

Morphological Universals and Diachrony

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Although linguistics is plausibly taken to be “the science of language,” the actual object of inquiry in the field has changed considerably over time. Prior to the influence of deSaussure in the first part of the twentieth century, linguists concerned themselves primarily with the ways in which languages have developed historically. For the next several decades, they devoted their attention to the external facts of sounds, words and sets of utterances. With the advent of the cognitive (or “Chomskyan”) revolution around 1960, however, they came increasingly to see themselves as studying the human language faculty: speakers’ knowledge of language and the cognitive capacity that makes this possible (Anderson & Lightfoot 2002), Universal Grammar. This is what our theories attempt to represent nowadays.

Unlike the documented facts of language history or the measurable properties of sounds and utterances, such a cognitive faculty is not directly observable, so the question naturally arises of how we might study it empirically. Two important modes of argument have emerged that are generally taken to aid in this enterprise. First, if we can show that speakers know something about their language for which relevant evidence is not plausibly present in the input on the basis of which they learned the language, we assume that this knowledge must be a consequence of the structure of the ‘language organ’. This is the argument from “the poverty of the stimulus,” and (despite the skepticism of some: e.g. Pullum & Scholz 2002) it has proven to have wide applicability, especially with respect to speakers’ knowledge of syntax.

A second line of argument is to assume that when we find that something is true of all (or at least nearly all) of the languages we can observe, it must be true of Language more generally, and thus a property of the human language faculty. The assumption that valid generalizations about language typology must be reflected in

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constraints within linguistic theory is widely agreed to, but is it really valid? Why should we believe that observed regularities across languages are a good guide to the structure of the language organ?

We can note that knowledge of language arises in the individual through the application of some learning strategy — a strategy that may be partly specific to the domain of language, and partly more general — to the data available during a sensitive period in early life. As a result, regularities which we find in the grammars attained by human speakers might have a variety of sources:

The Input Data: Only systems that correspond to the evidence available can be acquired.

The Learning Process: Only languages that are accessible through the procedure employed can be attained, so some cognitively possible grammars might not be learnable.

The Language Faculty: Only cognitively possible languages can be acquired, whatever abstract regularities may exist in the data.

The argument that cross-linguistic regularities provide us with evidence for the structure of Universal Grammar rests on the assumption that only the last of these is relevant. It assumes that a complete range of input data is (at least in principle) available, and that the learning system can (again, in principle) consider any possible account of those data, so that the only filter on the class of grammars acquired is the nature of the cognitive system, or Universal Grammar. But surely this is extremely implausible.

To provide a serious theory of the regularities we find across the languages of the world, we need not only a theory of the language faculty but also theories of the learning system and of various sources for regularities in the input data. In connection with the latter, an important source of regularities in the input is the nature and working of historical change. A variety of linguists from Baudouin de Courtenay to the present have suggested that many of the regularities we find in the grammars of the world's languages actually result from the fact that historical change tends to produce certain configurations and not others, rather than from cognitive limitations that would exclude the unobserved systems.

This paper examines the force of this argument as it applies to morphology. We look first at what seems to be a general correlation between case marking and verbal aspect, one which has been suggested to reflect a property of Universal Grammar, and show that the connection here is an adventitious effect of several converging patterns of diachronic change rather than a systematic property of human language. We look next at the claim that morphological theory should exclude

a particular formal device, metathesis, as the marker of morphological information, and show that the observed rarity of this device has plausible roots in the pathways of historical change rather than in a limitation of the language faculty. Finally, we consider the claim that morphological information should be biuniquely related to the markers that express it, as is implicit in morpheme-based models of word structure, and find that the general tendency to such isomorphism of form and content is again a reflection of plausible historical patterns, rather than being inherent in the structure of the language organ. We then briefly draw some broader conclusions.

Case 1: Split Ergativity and Aspect

Many of the world's languages display a pattern of nominative *vs.* accusative marking for the subject and (direct) object of a clause only under some circumstances, while other conditions result in ergative *vs.* absolutive marking. Such *split ergative* patterns are not distributed randomly, however. Typologists have observed that in a number of such cases, nominative/accusative marking is associated with a main verb bearing imperfective aspect (or some form derived from that source), while ergative/absolutive marking is associated with perfective aspect or its descendents. It has been widely assumed (Delancey 1981, Dixon 1994, Tsunoda 1985) that Universal Grammar should account for this correlation by positing some sort of privileged link between ergativity and perfectivity, accusativity and imperfectivity.

