



A-Morphous Morphology by Stephen R. Anderson

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BOOK REVIEW

Review of Stephen R. Anderson, *A-Morphous Morphology*, Cambridge University Press (1992) xiv + 434 pp., \$79.95 cloth, \$24.95 paper.

I consider this an important book. It is rich and well written and covers a tremendous range of topics and data with style and authority. The book argues for an architecture of morphological theory that is at once radical and conservative. The central points of the thesis are summed up in the following claims from page 5 of the introductory chapter:

- (1) “the general rejection . . . of the classical morpheme”.
- (2) “the claim that properties of individual lexical items . . . are not available to syntactic operations”.
- (3) “the claim that words do not in general have an internal morphological structure for phonological and morphological rules to refer to”. (p. 5)

There are also two somewhat more peripheral, or at least more particular, postures that are to be found in the same passage, namely, “the resolute assimilation of special clitic phenomena to morphology” and “the maintenance of a significant distinction among inflection, derivation, and compounding”, but the three above seem to define the theoretical center of gravity of AM.

Point (1) is in fact suggested by the punning title of the book: morphology for A is not concerned with the addition of morphemes to morphological constituents in something like the way that phrases are built up by the addition of words to syntactic constituents, but, as A sees it, words are amorphous, i.e., structureless, and a-morphous, that is to say, lacking morphemes. A argues that the form of words is better treated by means of ordered rules operating simultaneously on the semantics, external syntax, and phonological content of the input to the rules.

Claims (2) and (3) together add up to the idea that other components of the grammar cannot see what the morphology does. This makes good sense if words have no internal parts. However, claims (2) and (3) could be maintained in the absence of (1) (as was done in the heyday of the Lexicalist Hypothesis and as is the practice in such rigidly lexicalist but quite morphous theories as Di Sciullo and Williams 1987). Furthermore,

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(1) could be true, while (2) and (3) are abandoned, if, for example, phonological processing is permitted to take place hand in hand with morphology, which is, in fact, what A assumes, and if certain sorts of syntactic information are reified in morphology, as A's theory sometimes sanctions.

The book consists of an introduction and fourteen chapters, all of which are explicitly related, though with varying degrees of centrality, to the basic claims. The chapter titles, where declarative, are a good guide to their contents. The short answers to the interrogative titles of Chapters 2, 3 and 10 (which one could probably work out from the thesis of the work and Gricean principles) are: "Because morphology is not syntax or phonology", "No", and "None", respectively, though as we shall see, the detailed answers are a good deal less categorical.

- (1) The study of word structure
- (2) Why have a morphology at all?
- (3) Is morphology really about morphemes?
- (4) The interaction of morphology and syntax
- (5) The theory of inflection
- (6) Some complex inflectional systems
- (7) Morphology in the lexicon: Derivation
- (8) Clitics are phrasal affixes
- (9) The relation of morphology to phonology
- (10) How much structure do words have?
- (11) Composites: Words with internal structure
- (12) Morphology and the typology of languages
- (13) Morphological change
- (14) Morphology as a computational problem

AM is provided with a good index and an extensive, though somewhat incomplete, bibliography. For example, AM includes an extended discussion of split ergativity, but the source of this notion, Silverstein (1976), is not mentioned. In a somewhat too-brief discussion of Danish *stød* (pp. 249–252), A refers to work by Basbøl and Rischel, but instead of listing these works in the bibliography, the reader is told to "see Anderson 1975 for references".

There is much instructive and worthwhile reading in AM. I would like to mention in particular the clear and cogent discussions of how phonology and morphology are interrelated that are scattered throughout the book and emphasized in Chapter 2, section 3, and in Chapter 9. There is a nice retrospective on Sapir's (1921) notions concerning the position of morphology in grammar to be found in Chapter 12, and there are useful

remarks on some of the problems attending the computational parsing problem for words in Chapter 14.

Right around 100 languages or dialects are at least mentioned in the index. Several sets of facts from an assortment of languages are analyzed in considerable detail. For the most part, the interesting data that A gleans from this panorama of languages is clearly presented and discussed.

