

ON FRUITLESS TREES: A REPLY TO BOGDAN SZYMANEK

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That *Explorations in Seamless Morphology* (henceforth, *ESM*), the volume we edited with late Stanley Starosta, has received careful attention from Prof. Bogdan Szymanek (henceforth, S) cannot but be a source of pleasure for us. That pleasure is enhanced by the fact that the overwhelming majority of criticisms offered are potentially an invitation to advance our understanding of morphological matters together: an attempt on our part to answer some of the objections he raises against Whole Word or Seamless Morphology could potentially go a long way towards opening a dialogue that could benefit all of us morphologists. It is in that spirit that we offer the remarks contained in this reply.

First, some preliminary matters, partially to respect the norms of such a reply. Although S. concludes his review by citing Plag's newly published textbook on morphology, according to which, "word-based morphology can account in a straightforward fashion for a wider range of phenomena than seems possible in a morpheme-based approach" (Plag 2003: 189), he seems to want to concentrate on another conclusion of Plag's: "There is some evidence that word-internal morphological structure is needed to account for a number of phenomena, which are not easily accounted for otherwise" (Plag 2003: 189). Although S in his conclusion says that a compromise position is needed, he focuses on the idea that words do have internal syntax, something he, as the review shows, clearly believes in himself. We shall, accordingly, give more space to that idea in this reply.

S raises two different kinds of objections against *ESM*: (1) typographical and editorial and (2) substantive. We are happy to concede all of the former, and can only apologize for the fact that S did not find them as self-correcting as we imagined them to be when we first noted them, to our own horror. Neither the untimely death of our collaborator nor our own unavailability at the time of the production of the book are sufficient excuses for the typographical and other editorial mistakes that got left in.

Having said that, let us now turn to substantive matters. These can be profitably divided into two sub-categories: (a) sociology of knowledge issues and (b) morphological issues. As the questions that belong to the former sub-category can be dis-

posed off rather quickly, we shall take them up first. To keep this part of the response brief, we shall not cite S chapter and verse but only provide brief comments to invite S and other colleagues to reflect on matters that S's critique raises.

First, we find S's understanding of 'generative morphology' interesting. It excludes several things that could have been included if he had opted for a different interpretation of 'generative'. There are three fairly well-understood interpretations of the word *generative*: (1) philosophical, according to which a grammar is generative iff it invokes principles and concepts that are unique to language; (2) formal, according to which a grammar is generative if it is fully explicit; and (3) sociological, according to which a grammar is generative only if it comes from a particular city in the United States or from one of its satellites. This is a perfectly normal state of affairs, but we must assume responsibility for the choice we make. We point this out not because we think generativism is a vice or a virtue but because some of us may have some difficulty with the interpretation S seems to have chosen. Secondly, even if one ignores Aronoff's (1983) retraction of his earlier position regarding the word being the minimal sign, it is possible to show that even his 1976 monograph actually supports stem-based morphology. And so does Anderson (1992), our criticism of whose work makes S unhappy. The additional problem with Anderson (1992) is that he claims that words don't have any internal syntax except when they do. Without a clear characterization of what it means to talk about word-based morphology, even Pāṇini's *Ashtadhyayi* can be described as word-based, and in fact has been so characterized. In insisting that any theory of morphology that allows operations on units smaller than the word is not truly a word-based theory of morphology, we provide such a characterization. Needless to add that we need to know what a word-based theory of morphology is before we can decide if a particular theory of that sort is an adequate theory of morphology. Being word-based and being adequate are, in other words, two different things.

Under this sub-category, we cannot resist the temptation of adding one last remark: some of the papers advocating the sort of theory that Ford and Singh (1991), reproduced as chapter 1 in *ESM*, provides a consolidated outline of have been available since the early eighties, a full decade before Anderson (1992). This is not to blame anyone for anything but simply to point out that Chomsky's distinction between Plato's Problem and Orwell's Problem may not be as clear cut as he thinks it is, at least not in generative linguistics!

Be that as it may. Let us now turn to matters morphological. As *ESM* is a collection of articles, some originally published a few years ago, devoted to outlining, exploring, and testing the basic claims of Seamless Morphology (SM), the somewhat mischievous nick-name for Whole Word Morphology (WWM), it would perhaps be useful to provide a brief summary of it (our apologies to S for this nth repetition of the outline below). It is provided in (1) below, which is literally lifted from Singh (in press) and differs only in minor ways from summaries that have appeared elsewhere, including *ESM*. (1a) is the heart of the matter and (1b) an explicatory post-script:

- (1a) All that needs to be said about word structure in any language (of any type whatsoever) can and must be said by instantiations of the schema in (S1) below. These instantiations are referred to as Word Formation Strategies (WFS's) because as generalizations drawn from known particular facts, they can be activated in the production and understanding of new words. WFS's must be formulated as generally as possible, but, and this is crucial, only as generally as the facts of the matter permit.