An alternative possibility, however, is that this apparent connection actually results from a quite different source, the pathways of historical change that produce innovations or shifts in case marking patterns. This was the conclusion of an earlier paper (Anderson 1977), in which I investigated several established sources for ergative case marking in natural language, as well as one source that leads to innovative accusative marking.

It has long been known that perfective verbal forms in many languages are historical innovations. Benveniste (1952) studied this process in a number of branches of Indo-European, and documented one common source of such perfects in the re-analysis of originally passive forms. The semantics of a sentence such as *The fish was cooked (by Julia Child)* typically includes the interpretation that the cooking in question is a *fait accompli*, and thus it is entirely plausible that the use of passives should be generalized as a way to focus on perfectivity. If the morphology of the passive is then re-interpreted as a signal of the perfect, the result is a construction in which the original, notional subject is marked with a special form (instrumental, or with a preposition such as English *by*) while the original, notional direct object appears in the same form as an intransitive subject:

- (1) (Original) NP_{Obj}-NOM — Verb_{Pass} — NP_{Sbj}-INSTR ⇒

(Innovative) NP_{Sbj}-OBL — Verb_{Pass} — NP_{Obj}-NOM

This development is widely considered to be the source of the ergative constructions found in the modern Indic languages, such as Nepali:

- (2) Sita-le aluma nun haleko chə
Sita-ERG potato-LOC salt-NOM put AUX
Sita (has) put salt in the potatoes

While there is still much to be said about the precise sequence of developments by which passives can give rise to later perfects, the possibility of such a development is not seriously in question for a number of languages. The perfects thus derived may themselves be re-analyzed subsequently as simple past tenses.

Assuming the original state of affairs within which this innovation takes place had a nominative/accusative system of case marking, the result is one in which (the new) perfect or past tense forms are associated with an ergative construction, while the (unchanged) non-perfect forms are associated with an accusative construction. This is a standard sort of split-ergative system, but we should note that the parameters of the split are determined by the case marking properties of the (passive) ancestor of the new perfect, not by some constraint of Universal Grammar.

In other languages, though, Benveniste (1960) documents a different source for innovative perfects. He notes that in language after language, whatever verbal expression serves to express possession is also pressed into service as a marker of the perfect — as is the case, indeed, in English, where *have* serves both functions. The expression of possession is often a transitive verb (like English *have*, Spanish *tener*, Latin *habeō*—not cognate with *have*, etc.). In some languages, however, a distinct prepositional construction is used:

- (3) **Russian:** U menya ∅ kniga
at me (is) book
I have a book
- Breton:** Eur velo c'hlas am eus
A bicycle blue at-me is
I have a blue bicycle

In case a construction of this type comes to be employed as a marker of the perfect, note the consequences. The subject of a transitive perfect verb will be marked with some oblique (originally locative) case, while the object will be marked in the same way as the subject in copular constructions: as a nominative. But as in the case of perfects descended from passives, the result is a situation in which the

new perfects are associated with what is formally an ergative constructions, while non-perfects are associated with the original (presumably accusative) construction. Benveniste argues that this can be seen in the origin of the Armenian perfect. Here the subject appears in the genitive, betraying the possessive origin of the construction, while the object appears in the accusative, presumably by a later extension of this case to all objects.

- (4) zayn nšan arareal ēr nora
 that miracle-ACC performed AUX he-GEN
 He performed that miracle

Benveniste proposes that the Old Persian form *ima tya manā krtam* ‘that is what I have done’ represents this same evolution of a perfect from a possessive in a ‘pure’ form (i.e., without extension of the accusative to the object).

Again, we have a split ergative system in which the perfect is associated with ergative marking, the imperfect with accusative marking. The two developments (from passives and from possessive constructions) have nothing to do with one another, and in neither instance is the case marking of the original construction mandated by Universal Grammar. The two developments happen to converge however, on systems with the same inherited, synchronically accidental correlation of case marking and verbal aspect.