But when it comes to the thesis of the work itself as embodied in claims (1) to (3) above and the way that thesis is amplified, there are, to my way of thinking, serious inadequacies in AM. First, the theory itself is not what it is initially advertised as, but something more flexible, less radical, and, in a word, weaker. Second, A skirts some of facts that would seem to present problems for his thesis. Third, some of his arguments do not show what they are purported to show, but as far as I can see, something quite different. As I stated at the outset, this is a very big and very complex book. The largely critical observations that follow should therefore not be taken as implying that I have no positive reactions to other aspects of the book.

WEAKENED CLAIMS

Claim (1): Words Sometimes do Have Parts

The leading idea of AM is that words do not have internal structure such as they do in the classical morpheme theory. Of course the rejection of the classical morpheme does not extend to roots (as Richard Janda pointed out to me personally), but it is a further disappointment to learn that it does not include compounds. A does say: "This word-internal structure seems to be unique to compounds" (p. 292), but even this shakier version of his thesis is weakened further when in section 11.2 the class of words with internal parts is widened to that of 'composites' which include 'pseudo-compounds' like *Sino-Japanese*, *erythromycin*, and – surprisingly – prefix-stem combinations like *perceive*. Thus some classical morphemes do survive in AM.

For composites, A postulates a set of Word Structure Rules (WSRs) such as: $N \rightarrow N N$ (where N is the head). Such rules are exactly the kind of morphological phrase-structure rules A otherwise disapproves of, so they are distinct from the non-structure building Word Formation Rules (WFRs) that he employs for derivational and inflectional morphology. Problems arise, however, in connection with forms like German *Schwanengesang*, in which what appears to be a derivational affix, *-en*, is required on the first member of the compound. Here, as pointed out to

me by an anonymous reviewer, the derivational WFR has to be made sensitive to the word-internal structure generated by the WSR. Furthermore, non-compound composites like Georgian [_vmo[_vklavs]] ‘kill (perfective)’, are assumed to be formed by structure-building WFRs. Almost all that is left of the original thesis is this: “Word Formation Rules do not build structure, that is, unless explicitly stipulated to do so” (p. 298).

A different case where words (at least as phonological entities) have structure is that of ‘simple clitics’, namely those clitic elements that are the phonologically dependent versions of free words: “[simple] clitics do not constitute prosodically autonomous words Otherwise, however, their positioning is described by . . . the regularities of the syntax” (p. 20). In a structure of a K^wak^wala sentence on page 20, A makes it clear that phonological words containing simple clitics do indeed have parts, in fact the same parts that function in an independent way in the syntax.

So for a few different reasons, morphology is more morphous than the title of the book would lead us to believe.

Claim (2): Parts of Words are Sometimes Available to Syntax

While the theory in AM is fairly strictly lexicalist in the sense of disallowing interaction between essentially morphological operations and syntax, there are several respects in which it deviates from the lexicalist straight and narrow. For example, A handles German separable prefixes as “phonologically simple clitics attaching to the following word unless the Verb stem has been moved”. This kind of morphology must therefore know when a syntactic rule has affected the next syntactic position to the right. Furthermore, unlike other simple clitics, the separable prefixes are associated with words (*viz.*, verbs), not phrases. Nor is a German separable prefix a simple clitic in the sense of being “merely a lexical item whose phonological form does not include assignment to a prosodic unit at the level of ‘word’” (p. 201) since they do, in fact, have word stress when separated. The handling of these prefixes therefore tolerates a considerable interpenetration of syntax and morphology and in that respect is at odds with the stated principles of AM.

