (*Before the king rides his horse, ... vs Before the king rides, his horse ...*) requires a category choice at the verb, here between AdvP and AdvP/NP (i.e. the adverbial phrase is complete or requires a NP to become complete — cf. e.g. Briscoe, 1987).

combination of plural determiner and singular noun makes it clear that the noun can not be the head noun of the phrase.⁴ Similarly, while MacDonald (1993: 696f.) suggests that the two disambiguated versions of F&R's materials probably have the same point of disambiguation, i.e. the second word of the ambiguous phrase, it is clear from her argumentation that this applies to the whole phrase only, and that she too sees differences in the interpretation of the first ambiguous word, which is a modifier in *these desert*, but ambiguously modifier or head noun in *this desert*, though she does not pursue this contrast.

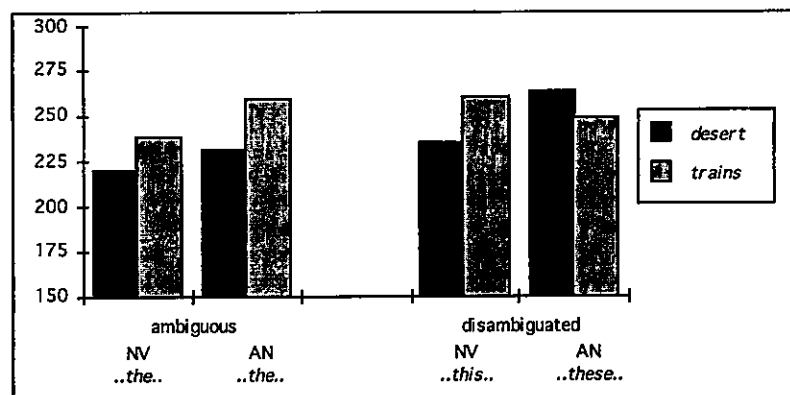


Figure 1: average gaze durations (ms) for words in the ambiguous region of materials used in F&R's Experiment 1 (data from F&R: Table 1).

There is a further aspect of F&R's reading time data that is of relevance here. They report a three-way interaction in reading times for words in the ambiguous region in their experiment, involving ambiguity, word order and syntactic class (adjective-noun vs noun-verb). This is significant ($p < 0.01$) for average gaze durations, and approaches significance ($p < 0.06$) for reading time per character. Figure 1 shows that this three-way interaction results from a relatively greater increase in reading time for the first than for the second word of the ambiguous phrase in the disambiguated adjective-noun reading (*I know that these desert trains...*) such that this first word has longer reading times in this version than in each of the others, which appear not to differ substantially from one another. If increased reading time reflects increased processing, then this pattern supports the above contention that the combination of plural determiner with singular noun in *these desert* in d) makes clear the structural interpretation of *desert* as a modifier. Importantly, it clearly also affects MacDonald's (1993) argument that the increased reading times in the ambiguous region of F&R's disambiguated test sentences are due to the lack of deictic motivation

⁴ There are exceptions to this pattern in F&R's material (cf. their Appendix 1). For example, the ambiguous *military might* has corresponding supposedly disambiguated forms in *the army might* and *the political might*, though these are to my mind no less ambiguous (e.g. *the political* could be a nominalised adjective). Some of F&R's other materials are also not as clearly disambiguated as they might have hoped—thus, the last word of *the trash can smells* could be verb (as they intended) or noun.

for the *this/these* determiners, since that explanation alone would not account for the difference between the *this* and *these* cases.

Note that in their own discussion of this interaction, F&R (p512) initially comment that the increased reading time for the first word in the disambiguated adjective-noun condition is attributable to greater complexity in the derivative adjectival form. Yet they give no principled account why these forms should be more complex, and indeed they seem to argue in their presentation of the delay strategy hypothesis that these forms are lexically specified, just like the nominal forms — presumably this should therefore mean that they are no more complex than these nominal forms. In a second experiment, with a 'systematic' semantic relationship between the two target words, F&R found that the adjective-noun version no longer took more time to read. The 'oddity' of the adjective form in the first experiment is then attributed to the 'unconstrained semantic relation between the adjective and noun' (F&R: 513). However, given that the reading time effect is found on the first word, this interpretation seems to suggest that the semantic relationship is already apparent before the second word has been read, or at least that when readers are processing the first word they are also influenced by the following word. I suggest that the account offered here is a more parsimonious one of the word-by-word interpretation of the sentence data.