A third, completely independent, development can also lead to the same result. Suppose that instead of innovating a perfect, a language were to reanalyze some construction as an *im*perfect verbal form. What original structure might be appropriate for this purpose? A plausible candidate would be a structure in which the object of a transitive verb, instead of being marked with a direct case such as the accusative, appears as a prepositional adjunct. English has a number of contrasting pairs of this sort:

- (5) a. i. Jones read ***War and Peace*** to his wife.
 ii. Jones read to his wife from ***War and Peace***.
 b. i. Fred shot my cat.
 ii. Fred shot at my cat.

In each of these pairs, the (ii) example is interpreted as an action not necessarily completely carried out, the object not completely affected, etc. Similar pairs form the basis of comparable contrasts in a wide range of languages, as discussed in Anderson 1988. The constructions in question clearly overlap semantically with the verbal notion of an ‘imperfective’. It would therefore be plausible for a structure in which a transitive verb is constructed intransitively, with its notional object

appearing in an oblique or prepositional form, to serve as the starting point for the development of such a category.

This is exactly what has happened in the history of Georgian, according to a suggestion originating with Braithwaite 1973, developed in Anderson 1977, and made much more precise in Harris 1985. On this account Georgian was originally a consistently ergative language. In the course of its history, a new series of imperfective forms developed from an ‘object demotion’ construction similar to (5). These forms underlie what are now called the ‘series I’ tenses, in which case marking is nominative/accusative. A different set of forms, the ‘series II’ tenses, continues the original situation.

Roughly, the division between series I and series II tenses can be seen as (originating in) a difference between imperfective and perfective forms. Again, as with the two paths of development for new perfects summarized above, the result is a split between ergative perfects and accusative imperfects. Again, however, this split should not be seen as mandated by Universal Grammar, but rather as the accidental consequence of the formal properties of the earlier construction on which the innovated forms — here the imperfectives, as opposed to the perfectives in the earlier cases — are based.

These completely independent developments all happen to converge on the same kinds of data. Each results in a state of affairs in which perfective forms (or their descendents) are associated with an ergative pattern, while imperfectives (or their later reflexes) are associated with nominative/accusative patterns. This is not, however, due to some regularity stipulated by Universal Grammar which relates case marking and verbal aspect: rather, it is an epiphenomenal regularity that emerges from a number of unrelated lines of development. This should suggest to us that not every pattern we can find in the data of language typology reflects the structure of the language faculty directly.

Case 2: Morphological Metathesis

Another set of issues revolves around the question of whether morphological theory should countenance the possibility of rules of metathesis: rules which simply rearrange the sequence of segmental material in a form to mark a grammatical category, with no concomitant addition of an affix or other marker. Some morphologists have argued that morphological metathesis rules ought to be excluded in principle from the theory, because such rules are (by definition) unformulable as concatenative affixes. Accommodating them would seem to entail a theory involving the full power of “the extremely rich transformational notation” (McCarthy 1981, p. 373), an undesirable result if we hope to provide a restrictive account of the notion

“possible morphological system.”

The possibility of metathesis (by itself) as a grammatical mechanism was first raised as a theoretical issue in Thompson & Thompson 1969, who cited a small number of potential cases. Although some of these have resisted all attempts to reduce them to affixal morphology, the number of cases is undeniably quite small, and this has led researchers to hope that the remaining ones would eventually yield to re-analysis as well, allowing for the preservation of the notion that all morphology is affixation.

Arguing that although rare, morphological metathesis must nonetheless be accommodated by a general theory of morphology, Janda (1984) proposes that the explanation for the very small number of plausible cases is rooted in facts about historical change. He argues that morphological metathesis is rare because historical changes that might lead to such a situation are rare. Non-affixal morphology arises when an originally phonological alternation is reanalyzed as morphologically conditioned. but Janda argues that *phonological* metathesis processes are quite rare, and thus the opportunity for a language to morphologize such a rule is hardly ever presented.