When it comes to ‘special clitics’, that is, clitics with no free-word alternates, A has a different treatment that is more in line with the a-morphous view of words in that processes produce the required morphological changes, which for special clitics is typically the addition of an affix. It does not, however, seem to fare any better as regards the segregation of syntax and morphology, as the following quotations show:

The syntactic peculiarities of [special] clitics will be seen to result from their introduction by a class of rules operating on phrases in the same way Word Formation Rules operate on words. (p. 198)

Where 'affixation' (in an appropriately general sense) is normally thought of as a formal change applying to words, 'special clitics' can be seen as exactly the same sort of changes, as applied to the concrete form taken by larger syntactically structured expressions (phrases). (p. 216)

A takes great pains to motivate an affinity between special cliticization and ordinary morphology. While technically not problematic, the treatment of special cliticization as closely akin to morphology should nevertheless be something of an embarrassment for AM since indubitable morphological processes are forbidden from interacting in such a direct way with the syntax.

Another important morphological arena where A officially recognizes intimate contacts between morphology and syntax is inflection, which is "precisely the domain in which the systems of syntactic and morphological rules interact" (p. 74). This should come as no surprise to readers familiar with A's famous (1982) paper. But in the context of the present work where "properties of individual lexical items . . . are not available to syntactic operations . . ." (p. 5), it seems incongruous that inflection and syntax are open and notorious bedfellows. Remarks like the following are hard to reconcile with the stated aims of AM: "As a whole, then, the theory of agreement forms a part of comparatively well-understood portions of syntactic theory: the theory of categorial (or 'X-bar' structure on the one hand, and the Binding theory on the other" (p. 18). It is possible to be more resolutely lexicalist with regard to the treatment of inflection (Lapointe 1980), but that is not the line adopted in AM.

Claim (3): Phonology Refers to the Internal Structure of Words

A largely accepts the basic tenets of Lexical Phonology (see Kaisse and Shaw 1985 and the references cited there) including the assumption that "morphological and phonological rules interact in a cyclic fashion, with phonological adjustment following each morphological operation" (p. 255). This conclusion seems out of place in a theory where "words do not in general have an internal morphological structure for phonological and morphological rules to refer to" (p. 5). They may not have internal structure, technically speaking, but this step-by-step inter-leaving of phonology and morphology clashes with the spirit of lexical integrity advertised in (3) above in a serious way.

NEGLECTED DATA

One of the most challenging of phenomena to any program such as A's is noun incorporation. Here A follows a standard but erroneous line, claiming that external constituents that seem to modify the sense of an incorporated noun are always possible arguments of base verbs. There are extensive arguments in the literature to the effect that this is not so in a variety of languages (Baker 1988, Sadock 1991), but none of the crucial data are taken up in AM. Instead, A's argument is the non-sequitur that since K^wak^wala incorporated nouns are syntactically opaque, then all incorporated nouns in all languages are. When it comes to Eskimo, where I naturally feel that the data are quite compelling (Sadock 1986), A chooses not to consider the facts (P. 269).

A number of other claims made in support of the thesis of AM strike me as dubious. For example, A says that inflectional morphology is always outside of derivational morphology and that his basic premise explains why this should be. When confronted with an apparent counterexample from Khalkha Mongolian (p. 127), he says "the same formative, even in association with the same 'meaning' . . . may be inflectional in some cases . . . and derivational in others". Here the claim concerning the relative position of derivational and inflectional formatives is saved, but by fiat. But, as an astute referee of this review has observed, A allows certain exceptions in his own terms to the derivation-inside-inflection thesis. For example, he treats Icelandic middle voice forms like *klæðast* 'to dress (oneself)' as composites with internal structure like [_v[_vklæða]st]. Now inflection of such forms is more-or-less identical to inflection of the internal stem, thus occurring inside the non-inflectional affix *-st*, cf. *kallast* 'to be called, named (something)', *kölluðumst* 'we were called . . .'.

