

On Some Issues in Morphological Exponence

Stephen R. Anderson*
Dept. of Linguistics, Yale University

A number of years ago, I presented an analysis of parts of the inflectional system of the Algonquian language Potawatomi (Anderson 1977), based on descriptive material of Charles Hockett (1948, 1966) and intended to illustrate some general points about the nature of inflectional systems. Essentially the same analysis appeared with some minor refinements in my morphology book (Anderson 1992) a few years ago, again to exemplify some basic issues of theory. This analysis has been subjected to a certain amount of critical examination in the subsequent literature, for which I am grateful, and some of the conclusions I drew from the Potawatomi material have been called into question. I do not intend in this article to deal with all of the points raised in this discussion, but rather to focus on a few issues that seem to me particularly significant for morphology *per se*, especially as these arise in the critiques of my analysis presented by Halle and Marantz (1993) and Steele (1995).

Both of those papers raise certain interesting and important points about the specific analysis of Potawatomi in my earlier work, points that surely need to be taken into account in any revision of that analysis, but which bear in only limited ways on more general issues, and which I will largely ignore here. Of more general interest is an issue raised in both papers, that of the status of “multiple exponence” in inflectional systems, and that will be my main focus in the present article. This is the question of whether a single property of (what constitutes from the point of view of the syntax) a single word can be realized at more than one point in the form of that word, and I think that properly understood it has extremely important consequences for our understanding of how morphological theory, and indeed the theory of grammar overall, ought to be interpreted.

I will begin by addressing two points that grow out of the analyses of Potawatomi presented by Halle and Marantz and by Steele, issues that are partly matters of detail, but which turn out to have significant implications for the more general issues at stake here. I will then go on to discuss the problem of multiple exponence itself. On the basis of some material from a completely different family of language, the Tibeto-Burman Kiranti languages of Nepal, as well as others, I will conclude that multiple formal realization of the same inflectional

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content does indeed occur in natural language, and go on to ask what we should make of that fact as far as the foundational premises of grammatical theory are concerned.

1 Some Background Issues

There are two general points that I must address about the analysis of Potawatomi before I can turn to my main concerns. These are, firstly, the difference between clitics and affixes, including the extent to which the demonstration that some material is a clitic renders it irrelevant to the morphological analysis; and secondly, the nature of mutually exclusionary or disjunctive relations among the principles governing the realization of inflectional content as overt morphological form.

1.1 Clitics *vs.* Affixes

Halle and Marantz (1993) take issue not only with my analysis of Potawatomi, but also with the account of Georgian inflection offered in Anderson (1984, 1992). Among the points on which they differ with those works is their claim that the underlined prefix elements in forms like those in (1) are not inflectional prefixes but rather clitics:

- (1) a. v-xaṭav ‘I draw him’
b. m-xaṭav-s ‘he draws me’
c. gv-xaṭav-t ‘you (pl.) draw us’

The claim that these prefixed elements are actually clitics is attributed by Halle and Marantz to Nash-Haran (1992), but in fact neither Nash-Haran nor Halle and Marantz actually cite arguments in favor this conclusion, which appears to be methodological in nature rather than empirical. And on the basis of the kind of argument often used to distinguish between affixes and clitics (cf. Zwicky & Pullum 1983 for example), the treatment of the Georgian markers in question as clitics seems rather dubious:

- (2) a. The prefixes interact seamlessly with the rest of the agreement system, which is certainly inflectional in character. For example, a few verbs have exceptional, idiosyncratic forms where the idiosyncrasy involves the prefixes in a way quite atypical for clitics. The copula, for example, shows a second person subject marking prefix *x*- which is structurally quite parallel to first person *v*-, but which does not show up with other Verbs. Such lexical selectivity is typical of affixes, and not of clitics.
- b. The prefixes bear the same relation to actual Argument positions in the clause that other agreement elements do: they do not replace Argument expressions, with which they can co-occur, but they sanction the appearance of null pronominals in the corresponding positions.