This argument has an affinity with the program of Evolutionary Phonology proposed recently by Juliette Blevins (to appear). She argues that much of what we find (or fail to find) in synchronic phonologies is not a product of the basic structure of the human language faculty (as represented by linguistic theories of various domains). Instead, many (perhaps most) typological generalizations result from the pathways of historical change and their results. If historical change operates in such a way as to favor or disfavor certain situations, its results are what we will find, and such generalizations are thus at best a poor guide to the structure of the language faculty itself.

Going back to Baudouin de Courtenay 1895 [1972], still one of the most comprehensive reviews of the processes governing the “life cycle” of alternations, we see that the main path by which morphological processes emerge is when an originally phonological regularity becomes increasingly opaque as a result of other changes. When the phonological conditioning factors for an alternation become lost (or at least difficult to recover from surface forms), it may be reinterpreted as aligned with morphological factors. To the extent phonological bases for such a change are lacking, we would expect the corresponding morphological rules to be rare or absent, regardless of the character of morphological theory *per se*.

Unfortunately for the viability of this explanation, phonological rules of metathesis are actually not rare. In a series of papers devoted to this subject, Blevins and Garrett (1998, to appear) have shown that there are several systematic types of sound change that can result in phonological metathesis rules, and that a substantial number of such processes do in fact exist in a wide variety of languages. If mor-

phonological metathesis is rare, then, it cannot be because there are no phonological processes to serve as its precursors.

Given that synchronic phonological metathesis is a real (and not especially rare or exotic) phenomenon, a historical explanation for the rarity of corresponding morphology must take some form other than the one proposed by Janda. Let us ask how morphological metathesis might be expected to arise in a grammar. As noted above, this is most likely where antecedent phonological processes have become opaque as a result of later changes. Eventually, language learners come to align the alternation with some grammatical category, rather than with a phonological trigger whose presence in the environment is highly abstract or perhaps no longer visible at all. On that basis, we can ask how plausible it is for phonological metathesis to be reanalyzed as morphological in this way.

Blevins and Garrett, in the works cited above, identify four categories of phonological metathesis processes:

Perceptual metathesis, in which a phonetic property realized over a multi-segmental span of the utterance becomes mis-allocated and is attributed to a segment other than the one from which it originates in the sequence.

Compensatory metathesis, in which a foot-peripheral syllable node is lost and the phonetic content originally assigned to it is re-assigned in a way that does not respect the original phonetic sequence.

Coarticulatory metathesis, in which overlap of gestures in adjacent segments leads to ambiguity with respect to their original order.

Auditory metathesis, in which fricative noise becomes decoupled from the sequential speech stream and re-assigned to a location other than its original one.

Of these possibilities, compensatory metathesis does not really count in a sense, because the primary operation involved is not a re-ordering but rather the loss of prosodic structure, with “metathesis” emerging as a concomitant. One of the instances cited both by Thompson & Thompson 1969 and Janda 1984 is the formation of the incomplete phase in Rotuman. Historically, the primary operation here is not a re-ordering, but rather, alternations due to distinct prosodic contours, where stress prevents vowel loss in one form, and its absence gives rise to vowel loss in another. The Rotuman example has been shown conclusively (Hale & Kissonock 1998, McCarthy 2000) to have this character.

Compensatory metathesis, as found in Rotuman, is extremely rare. In this example, it entails a prior morphological contrast whose only exponent is the location of stress. A two stage sound change with extreme CV coarticulation followed by

loss of peripheral unstressed vowels then yields, through restructuring, a morphologically conditioned relation which (in part) mimics CV metathesis. Where such systems occur, morphologically conditioned metathesis can emerge as a result, but they fall together with the “crazy rule” cases considered below.

The remaining three types of metathesis are each limited to specific combinations of segment types: laryngeal, rhotic, etc. and vowel for the perceptual type; $p+k$ (becoming $k+p$) for the coarticulatory type; and sibilant plus stop for the auditory type. Crucially, in all three varieties, the conditioning factors are entirely internal to segments undergoing the positional interchange. That is, there is no external conditioning factor for any of these processes, such that that aspect of the structural description could become opaque or be lost altogether. Since the elements that undergo the change are themselves its trigger, the normal historical processes of morphologization can gain no foothold.