In summary, I argue that the data presented by F&R and by MacDonald support the claim that all of the sentence forms are analysed word-by-word, using all available information. Of the sentences in Table 1, a) and b) remain ambiguous until the disambiguating region, but further analysis may show that one reading (i.e. of *desert* as a head noun) is initially favoured because of constraints on interpretation. F&R's reanalysis of the reading time data is based on an unsuitable post-test and so does not address this. The sentence in c) is initially also ambiguous, but the structural interpretation of both *desert* and *trains* is made clear by the lack of number agreement between *trains* and the preceding determiner *this*. In d) by contrast, the structural role of *desert* — and therefore the structural interpretation of the ambiguous phrase — becomes immediately apparent through the lack of number agreement between *these* and *desert*.

Ambiguity resolution in the spoken modality

The discussion above has attempted to re-interpret some of F&R's findings within a framework of word-by-word analysis that does not include a delay strategy. Rather, it is assumed that readers (and listeners) will make use of whatever information is available during the development of their interpretation of the input material. This information can of course include lexical and structural preferences, which act as constraints on interpretation (MacDonald, 1993). When we turn to consider spoken language input, it becomes clear that there are additional information sources that can be of relevance. This is the subject matter of this section of the paper.

What relevance then does the consideration of spoken language processing have to the interpretation of F&R's ambiguities, and *vice versa*?

Firstly, as the preceding sections have made clear, the interpretation of *desert trains* as [noun+verb], as [modifying noun+head noun], or as a noun compound has implications for the pronunciation of the sequence. Secondly, a chief difference between spoken and written language is the relative impermanence of the spoken form. In other words, since spoken language is not available for reanalysis in the case of garden paths, it might be expected that fewer such experiences are licensed. One way in which this may be achieved is through the effective and immediate use of differences between pronunciation forms.

If different readings of the ambiguity can be reliably linked to different realisations, then these can clearly be enlisted as further constraints on interpretation. In this section I investigate the realisation of such ambiguities and then ask whether and how listeners make use of the realisational differences in their interpretation of the spoken forms of such ambiguities.

In the following description I make reference to the ToBI system of prosodic transcription (Silverman et al 1992), based on the work of Pierrehumbert (1980; Beckman & Pierrehumbert 1986; Pierrehumbert & Beckman 1988). This system is based on two abstract pitch levels (H for high and L for low) which combine to form a set of five pitch accents (H*, H+!H*, L*, L+H*, L*+H — starred symbols indicate prominent syllables). In addition, the system allows two levels of phrase structure — the intonational phrase is delimited by the boundary tones H% or L%, while the smaller intermediate phrase is bounded by a phrase tone H- or L-. On a separate representational tier, break indices can be marked between each word, on a scale ranging from 0 (highest level of coherence) to 4 (least coherent), with 1 as the default for clause-internal word boundaries.⁵

As is usually the case with the prosodic realisation of grammatical structures, there are a number of possible forms for each of the readings of F&R's ambiguities. Amongst other things, these may depend on performance factors such as constituent length (Gee & Grosjean 1983), which can affect the intonational grouping of the utterances. That is, are the sentences likely to be realised as one or more than one intonational or intermediate phrase, or in other words, will there be more than one nuclear accent (i.e. pitch accent combined with boundary or phrase tone)? Thus we may be more likely to find a division between noun and verb (underlined) in *The local newspaper reported that the warehouse fires numerous employees each year* than in *Tom remarked that the summer flies by too fast*. Such issues are important, as they can mean that although sentences such as the ones under consideration are often disambiguated by intonation, they may not be disambiguated by all possible prosodic structures. It should be noted that there are additional factors which are not considered here, such as contrastive realisations resulting from narrow focus, for example for

emphasis as in *Tom remarked that the SUMMER flies by too fast (...not the winter)*.

The interpretation of sequences like *desert trains* involves two levels of structural description, each of which can be linked to a prediction of possible differences in the prosodic realisation of the sequence. The first distinguishes the cross-phrase [noun+verb] interpretation from the phrase-internal [noun+noun] interpretation, and involves a contrast in the location of a potential phrase tone (L-) and an associated minor break index (2 or maybe 3), which would fall mid-sequence in the [noun+verb] reading but at the end of the sequence in the [noun+noun] reading. The other level of structural contrast involves primarily the [noun+noun] sequences, and distinguishes left- and right-stressed items according to whether the pitch accent (H*) is on the first or second word of the [noun+noun] sequence, represented here by *desert* or *trains*.