- c. The prefixes are located strictly at the left edge of the verbal root, and do not appear on other elements as a function of phrasal context (is is the case, for example, with true initial or second position clitics).
- d. The putative clitics appear inside of aspectual preverbs in those inflectional forms that require them: e.g., *da-v-xatav*.
- e. Some irregular Verbs have a conjugation in which more than one instance of the same prefix appears (e.g., *mo-v-di-var* ‘I will come’) without any apparent syntactic foundation that would motivate the appearance of more than one clitic with just these Verbs.

As a result of these considerations, and in the absence of serious arguments to the contrary, I reject the notion that the Georgian verbal prefixes are ‘clitics’ in any substantive sense of that term (see also Anderson in preparation). In contrast to this situation, however, when Halle and Marantz make the same claim about prefixal elements in Potawatomi (more generally, Algonquian) Verbs and possessed Nouns, they are on much firmer ground and are probably correct. This is actually a suggestion of Hockett’s (1948, p. 141) which is cited as such by Steele (1995, p. 273), though not by Halle and Marantz. These elements can be seen to be clitics rather than verbal affixes by the fact that when the Verb is itself preceded by certain pre-verbal elements within a containing phrase, the personal affixes attach to these rather than to the Verb:

- (3) a. i. n-wapma
 1-see.3anim
 I’ll see him
- ii. n-ku wapma
 1-Pvb see-3anim
 OK, I’ll see him
- b. i. n-nus:a
 1-kill-3anim
 I’m killing him
- ii. n-kuko? ns:a
 1-quickly kill.3anim
 I’ll kill him quickly

Hockett gives some other less secure arguments for the clitic status of these markers based on prosody, but the kind of positioning illustrated in (3) seems sufficient to establish the conclusion.

In their positioning, the personal prefixes in Potawatomi are interestingly similar to another element in Algonquian morpho-syntax which appears in several languages of the family: a replaceive vowel change which affects the leftmost vowel in the same projection of V as that in which the personal prefixes/proclitics are initial. This ‘initial change’ (to adopt the terminology of Bloomfield 1946 and elsewhere) functions as a sort of complementizer in marking certain subordinate constructions:

- (4) **Potawatomi:** a. ʔe k_i mpot
 PRT PAST died
 He died
- b. k_a mpot
 PAST (< k_i) died
 the one who died
- Menomini:** a. $\text{ne}=\text{pa:pam-nato:n}\epsilon\text{:hok}$
 me=going about-he seeks me
 He goes about seeking me
- b. (emeq) $\text{p}\epsilon\text{:pam-esiat}$
 (yonder) going about-he goes
 It is over yonder that he goes about
- Fox:** a. e:shki kano:n-ehka
 first (<ashki) speak-3sg to 2sg
 The one who first spoke to you
- b. $\text{k}\epsilon\text{:no:n-ehka}$
 speak-3sg to 2sg
 The one who spoke to you

Although non-segmental, ‘*Ablaut*’-like relationships such as the initial change illustrated in (4) are not the sort of thing we are accustomed to seeing as a clitic, the theoretical framework of Anderson 1992, 2000, in preparation, which sees clitics as the phrase-level analog of word-level morphology, leads us to expect such phenomena on a par with comparable markers in the form of words. What is interesting here is that both sorts of clitic — the complementizer marked by initial change in (4) and the person markers in (3) — are located in the same way, at the left edge of a projection of V (VP or perhaps \bar{V}).

Accepting the point made by Hockett, Halle and Marantz, and Steele, to the effect that the person prefixes in Potawatomi are probably clitics, what are we to make of it? Halle and Marantz seem to assume that once something has been shown to be a clitic, it is essentially the business of the syntax and the lexicon, and no longer relevant to the morphology. Steele (1995, p. 273) says explicitly that “[s]hould the person markers be clitics, they would not fall within the domain of inflectional morphology, according to [her view].” But this does not in fact follow. There are compelling reasons (developed in the references cited just above, among others) to believe that clitics are actually a subpart of a language’s morphology: the morphology of phrasal, as opposed to lexical, categories. As such, the clitic status of an element does not eliminate its inflectional relevance at all, but merely locates its realization at a phrasal rather than a lexical level. As we shall see, this choice has consequences for more general issues.