Compare this situation with processes such as Umlaut, for example, in which some element (e.g., a high front vowel or glide in a succeeding syllable) conditions the change but is not part of it. When this element itself undergoes change (e.g., reduction to schwa in unstressed syllables), the alternation can persist in morphologized form. No such development is possible for the well established types of phonological metathesis, however.

If there is no natural path by which phonological rules of metathesis can be morphologized, does this mean that metathesis is confined to the phonological domain? No, for while the re-analysis of a corresponding phonological rule may be the most straightforward source for a morphological rule, it is not the only one. In fact, the case which was first cited (by Thompson & Thompson 1969) in this regard, the relation between the “non-actual” and the “actual” forms of the verb in Northern Straits Salish languages like Klallam and Saanich, turns out to be a valid instance of “metathesis as a grammatical device.”

In Klallam pairs like those in (6), for example, a sequence of consonant plus vowel in the “non-actual” form is inverted to produce the “actual” (a form with a semantic interpretation that includes that of the English present progressive), with no accompanying affix or other factor that could be said to condition the change.

(6) Klallam: $CCV \rightarrow CVC$

Non-Actual	Actual	gloss
qq'í-	qíq'-	tie up, restrain
pk ^w ǎ-	pók ^w -	smoke
čk ^w ú-	čúk ^w -	shoot

Where does such a relation originate, if not in an originally phonological rule of metathesis? Demers 1974 argues that in the related language Lummi, the original process involved a rule copying vowels (converting CCV into $CVCV$), followed

by a shift of stress in the resulting forms (converting $C\check{V}C\check{V}$ to $C\acute{V}C\check{V}$), and finally loss of the unstressed vowel to yield $C\acute{V}C$. This sequence is plausible as a historical account of the origins of the form of the “actual,” and may even be valid as a synchronic analysis of the facts of Lummi. Unfortunately, however, the crucial rules are not operative in Klallam, or in another relevant language, Saanich (Montler 1986, 1989):

(7) Saanich: $CC\grave{a}C \rightarrow C\grave{a}CC$

Root	Non-Actual	Actual	gloss
θk^w-	$\theta k^w\acute{a}t$	$\theta\acute{a}k^wt$	straighten (something)
$t's-$	$t's\acute{a}t$	$t'\acute{a}st$	break (something)
$t^{\theta}t\grave{a}k^w,$	$t^{\theta}t\grave{a}k^w,$	$t^{\theta}\acute{a}k^w,$	pinch (something)
$\lambda'p\grave{a}x$	$\lambda'p\acute{a}x$	$\lambda'\acute{a}px$	scatter (something)
$x^wq'p'\grave{a}t$	$x^wq'p'\acute{a}t$	$x^wq'\acute{a}p't$	patch (something)

The Saanich facts are discussed by Stonham 1994, who offers an analysis on which the alternations in (7) do not instantiate grammatically conditioned metathesis, but are rather the result of the addition of a mora in the actual forms with concomitant re-organization of segmental material. Stonham’s account involves unusual assumptions about the nature of the association between segmental and prosodic structure, but in any event it does not extend to a full range of the relevant cases. As he notes (Stonham 1994, pp. 175f.), metathesis of a CCV root to CVC would close the syllable, thus plausibly satisfying a constraint that the ‘actual’ should have one mora more than the ‘non-actual’ (assuming it could be shown that Saanich and Klallam are languages in which coda consonants are moraic, which is not obvious from the rest of their phonology). But the forms in (7) do not conform to this description. Montler (1989) shows that roots like the first two are actually vowel-less in their basic form, and become eligible for conversion to an ‘actual’ form through the addition of a stressable suffix such as $-\acute{a}t$ ‘control transitive’ which already has a closed syllable. Metathesis would thus not have the desired effect of adding a mora to such stems. The same is true of any root whose basic form already contains a coda consonant, such as the last three in (7), where the transposition of a prevocalic consonant into the coda cannot be said to satisfy such a prosodic requirement for an additional mora. We could only reconcile these examples with Stonham’s analysis by assuming that multiple coda consonants can contribute multiple moras to the prosodic weight of a form, something that has not been claimed for any language and which would be extremely hard to justify. See also Kurisu 2001 for discussion of this case, which we must conclude is a genuine (if isolated) instance of “metathesis as a grammatical device.”