Considering first the [noun+verb] structure, the optionality of a phrase-tone and intermediate boundary after *desert* gives the realisations in (1). The break index value of 2 in 1a) indicates a minor rhythmic discontinuity and the sentences seem acceptable without the parenthesised pitch accent on *trains*, (note also that detail is only given for the ambiguous sequence and the final nucleus — other pitch accents are possible, e.g. on *know* or *people*):

1a) $\overset{\text{H}^*}{\text{I}} \text{L- } (\text{H}^*) \quad \text{H}^* \text{L-L\%}$
 2 1 4

1b) $\text{H}^* \quad (\text{H}^*) \quad \text{H}^* \text{L-L\%}$
 1 1 4

As with the [noun+verb] form, the [noun+noun] structure may optionally have an intermediate phrase boundary, giving:

2a) $\text{H}^* \quad \text{L-} \quad \text{H}^* \text{L-L\%}$
 1 2 4

2b) $\text{H}^* \quad \text{H}^* \text{L-L\%}$
 1 1 4

But additionally (in the case of some of the materials at least) the [noun+noun] sequence may have right-hand stress, i.e. a later pitch accent, as in:

⁵ Cf. Steedman, (1991), Grabe et al (1994) and Warren et al (1995) for further examples of the use of this framework in parsing research.

H* L- H* L-L%

2c) I know that the desert trains are especially tough on young people

1 2 4

H* H* L-L%

2d) I know that the desert trains are especially tough on young people

1 1 4

These few examples illustrate the overlap that may exist between the realisations of the two structures. Thus the ambiguous phrase in 2b) is identical to that in the version of 1b) with no accent on *trains*. However, these are possible realisations and not actual ones, and research is underway to assess the likelihood of the variants in 1) and 2) in materials based on F&R's experiments.⁶ It is possible, for example, that the versions in 1a) and 2a) are more frequently associated with the structures under consideration. Previous analyses of similar ambiguities, involving monosyllabic words in a set of five sentence pairs, such as *park acts* and *port deals*, seem to support this, and have shown that there are reliable acoustic differences between the [noun+noun] and [noun+verb] readings (Warren 1985). First, there were significantly greater word and pause durations at the position of the first word (e.g. *park*) in the [noun+verb] reading and at the position of the second word (*acts*) in the [noun+noun] reading, as would be indicated by the location of the break index of 2 after *desert* in 1a) and after *trains* in 2a). Second, the fundamental frequency (F0) contour showed a greater fall on *park* in the [noun+verb] reading, but a more gradual fall across this and the next word in the [noun+noun] reading, reaching a low point at the end of *acts* — again, these reflect the patterns expected in 1a) and 2a), i.e. a fall in F0 to the phrase boundary in both cases, but spread across the two words in 2a) but only on the first in 1a).

If this pattern is consistent, and perceptible, how could it be used by listeners in their syntactic processing of these ambiguities? Clearly, if the possible realisations listed in 1) and 2) are representative, then there are differences in the potential information value of the various intonational markings. If, consistent with the word-by-word approach given above, we consider first the initial word of the ambiguous region, then a marked fall in F0 on *desert* will be a clear indication of a [noun+verb] reading (as in 1a); the absence of a pitch accent on this word will mark the right-hand stress version of the [noun+noun] sequence given in 2c) and 2d); but a level pitch accent (H*) will be ambiguous between the two — compare 1b) and 2b). The phrase tone in 2a) stretches from the pitch accent on *desert* to the end of *trains*, and may result in a slight fall on the first word that is discriminable from both the fall in 1a) (as indicated by the acoustic data from Warren 1985)

⁶ Unfortunately — due to the author's recent move from the UK to Wellington — auditory and acoustic analyses of recordings of F&R's materials are not available for inclusion in this paper.

and the level pitch accents in 1b) and 2b). The durational properties of this first word and any rhythmic disjunction caused by a pause after this word may also contribute to a decision between the two readings. Similar analyses could be pursued for the prosodic marking of the second word.