1.2 Principles of Disjunction

Much of the discussion surrounding my analysis of Potawatomi (and the theory of Anderson 1992 more generally) has focussed on the notion of disjunctive order as developed and applied

there. The theoretical importance of disjunctive relationships between rules was highlighted in early works such as Chomsky 1967 and in Chomsky & Halle 1968, where such relationships were argued to exist among the sub-rules of certainly formally related rule schemata. This particular conception of disjunction has all but disappeared from current discussion, though. Beginning with my own thesis (Anderson 1969), a notion originally found in Pāṇini’s grammar has become important, known, following Kiparsky 1973 as the ‘elsewhere’ condition. Continuing the discussion in Anderson 1986, Anderson 1992 pursues the question of where one morphological rule’s application precludes that of another. The conclusions reached there are the object of much of Halle and Marantz’s (1993) critical commentary.

Despite the force of the rhetoric in this paper, however, the issues dividing the position of Halle and Marantz from that of Anderson 1992 are not in fact great. Both theories maintain the core type of ‘elsewhere’ relation, whereby within a set of related rules (specifically, lexical insertion operations for Halle and Marantz and inflectional Word Formation Rules for me, though the issues are the same) a more specific case takes precedence over and blocks a more general case. Much criticism of the theory of Anderson 1992 has centered on the additional notion invoked there that morphological rules can be grouped together into blocks, such that within such a block, a disjunctive relationship can be essentially stipulated in the form of an extrinsic precedence relation. A number of commentators have rejected this grammar-specific variety of disjunction as theoretically undesirable.

The blocks involve actually correspond grossly to ‘position classes’ as these are invoked by other morphologists, though there are differences of detail which remain to be explored. In the theory of Halle and Marantz, the corresponding notion is that of the set of items which potentially provide realizations for a single abstract ‘morpheme,’ where the ‘morphemes’ are positioned with respect to one another (independent of their concrete realization) in part by the syntax and in part by autonomous morphological operations. Importantly, though, Halle and Marantz assume a stipulated relation of mutual exclusion in virtually the same cases as those of Anderson 1992, since the choice from among a set of potentially competing realizations for a single structural position does not always have a resolution in terms of specificity (the core ‘elsewhere’ principle). Some of the instances in which they explicitly assume this precedence to be a language-particular matter include:

- (5) a. Georgian personal prefixes (*g-* ‘2 Obj’ takes precedence over *v-* ‘1 Sbj’ at the same position);
- b. Potawatomi clitics (*k-* ‘2nd person participant’ takes precedence over *n-* ‘1st person participant’)
- c. Potawatomi ‘AGR₂’ (*-mun* takes precedence over *-wa* — cf. Halle & Marantz 1993, p. 151)

These are just the same as the cases where disjunction based on stipulated organization into rule blocks is invoked in the analysis Halle and Marantz are criticizing, cases that do not follow from more general principles on either view. In each instance more than one affix would appear to be motivated by the content of the form, but only one appears. I

described this by saying that the rules belong to the same block, and are disjunctive. Halle and Marantz say that they are alternative lexicalizations of the same abstract ‘morpheme’ and that such an element can only be realized once, with a stipulated precedence among the possibilities. The differences are clearly quite immaterial.

A third kind of disjunctive relationship assumed in Anderson 1992 precludes the application of a rule when another, more specific rule has already applied in an earlier block. Halle and Marantz criticize the technical adequacy of this formulation and its application to specific Georgian and Potawatomi examples, and no doubt there are refinements or revisions that must be made to the existing proposal. They, of course, face exactly the same class of examples, which they generally resolve by invoking language-particular rules of ‘impoverishment’ that remove certain features from representations in the presence of certain other features. The disjunction proposal of Anderson 1992 at least has the advantage of attempting to derive these relationships from a generalization of the well-supported core of ‘elsewhere’ cases, though the correctness of one or the other solution remains to be established through more extensive discussion. The point which should not be obscured, however, is that both theories invoke essentially the same three sets of disjunctive relations, with relatively minor differences of technical execution distinguishing them.