Cases of this sort do not counter-exemplify the claim above that natural processes of historical change do not produce morphological metathesis rules from

originally phonological metatheses.¹ The reason is that the origin of the non-actual metathesis in Salish is apparently something like the path identified by Demers. As such, it is a matter of restructuring rather than simply morphologization. Processes of rule inversion, telescoping, and the like were identified at least as early as Bach & Harms 1972 as the source of “crazy rules,” rules cut off from their original phonetic motivation through the ongoing reanalysis of alternations by successive generations of speakers. This is a known source of grammatically conditioned metathesis: Garrett & Blevins 2004 discuss other instances in which metathesis rules have arisen within the Lexical Phonology of a language through restructuring without having a source in a phonetically natural metathesis process.

However inconvenient this may be for theories that assume all morphology to be based on affixation, then, it is necessary for morphological theory to recognize purely non-affixal markers for grammatical categories. If such markers are rare, the explanation for that fact is to be sought not in the nature of the human cognitive capacity for language, but rather in the paucity of historical scenarios that could yield such a process in practice.

This should not be particularly surprising, if we look at a broad range of evidence for the nature of the capacity with whose structure we are concerned. Language games, secret languages, and similar systems show widespread use of re-ordering, as is evident from a systematic survey such as that of Bagemihl 1988. These often instantiate processes which are extraordinarily unlikely ever to be found in any naturally occurring language. One might claim, of course, that such systems are outside the scope of normal language, but the facility with which they are acquired and used in a wide range of the world’s cultures makes that unlikely. Indeed, Bagemihl shows that the processes that set them apart from “normal” systems can be precisely placed with respect to the rest of the grammar, and that it is really only their unusual content that differentiates them from other rules of phonology and morphology.

We should probably conclude that the rules of such systems display a freedom not available to naturally occurring languages precisely because they are not constrained to arise through the usual processes of historical change. Their rules need not originate in perceptual or articulatory effects of the sort argued by Blevins (to appear) to underlie changes of the more familiar sort, but are constrained only by the imaginations of speakers. Further, since there is no “intelligibility constraint” on the relation between the base language and a secret or language-game variant (indeed, precisely *unintelligibility* is sometimes the essence of this relation), these can differ much more dramatically than in the case of systems developed through

¹Examples of this sort, where metathesis emerges from the historical re-analysis of other cumulated processes, are referred to as “pseudo-metathesis” by Blevins & Garrett 1998.

transfer of a language across generations. These examples provide us with a kind of laboratory, then, in which we can observe some of the differences between what is “natural” (in terms of our phonetically based expectations) and what occurs in nature. The existence of grammatically conditioned metathesis rules is not at all unexpected in this context.

Case 3: Multiple Exponence

A number of views of morphology assert, as a matter of theoretical necessity, that a single category of content which is reflected in a given word must be indicated by exactly one formal marker (Halle & Marantz 1993 Noyer 1992 Steele 1995). That is, they deny the possibility of what some (e.g. Matthews 1972) refer to as “extended” or “multiple exponence,” in which the same category is reflected formally in two or more distinct components of the word’s morphology. The more seriously one is attached to a model based on the classical notion of the “morpheme” (an irreducible one-to-one association of a piece of form with a piece of content, the minimal Saussurean sign), the more important this matter becomes.

A historical perspective might suggest that the requirement of simple or unique exponence of morphological categories is a plausible one. Morphological markers typically represent pieces of form that have gradually shifted in status over time from fully independent words through phonological reduced forms (“simple” clitics) to clitics more intimately associated with their host, eventually becoming affixes. If this path of development is indeed the origin of all morphological markers, it makes sense that the components of content within a given word should be bi-uniquely related to the components of its form.