There are obvious exceptions to the universal ordering of the elements signalling derivation and inflection in cases A does not consider as well. In numerous American languages (and not just in these either) morphological templates specify positions for inflectional material inside of positions for derivational material. In the Athabaskan languages the verb template includes agreement markers closer to the root than certain derivational affixes. In Ahtna (Kari 1989), for example, certain agreement markers occur in the fifth position to the left of the root, and various kinds of derivational material occurs in the sixth through tenth positions. Especially problematic is the occurrence of incorporated nouns in the seventh pre-root position. While in numerous languages incorporation is a matter of compounding and hence could involve complex morphological structure for A, he actually treats noun incorporation as involving morphological

processes of the a-morphous type (p. 33). It seems to me that the claim about the relative position of derivation and inflection in the case of Athabaskan incorporated nouns produces a paradox for a-morphous morphology. One would either have to say that the agreement markers in the fifth position are not inflectional (which seems wrong and is out of keeping with the program of AM) or that incorporated nouns in Athabaskan are inflectional. But if noun incorporation can be a matter of inflection, and inflection is “precisely the domain in which the systems of syntactic and morphological rules interact”, then noun incorporation should be able to interact with the syntax in ways that A says it never does.

As part of the subsidiary program of separating derivation and inflection, A makes the remarkable claim that “there do not ever seem to be elements which combine inflectional and derivational categories” (p. 76). I believe that this claim is either circular or false. It is circular if all categories that combine with inflection are steadfastly labeled inflectional regardless of other considerations. If we let other facts of a language decide whether some category is inflectional or derivational, then the claim that these can never be combined would seem to be falsified by examples such as the following. In West Greenlandic negation is ordinarily derivational: *neri-* ‘to eat’, *nerisoq.* ‘an eater’ *nerinngit-* ‘not to eat’, *nerinngitsoq* ‘a non-eater’, etc. But just in case the negation immediately precedes an inflectional marker of mood and person, it is sometimes realized in a portmanteau morph combining mood and person, as, for example, in the contemporative mood form *nerinani* ‘(s)he(reflexive) not eating (contemporative)’ (cf. *nerilluni* ‘(s)he(reflexive) eating (contemporative)’).

Another example closer to home is provided by the markers of gender in Spanish. Now gender is spread in the syntax and hence is inflectional. But the same markers are also (and I would say simultaneously) derivational when they indicate not just gender but sex, as in *hijo* ‘son’, *hija* ‘daughter’, *hermano* ‘brother’, *hermana* ‘sister’, and so on. Ojeda (1982) provides some interesting evidence bearing on the difference between the indication of sex/gender and the indubitably inflectional category of number. He argues that inflectional differences do not count for deletion under identity, while derivational differences do. Thus (1), where a plural head is absent on the basis of a preceding singular, is grammatical, but (2), where an attempt is made to reduce a masculine (and male) form on the basis of a feminine (and female) form, is ungrammatical.

- (1) Su hermano es más fuerte que todos los tuyos juntos.
is brother is stronger than all of yours together.
- (2) Mi hijo es más inteligente que la *(hija) tuya.
*My son is more intelligent than your *(daughter).*

The conclusion would seem to be that the affix *-a* in *hermana* is a portmanteau including the inflectional category of gender and the derivational category of sex, contrary to A's claims about what is possible. The problem for the thrust of the book that this kind of data highlights is the fact that the boundary between inflection and derivation is unsharp. In a theory in which derivation and inflection are as isolated from one another as they are in AM, it is not clear how such a cline could be handled. (Incidentally, Sapir (1921) emphatically pointed out that such borderline cases exist, a fact not mentioned in A's otherwise thorough discussion of Sapir's morphological typology.)

There are some fairly obvious cases besides incorporation where it would seem that derivational morphology does, in fact, interact in a deep way with syntax. A brings up the case of Georgian where the reference of reflexives seems to indicate that morphological causatives in that language are bi-clausal: reflexive pronouns may refer to either the formal subject or object of the causative verb, whereas with underived transitives, only the subject can count as an antecedent (p. 270f). To handle this, A proposes borrowing a treatment of Chimwi:ni reflexives presented in Di Sciullo and Williams (1987) whereby reflexivization, including the binding properties thereof, is made an entirely lexical matter, the process of adding a null affix that produces a form subcategorizing a reflexive object. Now this treatment fits poorly in a theory where agreement is treated as syntactic anaphora governed by "comparatively well-understood portions of syntactic theory: the theory of categorial (or 'X-bar' structure on the one hand, and the Binding theory on the other". Is binding syntactic or not? Sometimes one, sometimes the other?