There is, nonetheless, a more essential difference between these views, even if this is difficult to present in truly empirical terms. This concerns the basic nature of the sets of mutually exclusive alternatives (rules, lexical realizations of a single ‘morphemic’ position, or in the case of Steele 1995, distinct morphological stem types), assumed by all theories to form some kind of unit within the grammar. Halle and Marantz assume these units are formed basically in the syntax, and correspond to syntactically coherent functional categories. Within their theory, however, purely morphological rules (with no syntactic motivation) can so alter these groupings through operations of fission, fusion, feature insertion and impoverishment, etc. between the output of the syntax and their overt realization that it is hard to see how the claim of a syntactic basis could ever be disconfirmed (or verified).

Both Halle and Marantz and Steele are quite insistent that the morphological content corresponding to a single structural position ought in principle to constitute a functionally coherent collection of features (e.g. ‘TENSE’ as opposed to ‘SUBJECT NUMBER,’ etc.). The theory of Anderson 1992, on the other hand, makes no such claim, and in principle allows for the gathering of functionally unrelated morphological material within a single rule block. The descriptive power of the devices assumed in a theory like that of Distributed Morphology makes this an issue which is virtually impossible to see as strictly empirical, but it is nevertheless one to which we will return below.

2 Multiple Exponence

All theories of morphology based on variants of the traditional morpheme must reject the notion that a single functionally present property can have multiple realizations, for contentful features in such a theory must be localized in particular morphemes, and there is no room in morphemic views for such duplication of the same property at more than one

structural position. It is no accident, then, that Halle and Marantz make a major point of the the claim that “[t]here is no ‘multiple exponence’ of features from a single syntactic or morphological node.” (Halle & Marantz 1993, p. 138).

Halle and Marantz have a strong commitment to the notion that a given feature is realized in exactly one place, though its presence may be relevant to the way other features are realized elsewhere. This derives from their embrace of a model where features are the content of abstract morphemes, manipulated and placed by the syntax. The fact that this claim is not in fact an empirical one, given the availability of devices such as fusion, fission, impoverishment, arbitrary morpheme-to-morpheme concord, etc., does not alter the principle. But it does not confirm it either, and indeed the claim does not seem to be correct.

The claim of unique exponence is important in Halle and Marantz’ reaction to the analysis of Potawatomi, since in that language, the same arguments trigger both the prefixal elements we have already noted and also suffixal agreement on Verbs (and possessed Nouns). If the same featural content were realized in multiple places in this way, they would have to recognize multiple exponence as a genuine problem for a morpheme-based view such as the one they present.

As acknowledged above, however, Halle and Marantz propose (correctly, it appears) to treat the prefixal elements as proclitics rather than as verbal agreement. According to them, the Potawatomi clitics “are not part of the Verb,” and thus are not comparable to the agreement elements that appear as verbal suffixes. Halle and Marantz adopt a position associated with Jelinek (1984) and Baker (1995), according to which in certain languages, actual A-positions are not occupied by overt argument expressions, but rather by empty pronominals with which overt expressions may stand in a relation of apposition. For them, the Potawatomi clitics are simply the realization of the featural content of these pronominal elements.

There are some difficulties with extending this account to Algonquian language: Baker (1995) examines the resulting syntax in some detail, and derives a number of highly specific predictions from it. These predictions are argued to be borne out in, e.g., Mohawk, but even accepting all of Baker’s data and the conclusions he bases on them, it is far from clear that this is also the case in Potawatomi (and more generally, in Algonquian). Even granting the claim that these languages have a structure parallel to that attributed by Baker to Iroquoian, though, this does not resolve the question of how to analyze the Potawatomi clitic prefixes. These cannot be adjoined expressions linked to the arguments themselves: there are overt pronouns, for example, which can serve as the associated appositional expressions on a Baker-type account without thereby replacing the proclitics. If the clitics are not to be treated as agreement, then, they must originate in the A-positions themselves; but the whole point of Baker’s analysis is that in a language of the type he considers, A-positions cannot be directly associated with overt phonological content, while the Potawatomi clitics certainly have phonologically overt realizations.