$$(S1) /X/_{a} \leftrightarrow /X'/_b$$

where:

- (a) $/X/_{a}$ and $/X'/_b$ are words and X and X' are abbreviations of the forms of classes of words belonging to categories a and b (with which specific words belonging to the right category can be unified or on to which they can be mapped);
- (b) ' represents (all the) form-related differences between $/X/$ and $/X'/_b$ that fall outside of automatic phonology;
- (c) a and b are categories that may be represented as feature-bundles;
- (d) the \leftrightarrow represents a bidirectional implication (if X, then X' and if X', then X);
- (e) the interpretation of $/X/_{a}$ is a semantic function of $/X'/_b$, and vice versa;
- (f) ' can be null iff $a \neq b$.
- (1b) It should be obvious that according to WWM, (a) morphological complexity is a matter of the analyzability (\neq segmentability) of a word into a variable and a constant component with respect to a WFS, (b) 'morphophonology' (or its contemporary *avatar* 'lexical phonology') is an integral part of morphology (cf. S1 b above), (c) both intra-linguistic (inflection vs. derivation, affixation vs. compounding etc.) and inter-linguistic (flectional, isolating etc.) morphological diversity can be derived from S1 above without affecting it in any fundamental way, (d) it (S1 above) offers a unified account of what have sometimes been seen as different types of morphologies and encapsulates the rejection of multipartite analysis of words into 'roots', 'affixes', 'stems' etc., entities that are hard to define and harder to tell apart, and (e) neither the strategies nor their 'outputs' have any syntactic constituency relationships marked in them in any fashion whatsoever. These strategies license the words a speaker has or may come up with.

WWM, to summarize, sees morphology not as a combinatorics of morphs or morphemes but as a system of generalized and abstract bi-directional correspondences amongst sets of whole words that exploit the same formal contrast. These corre-

spondences are exploited by speakers to create new words, reconstruct words they have forgotten, and interpret morphologically complex words they have not heard before.

In defense of the orthodox generative idea that words have internal constituent structure, S invokes several folk-tales common in generative circles. The first of these is taken from Aronoff (1976). S observes:

[...C]onsider the well-known limitation on the formation of *-al* adjectives in English, from nouns terminating in *-ment*. According to Aronoff (1976: 54), the constraint in question depends on the internal constituent structure of the nouns, in the sense that (apart from a few exceptions) those *-ment* nouns which are derived from verbs, i.e. contain the morpheme (suffix) *+ment*, do not lend themselves to the process in question (cf. *commit* – *commitment* – **commitmental*). On the other hand, nouns which, in traditional terms, have no internal structure, i.e. do not contain a suffix, may derive corresponding adjectives in *-al* (e.g. **orna_v* – *ornament* – *ornamental*). The problem is, then, how this contrast is to be expressed in a framework which dispenses with morphemes and morpheme boundaries.

(Szymanek this volume: 220)

On the basis of the ungrammaticality of sequences like **employmental*, **containmental*, and **derangemental*, Aronoff argued that the ‘suffix’ *-al* can be attached only when the input does not contain a verb. In their search for possible arguments for word-internal constituent structure, both Aronoff and S seem to miss the fact that the ungrammaticality of sequences like **employmental* has to do with the fact that the initially accented *-mental* creates an accentual conflict when strung together with the finally accented *employ* or *contain*. If the prosodic explanation we provide is in fact the correct one, one would expect monosyllabic and non-finally accented polysyllabic verbs in English to allow *-mental* to follow them without much difficulty. As words like *judgemental*, *governmental*, and *developmental* show, that in fact is the case, and there is no need to invoke any morphological brackets in stating the restrictions that are responsible for the state of affairs brought to our notice by Aronoff. We cannot resist the temptation of adding that only a generative grammarian can equate the unaccented *-mit* of *vomit* with the accented *-mit* of *permit*. Without such confusion, neither “the Latinate stem *mit*” nor the idea that perhaps the morpheme can be saved by abandoning the condition that it be meaningful can be sustained (cf. Aronoff 1976: 15). Be that as it may. It is clear that the explanation for the ungrammaticality of sequences like **employmental* does not require any appeal to word-internal syntax, something generative morphology seems to take for granted rather than seriously argue for.