Making reflexive binding a purely lexical phenomenon in some cases also threatens to render the separation of syntax and morphology vacuous, something Di Sciullo and Williams recognize, but A does not remark on. For Di Sciullo and Williams, the empirical content remaining in the lexical/non-lexical distinction lies in the inability of lexical reflexives to stipulate the binding of any but NPs in the argument structure of the verb, whereas syntactic reflexive binding could involve NPs in adjuncts, possessors, and so on.

For Chimwi:ni, it so happens that the reflexive pronoun only occurs as a direct object (Abasheikh 1978). Unfortunately, A does not tell us

whether this crucial fact characterizes Georgian as well. If it does not, and if, say, a sentence along the lines of *A made-sneeze B in own house* is possible, where *own* can refer ambiguously to A or B, then the lexical analysis does not carry over from Chimwi:ni to Georgian. Regardless of whether counterexamples to the lexical treatment of reflexives are forthcoming from Georgian, they do exist in other languages. This is very clearly pointed out in connection with Japanese in a work cited in this connection by both A and Di Sciullo and Williams, namely Baker (1988). In Japanese, reflexives in **adjuncts** can refer to the formal object of a causative verb, but not to the object of an underived transitive verb. Thus the lexical analysis will fail for Japanese (and, I suspect, a large number of other languages) and morphological causatives will apparently have to be analyzed as having parts available to the syntax.

Some additional empirical inadequacies: In several places in the exposition, it is important for A to show that the formal and substantive powers of syntax and morphology are distinct (p. 261), employing three of Zwicky's (1992) observations concerning the distinctions between syntactic and morphological organization. Two of these are strong tendencies but cannot be used diagnostically. *Pace* Zwicky, there do seem to be cases of morphological constituents with optional modifiers. In West Greenlandic, the derivational affix *-(r)suaq* 'big', which derives noun stems from noun stems, can be optionally modified by a preceding *-rujuk-*, which otherwise means 'giant', but here serves only to intensify the sense of the affix. The resulting affix can be further modified in the same way, and so on ad libitum, producing forms like *illorssuaq* 'big house', *illorujussuaq* 'very big house', *illorujorujussuaq* 'very, very big house', *illorujorujorujussuaq* 'very, very, very big house', and so on. Zwicky also claims that there can be no agreement within words, but at least a few languages, such as Crow (Graczyk 1991), have obligatory agreement markers not just once in a verb, but in association with various auxiliary-like elements incorporated into the verb.

- (3) baa-lée-w-isshi-ssaa-k
 1A-go-1-ready-not-DECLARATIVE

I am not ready to go.

In Southern Tiwa (Allen et al. 1984), the grammatical class of an incorporated noun determines the agreement affixes of the verb in which it is found. Thus *shut-* 'shirt' belongs to agreement class A and *mukhin-* 'hat' belongs to class B. The verb agreement prefixes register these categories for both the subject and the object. In the examples (4) and (5), the

subject is of the A class and the object is A in the case of *shut* and B in the case of *mukhin*. Note that the agreement in (4) and (5) is with a part of the verb itself, namely the incorporated noun, contrary to what Zwicky claims is possible.

- (4) u-shut-tuwi-ban
A:A-shirt-buy-past
 (S)he bought the shirt.
- (5) i-mukhin-tuwi-ban
A:B-hat-buy-past
 (S)he bought the hat.