An alternative is provided by the theory of clitics as phrasal affixes developed in Anderson 1992 and subsequent work cited above. On that view, the clitics are phrase-level agreement markers, introduced into the sentence as a reflection at the phrasal level of essentially the

same morphosyntactic material as that which triggers ordinary agreement at the word level. But in that case, we do in effect have ‘multiple exponence’ after all, since the same featural material is realized overtly more than once, at multiple structural levels.

Steele takes a position quite similar to that of Halle and Marantz:

Similarly, A[rticulated] M[orphology — Steele’s own theory] explicitly precludes, as we have seen, what Halle and Marantz (1993, p. 138) reject under the term ‘multiple exponence’ [...] in the form of repeated and arbitrary reference to the same features in multiple blocks. [...]

The apparent multiplicity of marking found in Potawatomi, which is entirely unproblematic in A-Morphous Morphology, is impossible in AM. Because operations are informationally additive, multiple additions of identical information are precluded. [Steele 1995, p. 280]

Like Halle and Marantz, Steele also argues that the prefixes are probably clitics, and that therefore the apparent redundancy between prefixes and suffixes does not really represent multiple exponence. It is difficult to evaluate this in the absence of an explicit account of clitics within Steele’s theory, but there is no reason to doubt that the same objections raised above would apply in this context as well.

The other kind of multiple exponence apparent in Potawatomi concerns the suffixes which according to all views are genuine agreement markers. Since there are at least three positions in which such material is realized, and a good deal of overlap among the properties relevant at each position, multiple exponence would at least appear to be present here. Halle and Marantz resolve this by simply stipulating that their ‘AGR₃’ position shows ‘concord’ with ‘AGR₁’ in certain features (a move that surely evacuates any empirical claim one might have been tempted to associate with the rejection of multiple exponence), while Steele argues that it is possible to partition the featural content in such a way that her principle is obeyed in the letter. I will not attempt to evaluate the details of either account here, but it does not appear that either framework has successfully exorcized the specter of multiple exponence in the realization of Potawatomi agreement.

3 How Localized are Features?

Halle and Marantz’s ‘morpheme’-based view of morphology and Steele’s information-based theory have some important things in common. Both agree on the answers to two sets of questions:

- (6) a. Assuming the morphology of a language is divided into elements realized at distinguishable structural positions (corresponding to the features of ‘morphemes,’ the content of nested stem types, or rule blocks within the A-Morphous account), do the features associated with each position constitute a discrete, coherent and motivated subset of the total?

- b. Is it possible for the same element of featural content to have realizations at more than one position in structure (‘multiple exponence’)?

Halle and Marantz, and work influenced by them such as Noyer 1992, and Steele all say that the answer to the first question must be “yes” and to the second, “no.” Each has to introduce some unusual mechanisms to accommodate apparent counter examples, but these are grafted onto models of morphological structure in which inflectional properties are discrete and uniquely localized. All of these authors agree in criticizing the position of A-Morphous Morphology, which gives the opposite answers to both questions.

It is undeniable that morphological properties tend to cluster in their formal realization, in discrete and featurally coherent ways. This is why e.g. Bybee 1985, Cinque 1999, and other writers representing a variety of morphological frameworks can talk about the relative order of e.g. Tense, Aspect, Subject Agreement, Object Agreement, etc. in particular languages and in general, rather than always having to refer to specific morphological markers. It is also undeniable, I think, that while this is a strong tendency, it is not an absolute constraint. We can see this in many languages, of which two are chosen here as typical.

Skou, a language spoken on the north coast of New Guinea, has a striking degree of multiple exponence in its system of subject agreement, as described by Donohue 1999. There are four distinct ways in which the person, number and (in the third person) gender of the subject can be reflected in verbs in this language:

- (7) a. Pronominal agreement clitics appear on all Verbs;
 b. Some Verbs show vowel alternations corresponding to subject features;
 c. Some Verbs show alternations in the initial consonant in accord with the properties of the subject; and
 d. Some Verbs take a semantically empty cognate object, which may also show variation conditioned by the properties of the subject.