His second argument for the internal syntax of words relies on a story constructed by Siegel and Allen. He notes:

It is even harder to imagine how WFSs are to cope with the known cases of “affix” attachment which are sensitive to the structurally (but not linearly) adjacent morphemes or, in other words, to the derivational history of a complex word. Consider, for instance, the Adjacency Condition, as formulated in Siegel (1977) and Allen (1978). Allen (1978: 154) states this principle as follows: “morphological rules may only relate material contained in structurally adjacent cycles”. Siegel (1979: 190-2) illustrates the Adjacency Condition with her analysis of the distribution of the negative prefix *un-* with respect to base adjectives in *dis-*. It turns out that *un-* cannot be prefixed to adjectives containing *dis-*, where *dis-* is structurally adjacent to the point of prefixation. Hence, impossible are derivations like **un[discrete]_A* or **[un[dis[honest]_A]_A* but perfectly well-formed are structures of the type *[un[[distract]_ving]_A]_A* or *[un[[discover]_vable]_A]_A*.

(Szymanek this volume: 220)

The story S re-narrates above goes back to Siegel (1979: 192), who, on the basis of contrasts like *undistracting!**undishonest and *undiscoverable!**undisloyal, argued that words in *un-* are “thrown out if the morpheme *dis-* is uniquely contained in the cycle adjacent to *un-*. She doesn’t quite tell us why she thinks the sequence *dis-* in *distracting* and *discoverable* is a morpheme. It certainly seems to have nothing to do with the *dis-* in *dishonest* and *disloyal*. Neither does she consider the possibility that if forms like **undisloyal* and **undishonest* were allowed, one would be hard put to find an interpretation for them that would be different from the interpretation assigned to the simple adjectives from which we are told these forms are to be derived. And the problem with **undiscrete* seems to have something to do NOT with *un-* appearing immediately before the subsequence *dis-* of *discrete* but with the competition between *in-* and *un-* and with the fact that *discrete* “takes” *in-* and not *un-*. While it is true that adjectives like *undistracting* and *undiscoverable* are acceptable words of English, and that their corresponding verbs (**undistract*, **undiscover*) are not so easily accepted, it has to do NOT with brackets and cycles but with the fact that the so-called *un-* morpheme that attaches to adjectives roughly means ‘not X’, while the other *un-* which attaches to verbs indicates some sort of reversal. One only has to think of the two possible interpretations for *undoable*, now that everyone uses computers, to understand why a word like *undiscover* sounds odd at best. One should also note that given the right context, one that involves going back in time for example, **undistract* and **undiscover* are quite acceptable.

Without belabouring the point, we would like to invite S to go over the “refinements”, such as bracket-erasure, “mainstream generative morphology” has introduced since Siegel – from Allen (1978) to Lieber (1981) and beyond. He may be surprised to find that the net result of these “refinements” has been to make it clear that there are no morphological processes that need to appeal to word-internal constituent structure.

His next somewhat indirect argument comes from what we believe to be a serious misunderstanding of the model(s) he is evaluating. He notes:

It would appear that no such dogmatic considerations should prevent the proponents of Whole Word Morphology from recognizing the existence of semantic constraints on word-formation. Indeed, some of the WFSs discussed in *ESM* make reference to the semantic properties of words. For instance, while discussing the behaviour of noun “compounds” in Bangla, Singh and Dasgupta (p. 81) make use of semantic labels like “abstract”, “botanical” or even “leaf”, which are made an integral part of the relevant WFSs. These are used as diacritics encoding significant semantic constraints on the processes discussed. But some of the semantic conditions discussed in the morphological literature are just too subtle, and too complicated, to be expressed as simple, binary diacritics. For instance, Zimmer (1964) demonstrated that the formation of negative adjectives in English (by means of *un-*) is, as a rule, impossible when the semantic content of an adjective is “evaluatively negative” (cf. **unbad*, **unugly*, **unstupid*). Moreover, Zimmer pointed out that *un-* prefixation is not applicable to lexical, “strictly monomorphemic” adjectives that have “strictly monomorphemic” antonyms (cf. **ungood* vs. *bad*, **unlong* vs. *short*, etc.). The latter claim is, in fact, another example of a morphological generalization that is impossible to make under Whole Word Morphology. The former one refers to a semantic property (“evaluatively negative”) which is, of course, difficult to define in any framework, but will make a particularly cumbersome diacritic, if added to a WFS.

(Szymanek this volume: 221)

Proponents of WWM take one of two different, though perhaps practically equivalent approaches to semantics. Ford, Singh and Dasgupta, on the one hand, assume that morphology is dissociative and that rules of interpretation are distinct from the WFS to which they are associated. Neuvel (2003), on the other hand, places semantics on separate levels of representations and makes the semantic correspondences part of the WFS. Most importantly though, neither Ford, Singh and Dasgupta in their separationist view, nor Neuvel in his, shall we say ‘globalist’, approach believe in or ever require binary semantic features.