Steedman (1991) marries a combinatory approach to the analysis of prosodic units in spoken sentence processing with a parser based on categorial grammar, whereby each of these domains serves to license or exclude structural analyses in the other. His 'Prosodic Constituent Condition' states that 'combination of two syntactic categories via a syntactic combinatory rule is only allowed if their prosodic categories can also combine (and vice versa)' (Steedman 1991: 279). Thus, while a syntactic analysis might see the main division in a sentence to be between subject and predicate (NP and VP in phrase structure terms), Steedman acknowledges that prosodic analysis might favour a different division, reflecting the thematic structure of an utterance, such as 'Fred ATE / the beans' as an answer to the question 'What did Fred do with the beans?' One of the mechanisms by which this is achieved is a principle that prevents composition of prosodic units across a boundary tone (under which Steedman includes the phrase tone). If 1a) and 2a) above prove to be typical realisations of the [noun+verb] and [noun+noun] versions of F&R's ambiguities, then it is easy to see how parsing principles such as those proposed by Steedman could account for the use of prosodic information in structural analysis.

The foregoing discussion is necessarily somewhat speculative. It also makes the process of analysing sentences with the benefit of prosodic information seem a little like finding one's way through a decision tree, answering binary yes/no questions on the prosodic and syntactic categories of the utterance that affect the next stage of processing, and so on. I should emphasise that this is purely illustrative — in reality I suspect that further experimental data will show that prosodic information serves as yet another source of information constraining structural interpretation in a probabilistic manner (Altmann & Steedman 1988; MacDonald 1993; 1994; Spivey-Knowlton et al 1993). Current research in the Linguistics Department at Victoria is investigating the use of such cues in the on-line processing of spoken forms of these ambiguities.

Appendix 1

Written continuation data for materials used in F&R's experiments 1 & 2. Proportions are given for 'adjectival' interpretations of fragments ending with the first word of the ambiguous phrase; the second word of this phrase is given in parentheses, together with F&R's results for the continuation task where the phrase included that word. Sentence 1 of experiment 1 contained *pants suit*, and was excluded for the panel of British English speakers.

Experiment 1

2. The local newspaper reported that the warehouse	0.08	(fires	0.96)
3. In my opinion, the military	0.42	(might	0.16)
4. We all should have known that some metal	0.42	(rings	0.96)

5. Personally I find it difficult to believe that China	0.08	(figures	0.40)
6. Susan was extremely surprised that the winter	0.33	(bears	0.76)
7. Lots of people know that the cashier	0.00	(checks	0.24)
8. Local people are concerned that the theatre	0.00	(shows	0.32)
9. Everyone agrees that the trash	0.17	(can	0.68)
10. I know that the desert	0.08	(trains	0.60)
11. In many large nations, the poor	0.00	(state	0.76)
12. In an old version of that movie, the detective	0.00	(cases	0.40)
13. We have been told that the Swiss	0.17	(watch	0.72)
14. For some reason, Sally was surprised that the cotton	0.58	(felt	0.08)
15. Tom remarked that the summer	0.08	(flies	0.40)
16. As I understand it, the official	0.33	(will	0.32)

Experiment 2

1. I don't understand why it is that the wood	0.00	(surfaces	0.96)
2. Despite last year's report, the city	0.00	(hopes	0.04)
3. Every American knows that the government	0.00	(promises	0.04)
4. Nobody knows if it's true that the university	0.08	(fines	0.28)
5. Mrs Baker is convinced that the school	0.08	(fears	0.12)
6. We just found out that the post office	0.08	(packages	0.60)
7. Some of us weren't aware that the church	0.08	(pardons	0.28)
8. Nobody seems to complain about the fact that the department store	0.00	(buys	0.04)
9. It is no secret that the official	0.50	(lies	0.32)
10. Mrs Jones is pleased now that she has discovered that the greenhouse	0.42	(plants	0.72)
11. We should have realised that the tractor	0.17	(wrecks	0.12)
12. We could see immediately that the tree	0.00	(swings	0.52)
13. The agency reported that the family	0.00	(worries	0.48)
14. Some people think it's ridiculous that the county	0.17	(buses	0.52)
15. John quickly learned that the hardware store	0.00	(prices	0.84)
16. According to the news report, the state	0.00	(pleas	0.84)

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