All four of these types of marking may be found in a single sentence, as illustrated in (8):

- (8) *Te* **te=i** *ti*
 3PL 3PL=fall.PL 3PL:go.3PL
 They fell over

Skou is a language that does not permit the dropping of subject pronouns, despite the robust marking of their properties on the Verb. The first *Te* in this example thus represents the subject position, with which the clitic **te=** agrees. Such clitics systematically double the content of overt argument expressions, and constitute a form of morphological agreement marking. In this same example, however, both the initial consonant and the vowel of the Verb root vary as independent functions of the properties of the subject, as does the shape of the preceding element (described as a ‘cognate object’ by Donohue). The full paradigm of this Verb is as in (9).

(9)	'fall'	SG	PL
	1	<i>kú re</i>	<i>i ne</i>
	2	<i>kú me</i>	<i>i ri</i>
	3.M	<i>kú ti</i>	<i>i te</i>
	3.F	<i>pí te</i>	<i>i ti</i>

It is always possible, of course, to say something such as that only the proclitics are 'real' agreement, with all of the other formal marking constituting merely the conditioned realization of lexical elements in the presence of certain features. The fact remains that this move only obscures the fact that the same features are realized at multiple points in structure, and does not really eliminate it.

Particularly dramatic problems are posed for the doctrines of locality and uniqueness of exponence by languages of the Kiranti family, a group of about a dozen Tibeto-Burman languages spoken in Nepal and neighboring areas. Surely among the world's more exuberant agreement systems, a number of these languages are described in sufficient detail to allow for reasonably secure conclusions, including at least three (Limbu, Dumi and Hayu) for which one or more full-length modern grammars are available. Representative of the family is Limbu, whose verbal morphology is summarized here:¹

(10)	Slot	Content	form	gloss
	Pfx-1	Person	a-	1
	Pfx-2	Person	kɛ-	2
	Pfx-3	Person	mɛ-/m-	3 non-sg A/S
	Pfx-4	Neg	mɛ-/n-/mɛn-	neg
	(Root)			
	Sfx-1	?	-siŋ/-nɛ/-n -nɛ/-n	ref 1→2
	Sfx-2	Tense	-ɛ	pret
	Sfx-3	Dual	-si/-s/-tchi/-tch	dual
	Sfx-4	Patient	-p -u	3 P pret (with 1sg A) 3 P
	Sfx-5	?	-mʔna -ʔɛ -aŋ/-ŋ -m	1pl excl A/S pret 1sg non-pret 1sg pl A
	Sfx-6	Neg	-n	neg
	Sfx-7	number	-si/-tchi	non-sg
	Sfx-8	Copy	-ŋ -m	1sg pl A
	Sfx-9	Excl	-gɛ/-bɛ	excl
	Sfx-10	Neg	-nɛn/-n	neg

¹"A" represents transitive subject, "P" transitive object, and "S" the intransitive subject.

The analysis here is that of van Driem 1997, a revision in various minor ways of the earlier account in van Driem 1987. The later paper, taken together with the earlier full grammar, provides a rather detailed justification for each of the affixal slots posited, including their separateness and their relative ordering. I have omitted the various zero morphs posited in van Driem's analysis as not directly relevant to the issues of locality and uniqueness of exponence.

Notice that the fourteen affixal positions in the Limbu Verb are not discrete or coherent: the same property (e.g. negation, 1sg) may be scattered across more than one position, and the same position may contain categorially heterogeneous elements.

A particularly obvious case of multiple exponence is presented by negation in Limbu, which is represented in either two or three slots, depending on the other features that are present. Thus, in *mɛ-uks-ɛ-tch-u-n-chi-n* 'they (du) didn't pick them' negation (and negation alone) is marked by all three of *mɛ-*, *-n-* and *-n*. Negation seems a particularly common candidate for multiple exponence: see the Muskogean examples cited in Anderson 1992 for example.