Neuvel (2003) for example, creates the semantic correspondences that are part of the WFS the very same way that the formal correspondences are created. Contrasts that are found in at least two pairs of words of a lexicon license the creation of a WFS, the formal part of which shows as constant material all the phonic differences between the relevant words; and is restricted by adding all the formal similarities found in all the related word pairs. The words *conceive/conception*, *receive/reception*, etc., for example, license a WFS whose formal correspondence includes the differences between the words (/Xɪ:v/ vs. /Xɛpʃən/) and is restricted by also specifying as constant the /s/ found immediately before this contrast in all the

words that participate in this morphological relation. While none of us are completely happy with the current state in lexical semantics, creating the semantic correspondences that are part of the WFS works in exactly the same way. No matter what kind of semantic representation one uses to represent the meaning of words, the semantic portion of the WFS simply shows as constant the semantic differences between related words and is restricted by also specifying what is similar in all the related word pairs. The WFSs that form the morphology of a lexicon are a function of the words of that lexicon and they include absolutely nothing that was not already part of the representation given to the words they relate. Not only are semantic diacritics not necessary in WWM, as S seems to imply, they are more than superfluous and in fact prohibited by the very nature of the WFSs.

Despite the fact that it has been made clear in relevant papers that each WFS is accompanied by a rule of semantic interpretation – though these rules are admittedly not formulated – and that the “features” S points to are really only mnemonic labels to suggest what these rules will give us, he takes us to task for our inability to provide the features he thinks we ought to provide. Despite the popularization of such features by Katz and Fodor (1963), it has been clear, at least since Weinreich (1966), that no such Liebnitzian exercise is ever likely to succeed. We think S fails to grasp that what Ford and Singh and Singh and Dasgupta are saying is that morphology needs to be dissociative and that when you cannot write a plausible rule of semantic interpretation, the derivation you have in mind will, to borrow from a programme he is likely to be familiar with, ‘crash’. To put the matter differently, English nouns, for example, can be divided into several morphological categories, including those that end in *-ity* and *-ness*. The only morphological features they have are features like ‘ends in *-ness*’. The other “features” they can be said to have are only reductionist construals attributed to them by the rules of interpretation they are subject to. It was perhaps a mistake on the part of some of us to yield to the pedagogical temptation of abbreviating the results of such rules.

In response to Probal Dasgupta’s very compelling demonstration that traditional morphemic segmentation is often incoherent and in many cases impossible (“The importance of being *Ernist*”, pp. 284-300), S merely dismisses the issue by suggesting that the evidence used is too “idiosyncratic”. While we regret that S was not convinced by the demonstration, we would like to point out that the words used in this paper are not place names from an obscure extinct language but common words of English. Furthermore, if we are to consider every word that does not lend itself to a neat segmentation as idiosyncratic and irrelevant data, we are left with very little room for discussion since, of course, classical morphology works perfectly in all those cases where it works perfectly. It seems to us that it is precisely in those less common cases that lies the real test for a theory of morphology. It doesn’t take much to account for the relationship between *sing* and *singing*. As Aronoff (2001) puts it, “the waste remains”, and it is in that “waste” that we find the real problems we have to solve.

Although we are tempted to entertain the idea that in his review S is not doing anything more than merely wrapping his endorsement of what he calls mainstream generative morphology in a blanket of superficial acceptance of the potential of word-based morphology, we shall not give in to that temptation and instead invite him to reconsider his somewhat cavalier dismissal of Dasgupta's demonstration and to consider how a morpheme-based theory of morphology can account for the sort of observations Bender and Ratcliffe, for example, make regarding Latin and Arabic. We sincerely hope that he will do so; and if he does, we shall certainly join him on the difficult, perhaps even illusory, road mentioned but not chosen by him in his review.

To conclude, we return, as we must, to the matter of word-internal hierarchical structure, something that allows S to accuse us of "fundamentalism". Given that the generative evidence for the internal syntax of words is not even half as good as S thinks it is, one cannot but wonder precisely where in modern linguistics fundamentalism is located. We, of course, take the word to designate NOT 'strong' but 'unexamined, unreflected' positions. The basic difference between generative syntax on the one hand and generative morphology on the other is, it seems to us, that the latter is largely only a formalization of standard IA morphology and has not been the subject of much serious reflection. Even the revised definition of the morpheme offered by Aronoff (1976: 15) and cited by S reveals this. "A morpheme", we are triumphantly told, "is a phonetic *string* which can be connected to a linguistic entity outside that *string*" (italics ours). As for generative phonology, it seems to be largely an enterprise dedicated to designing representations or inventing *ad hoc* families of constraints to hide the fact that morphonology is NOT an integral part of phonology. Ignoring the easily accessible voluminous evidence against the phonologization of morphonology (cf. Singh 1996a and 1996b, amongst others) is surely a fundamentalist impulse.

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