Apparent counter-examples to the requirement of uniqueness of exponence are typically dismissed by designating one of the markers as the “real” one, and assigning other formal reflections of the same category the status either of special stem forms associated (non-distinctively) with certain categories, or of morphophonemic changes triggered by the primary marker. For instance, in German *Kraft/Kräfte* ‘strength(s)’ the category of plural appears to be marked twice, once by the ending *-e* and again by Umlaut of the stem vowel. One might say that Umlaut is a “morphologically conditioned phonological rule,” or that Umlaut is a property of a special variant of the noun’s stem; and that only the ending is a genuine plural marker. At minimum this analysis is not obvious, given the existence of other words such as *Tag/Tage* ‘day(s),’ *Jahr/Jahre* ‘year(s)’ in which the ending *-e* alone marks the plural, without Umlaut, and *Apfel/Äpfel* ‘apple(s),’ *Graben/Gräben* ‘ditch(es)’ in which Umlaut alone serves this function.

I have argued (Anderson 2001) that it is impossible to maintain the constraint of

“one category, one marker” as a requirement on morphological theory in this way without completely trivializing it (as Distributed Morphology does, for instance, with its array of post-syntactic morphological manipulations including fission, fusion, impoverishment, arbitrary and stipulated morpheme-to-morpheme concord, etc.). Despite the fact that morphological categories and markers line up in a one-to-one fashion in the vast majority of cases, this cannot be a requirement on morphological structures, because in at least some cases, it is violated without any evidence that the result is ill-formed or unstable.

A particularly robust system displaying such multiple exponence is that of verbal agreement in the Kiranti languages of Nepal and neighboring areas (van Driem 1990, 1997). In a form such as Dumi *dza-ŋ-pə-t-ə* ‘I’m going to eat’ both the *-ŋ-* and the final *-ə* are markers of the first person subject. Such multiple marking of the categories of a verb’s arguments is very widespread in all of these languages—indeed, it is the exception, rather than the rule, that a given argument is marked only once in a language like Dumi.

Again, we can look to historical change for the bases of (at least some) instance of multiple exponence. In Dumi or, somewhat more perspicuously, Limbu (van Driem 1987), the verbal agreement markers (apart from a limited set of prefixes) group themselves into two suffix clusters, each of which may contain markers for the same or similar properties of the same argument(s). What is responsible for this state of affairs is clear, on van Driem’s reconstruction of the family.

A reasonably common historical source of agreement markers in a language is an original inflected auxiliary. Such an auxiliary may be associated with some or all (lexical) main verb forms; like other words, it may undergo reduction to a simple (and later a special) clitic, thus coming to be attached to an associated uninflected form of the lexical verb. This reduced form of the auxiliary may then come to be reinterpreted as morphology on the verbal base, rather than a separate element. The Muskogean languages, for instance, have undergone such a development, as argued originally by Haas (Haas 1969) and subsequently confirmed in the study of several of the individual languages.

What has happened in the Kiranti languages is that this developmental pattern has occurred not just once, but (at least) twice in the history of languages like Limbu and Dumi, each time leaving a new set of inflectional markers on the verb. When one examines the patterns of marking within each subset of the suffixes, it becomes clear that the pattern of marking was not the same in the two historical inflected auxiliaries that are now reflected on the verb, but the arguments with which they show agreement are the same, and many of the same category distinctions are made in both cases. The result is a pattern that displays (at least) two distinct markers on the verb corresponding to the same agreement information relevant to a given argument.

While this repeated process of auxiliary reduction is obviously unusual, it does not seem theoretically problematic, and thus the clear instance of multiple exponence to which it gives rise should not be rejected either. Though inconvenient for morpheme-based models of word structure, many-to-many relations between a word's formal markers and the categories they reflect are simply a fact of linguistic structure. Just as the predominance of one-to-one marking has its explanation in the paths of historical change (along which markers typically originate in the progressive reduction of full words), so also the exceptions to this principle have a clear motivation in the historical morphology of individual languages.

Conclusion

We conclude that what we find in language is only partially explained by what is “natural.” Some things that we find in the morphology of a language are there not because the language faculty requires them but because change tends to create them for independent reasons; while some things that are rare or perhaps even non-existent are not to be found because there are few if any pathways that could produce them from an available source. These observations have surprisingly important consequences: they mean that our account of the human cognitive capacity for language cannot be based simply on generalizations about what we find in the languages of the world, or on what can be grounded in some other domain, such as phonetics. The cognitive capacity we hope to capture may well be much more flexible than we might think at first glance, and as a result, it may be considerably harder to determine its properties than has been assumed.

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