In a short section (pp. 262–264), A discusses possible semantic motivations for complex morphological structure. He suggests that a functional composition approach such as proposed by Mercier (1988) is capable of handling the facts, though aside from the bracketing paradoxes presented by well-known examples like *ungrammaticality*, he does not discuss any real data. Now functional composition is an extremely powerful approach that is capable of doing much of the hard work of syntax and morphology all by itself, so that it is not clear that given such a mechanism all of the rest of the power of A's morphology would be required. (See Schmerling 1983.) Be that as it may, there would seem to be tough cases for any local approach to the semantic scope of morphological elements. In West Greenlandic (a language I cite frequently here because it stands at the extreme of the spectrum of derivational fluidity), the negative verbal suffix, a derivational affix that can be buried deep inside a word as shown above, can nonetheless have variable scope with respect to semantic units represented by separate words. Thus *inersimasut kisimik qamuteqa-nngil = lat* ('adults only sled.have-not = INDICATIVE.3p') is ambiguous between 'Adults are the only ones who don't have sleds' and 'Not only adults have sleds'. I have no idea how facts like these might be handled in a system where the entire meaning of a word-sized unit must be self contained.

UNINTENDED IMPLICATIONS

AM contains much detailed probing into the morphological connective tissue and the results, while interesting and important, sometimes point not in the direction A says they do, but, it seems to me, in a completely different one. A does a lot in the way of demonstrating the essential unity

and autonomy of the morphological system of language. But the unity of morphology does not entail a-morphousness and syntactic opacity. Further, A's demonstration of the unity of morphology makes rather suspicious the deep partitioning of morphology into inflection, derivation, compounding) and two types of cliticization.

Even English, with its relatively limited morphology, presents some difficult examples, negation being among them. Following the rather radical claim of Zwicky and Pullum (1983), A analyzes auxiliary-negative combinations in English as essentially inflectional and therefore relevant to syntax. Since this negation is inflectional and is relevant to syntax, one can at least imagine how to handle the variable scope of negation in such examples as *John can't swim* (i.e., 'It is not possible for John to swim') versus *John mustn't swim* (i.e., 'It is necessary that John not swim'). (See Horn (1989) for just about all one would ever want to know about this.) The problem comes in accounting for the apparent alternation of a free negative with an inflection, a kind of behavior that is elsewhere attributed to 'simple cliticization'. A writes: "The phrasal value [+Negative] can be realized as the element *not* in second position; but if the head (i.e., the first Verb) of the VP is an auxiliary, the value [+Negative] may be reassigned from the phrasal node to its head". This idea keeps the morphology pure and a-morphous, but what it does to syntax is give it something like the character of A's morphology to realize abstract features as 'processes'. Here the process is the addition of a word in a certain position in a phrase and if that is a possibility, then perhaps syntax could be handled as a-morphous as well! The disarmingly familiar facts of English negation are really quite intricate, involving simultaneously, and quite deeply, syntax, semantics, and morphology in a way that causes problems for the framework of AM.

Demonstrating the independence of morphology and syntax is also not the same thing as showing that the two components are separated in the traditional, hierarchical fashion that goes back to Sapir (1921) as filtered through Chomsky (1970). Much of what A digs up might, in fact, be used to demonstrate that syntax and morphology operate in parallel, or perhaps cyclically, as A assumes morphology and phonology do.

To see this, consider the Morphosyntactic Representations (MSRs) that A introduces in Chapter 4. These complex 'layered' feature representations are actually images of the syntactic organization of sentences into subjects, VPs, and objects. In fact, operations that are highly reminiscent of syntactic transformations can operate on these quasi-trees as illustrated by the following rule on page 152, which A considers in his analysis of the fabled inversion construction of Georgian.

$$\begin{array}{l}
 (6) \quad [\quad T/A, 0 [\quad Y [\quad Z]]] \\
 \qquad \qquad \qquad 1 \quad 2 \quad 3 \quad 4 \\
 \Rightarrow [\quad 1 \quad 4 [\quad 3 [\quad 0]]]
 \end{array}$$

Because morphology and syntax are separate and unequal in AM, A is careful to point out that (6) is a “*purely morphological rule*” [emphasis original]. The need to add this sort of power to the pure morphology is clearly a function of the way morphology and syntax are assumed to interact in AM.