In the case of Limbu negation, the affixes involved are all quite similar in form, which leads to the question of whether they are in fact completely distinct, or rather represent multiple instances of the same affix. In fact, as we will note below, a number of the Kiranti languages do display affixes at one structural point that are more or less direct copies (at least etymologically) of other affixes appearing elsewhere in the same form. One example suggested to me by Matt Richardson (p.c.), who is working on a formal account of Limbu, concerns two separate Limbu affixes with the form *-m*. These appear at distinct positions (Sfx-5 and Sfx-8 in terms of the chart in 10). One appears alone and represents a plural subject in the presence of a third person object, as in *kɛ-kir-um* 'you (pl.) were afraid of him/her/it.' The other *-m* appears in addition to the first in forms with plural subject and third person non-singular object: e.g., *kɛ-kir-um-si-m* 'you (pl.) were afraid of them (dual or plural).' The feature combinations that trigger these two affixes, in the format of Anderson 1992, are [+pl [-me -you]] and [+pl [-me -you -sg]] respectively. One might describe this by saying that the first *-m* is copied or repeated under the more specific condition that the object be non-singular.

By no means all instances of multiple exponence in Kiranti languages are cases where a single affix can be said to be copied, however. In Dumi, for instance (cf. van Driem 1993), both the marker *-ŋ* appearing early in the suffix string and *-ə* appearing much later are markers of first person subjects; the two typically appear together, as in *dza-ŋ-pə-t-ə* 'I'm going to eat.'

The Kiranti languages, like many others in the world, display genuine multiple exponence, as well as a certain amount of deviation from the functional coherence of material filling the same structural position in inflectional morphology. Indeed, the existence of such phenomena will hardly come as major news to the majority of linguists working on morphologically rich languages: it would not really merit extended justification if it were not for the fact that such multiple exponence actually has very important theoretical ramifications, as seen explicitly both by Halle and Marantz and by Steele.

4 What is Morphological Theory a Theory of?

We have observed that the languages of the world tend to have a unique marker within a given form for a given property; and that the properties marked at a single position in morphological structure tend to be functionally related. On the other hand, we have also seen that languages involve violations of both of these tendencies. This naturally raises the question of how morphological theory ought to be constructed so as to respond to both of these seemingly contradictory sets of observations.

Apparently, we might proceed in either of two ways, depending on which of the two sets of facts we take as primary. We might, on the one hand, build morphology on a ‘morpheme’-like model, with unique informational loci for functionally related properties. In this case, we would treat the deviations as added complications introduced by distinct language-particular mechanisms (rules of fission, etc.). On the other hand, we might build morphology on the model of a generalized many-to-many association between properties and their exponents, in which case we would have to ascribe the tendency for these exponents to cluster and to be unique to some other factor that interacts with morphology *per se* to yield the class of languages we actually observe.

The first of these lines, which I associate with various of my critics cited (and uncited) above), winds up treating the complications as part of a theory of ‘Markedness.’ This may appear to be a principled move, but in fact I think it is an excellent instantiation of a tendency to invoke Markedness as a substitute for further thought. The effect of identifying some phenomenon as a ‘marked’ case is to provide a *label* for the instances that do not conform to the other principles of the theory, but that hardly provides an *explanation* of the facts. In this respect, the ‘Markedness move’ is entirely similar to the invocation of ‘zero morphemes’ within a theory that holds that every relevant property of a structure should be a property of some formal morpheme. In that context, ‘ \emptyset ’ is a label for the places where that requirement is not met, rather than an actual theory of these cases. Of course, some \emptyset ’s may have structural motivations that are not purely theory internal, but most of those we see are simply instances of a structurally relevant property which is not reflected in the overt shape of any form.

Because there is no apparent alternative to the (ultimately unsatisfactory) account of deviations from a strictly morphemic organization in terms of ‘Markedness,’ it is worth exploring the second alternative above: that morphological theory *per se* allows for a very general relation between properties and their exponents, including connections that are not one-to-one, that leave some properties unmapped and some markers unmotivated by substantive features, etc. This is the view argued for in Anderson 1992.

Why might we be tempted to take such an approach? Let us recall what linguistic theory (including morphology, of course) is in fact intended to be: an account of the structure of a particular cognitive system, and an explicit characterization of the kinds of structure which the mind can accommodate within a particular domain (e.g., word structure as the overt correlate of content). From that perspective, the fact that some languages *do* display multiple exponence and featural incoherence suggests that the minds of speakers can indeed come to

terms with such phenomena. But if that is the case, a cognitively adequate formal theory of morphology ought to have this kind of generality as well.