Another indication that morphology has been unjustly imprisoned and its visiting privileges with the syntax unfairly curtailed is the fact that A is able to claim in Chapter 13 that “ergativity is completely accounted for within the morphology” (p. 352). This he is able to assert only because the relevant syntactic notions have been smuggled into the morphology in the featural layer cake of MSRs. But ergativity is surely a notion that demands a partly syntactic and/or semantic definition. It is not possible to discover ergativity or accusativity in any set of words and purely morphological processes in the absence of information as to what they mean or how sentences using them are constructed.

Or consider this: In Chapter 8 A discusses the parallelism that he finds between WFRs and a distinct set of rules called Clitic Placement Rules (CPRs) that operate to locate special clitics with respect to syntactic constituents. As motivation that these are to be identified with their morphological Jekyll, rather than their syntactic Hyde, he points out that there is something very like an inflection/derivation contrast among clitics and that furthermore, derivational-like clitics occur inside inflectional-like clitics, just as derivational morphology generally occurs inside inflection. But since WFRs are not CPRs, and since CPRs do not divide, *sensu strictu*, into derivations and inflections, A admits that this fact “has no obvious foundation in other principles” (p. 221). Note that something like the same is true of syntax: function words, corresponding in a rough way to inflections, generally occur outside modifiers, the syntactic analogues of derivational morphology. We expect to find determiner-adjective-noun, not adjective-determiner-noun. Now the functional similarity between the order of elements in syntax and morphology does not reduce the one component to another. In fact, I think that the clitic ordering facts are probably at least sometimes to be attributed to independent syntactic considerations, and sometimes to independent morphological factors. This could be made explicit in a theory where morphology and syntax are parallel, autonomous representations, but in AM, the artificial barriers to

communication between syntax and morphology requires the setting up of a special quasi-component that is neither syntax nor morphology just to handle special clitics.

On the question of the unity of morphology, one of the official themes of AM, there is a considerable inconsistency between what is preached and what is practiced. Chapter 2 concludes: "All of these facts suggest that the development of a theory of morphological structure is a well-motivated and distinct object of inquiry within linguistics. The following chapters will attempt to delineate the major properties of such a theory" (p. 47). Yet after the theory has been fully limned against a background of a-morphousness and asyntacticity, it turns out that there isn't a single theory after all, but a number of small theories: inflection, derivation, compounding, simple cliticization, and special cliticization. Each one of these is not only formally distinct, but apparently operates at a different place in hierarchical grammar. This leads, among other things, to a certain disappointment in A's quest for a morphological typology of language and the reason for this is, contrary to the conclusion of Chapter 2, "the fact that word structure is not really an autonomous domain in the same way syntax and phonology are" (p. 324).

CONCLUSIONS

AM makes a valiant attempt to build a morphological theory embodying the particular combination of ideas listed in (1) to (3) at the outset of this review. Judging from the factual and logical problems that the program of AM encounters, I think we can conclude that at least one of these assumptions must be abandoned. To my way of thinking, almost all of the logical inconsistencies and empirical woes that beset AM can be avoided by assuming an organization of grammar that departs from the ordered rule and ordered component architecture of grammar inherited from American Descriptivism. The ordered process approach results in lengthy and, to my mind, improbably intricate stages in the development of words. Indeed, A himself finds long, sequential derivations cognitively and computationally undesirable. In the conclusion to the fourteenth and final chapter, he suggests that the actual processing of language is quite at odds with the rule-based theory he espouses throughout the preceding text:

Human cognitive processing is unlikely to make use of long and complicated procedures applied very quickly one step at a time Suppose, however, that the processes underlying human linguistic performance are actually massively parallel, relatively slow, and locally rather simple. (pp. 400–401)

Here is a statement I can agree with completely, but one that cuts straight against the hierarchical, ordered-process grain of the rest of this book. After reading AM, I am more convinced than ever that we need to abandon hierarchical theories of grammar in favor of a research program that recognizes the full, parallel autonomy of the various components and puts all and only morphology in one component, all and only syntax in another, and all and only semantics in a third.

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