If that is the case, though, how are we to account for the obvious strong tendency toward coherence and uniqueness of exponence that we find in the languages of the world? The answer lies in a recognition that linguistic theory (and specifically, morphological theory) is not alone. That is, linguistic theory *per se* is not the only factor that determines the range of linguistic systems found in nature. The theory of possible linguistic systems interacts with other effects, and in particular with the range of possible diachronic developments and their sources, to yield the range of attested linguistic systems. That is, the set of actual languages lies in the intersection of those permitted by linguistic theory with the set of those for which a possible developmental scenario can be constructed. And some asymmetries in the set of observed languages may thus reflect asymmetries of developmental plausibility rather than constraints which should be embodied in the theory of the human language capacity itself.

Now we know that much of inflectional morphology arises over time as the result of a well-known cycle, by which e.g. full lexical words such as Nouns or Adverbs may be reduced (semantically and phonologically) to pronouns or particles; these in turn may become attached to other words as clitics, and subsequently re-analyzed as affixes or even incorporated in non-concatenative ways into the shapes of inflected words. This is a common account often offered for the development of Tense and Agreement markers, for example. another possibility, less often cited but still quite robustly attested, is the reduction of an original inflected auxiliary Verb to a formal extension of the lexical Verb stem with which it was originally associated in a periphrastic construction. Examples include the development of future forms in Romance (from cliticization and re-analysis of original forms of the auxiliary Verb ‘have’), and the complex inflectional paradigms of Muskogean languages documented by Haas 1969. But if such ultimately lexical sources are in fact valid for much if not all inflectional morphology, then it makes perfect sense that properties will cluster in a unique and coherent way, since their formal markers will have a unitary and coherent source — in their history. Actually, it requires some substantial historical restructuring to alter this picture and reduce the coherence.

But of course such historical restructuring *can* take place, as we see from the cases where coherence is not in fact maintained in the distribution of features. Furthermore, since the same forms can undergo more than one cycle of such cliticization, coalescence and reanalysis, the same features may come to be represented more than once within a single word.

Just such a development is proposed by van Driem to have taken place in the Kiranti languages, where the complex sequence of verbal suffixes reflect layers of original auxiliaries. He reconstructs two earlier stages of auxiliary structure, each of which underwent ‘univerbation’ with the associated base. A proposed reconstruction of the pieces of the proto-Kiranti Verb from van Driem 1990, 1991 would be roughly as follows:

(11)	me- 3pA	Stem	-nši Refl	-k non-Pret	-ŋa 1s/NPT	-ci 12dPS	-ŋ 1sA	-k 1p	-ni 2p	-ya excl
			+Aux ₁		-na 2	-ci dA(S)	-u 3P		-m 12pA	-i incl
			-tε Pret		-nya 1s→2				-ci 3dP	

Some of the same properties of agreement could be inflectionally relevant to more than one of the historical constituents of this structure, and the result is multiple exponence in its reflexes in the modern languages. Originally, the separate instances of marking for a single property were associated with structurally distinct auxiliaries, but we have no reason to believe that these earlier auxiliaries still correspond to syntactically relevant layers of structure. Synchronically, we must recognize it as we find it in, e.g. Limbu as genuinely multiple exponence.

I think, therefore, that it makes sense to develop a view of morphology on which rule blocks constitute potentially arbitrary groupings, even though they will generally have more or less coherent content. The coherence reflects the fact that rule blocks typically reflect the material that was realized by a unitary independent element which came historically to be part of the inflected word. The fact that this may be its origin, however, does not establish a constraint on synchronic reality, any more than other etymological factors do. Restructuring and other reorganization over time can and do obscure this kind of original uniqueness and coherence.

In the process of disentangling the the contributions to synchronic reality that are made on the one hand by the nature and constraints of the human language faculty, and on the other hand by the historical origins of particular systems, I think we learn some things about where we should and should not put our emphasis, e.g. in refining the analysis of Algonquian inflection. More importantly, though, I think we see some interesting lessons about the content of linguistic theory within a more general theory of the mind, when we realize that that theory only contributes to the determination of what we may find in nature, in a complex interaction with other aspects of linguistic reality.

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