Why do Languages Develop and Maintain Non-Concatenative Morphology?

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0. Abstract

It seems to be widely assumed that morphological apophony of the foot/feet type always arises diachronically from concatenative morphology. Comrie (1992) explicitly proposes this, concluding logically that if one goes back far enough into the history of any particular language one must reach a point at which there was no non-concatenative morphology. Meanwhile, a recurrent trend of research in synchronic morphology has been to show that at the deepest level all non-concatenative morphology can be explained by syntactic—combinatorial—principles (Lieber 1992, Stonham 1994), hence that at the level of universal grammar (the innate knowledge of linguistic principles hypothesized to underly language acquisition) there are no principles specific to morphology, much less to non-concatenative morphology. But if non-concatenative morphology was not in any language to begin with and if there are no innate principles which allow speakers to create or interpret morphology of this type, how could it ever possibly have arisen? In other words, to take a concrete example, why has the original subphonemic variation that gave rise to Germanic umlaut plurals been phonologized and then morphologized by subsequent generations rather than simply ignored?

I propose that the solution to this paradox is to be found in empirical evidence from first language acquisition and a maturationist theory of acquisition as developed by researchers in this field (Piaget 1948, Ingram 1989).The maturationist theory says that the language faculty (defined not as innate knowledge but as the cognitive abilities required to process language structures) develops in stages as an infant matures. The ability to process phonology and to memorize words appears before the ability to process syntax or to apply morphological rules. For morphology specifically, it has been shown that ablaut past tense and irregular plural patterns in English are initially learned without difficulty, and only at a later stage do children over-generalize -s plurals or –d past tense forms, before eventually settling on the standard system (Bowerman 1982, Brown 1973). I propose that the structure of normally transmitted languages reflects this developmental process. Children create non-combinatorial morphology from combinatorial input, because the former is actually easier for the immature language faculty to process. Non-concatenative systems tend to become structured in such a way as to facilitate memorization—by being organized on the principle of marking minimal functional/ semantic differences by minimal phonological differences. This hypothesis explains certain widely attested and systematic properties of non-concatenative (sub-) systems which can find no coherent explanation when such systems are analyzed as being underlyingly combinatorial. The hypothesis can also be extended to account for one of the characteristic differences between naturally transmitted languages, on the one hand, and pidgins and creoles, on the other. The latter are characteristically less rich in morphological devices than their sources. This is due not to the fact that creoles are created de novo by children using only the resources of UG (Bickerton 1981, 1990) but because creoles by definition derive from pidgins; and pidgins are created by adults, who have the full cognitive resources of the mature language faculty. Artificial languages like Esperanto, also created according to adult conceptions of simplicity, share this feature with pidgins. Finally, and quite speculatively I propose (in agreement with Carstairs-McCarthy 2005) that the principles governing non-concatenative morphology may reflect an older pre-syntactic phase of human speech.

1. Theoretical Preliminaries: Language acquisition and language change

Three major hypotheses are implicated in the paradox laid out in the first paragraph, along with several variants and implications of each. The first hypothesis is that child language acquisition is the locus of grammatical reanalysis which gives rise to grammatical change. This is a venerable hypothesis (Paul 1880, Kiparsky 1968, Andersen 1973). It is not uncontroversial. It has been challenged particularly from the side of sociolinguistics (see, for example the discussion in Hock 1991, ch.20). But I am not going to challenge it here. Rather I am going to assume this to be the case in order to focus on a paradox that arises from this assumption: Children can only be expected to change language if there is something different about the way that children and adults process language. (That is, if the initial state of the language faculty and the mature state are different.)

The second is Comrie's hypothesis regarding the historical source of non-concatenative morphology. This should be broken down into several variants. The first is that concatenative morphology is the *only* source of non-concatenative morphology. I am personally skeptical of this, and can at least imagine two other ways in which such morphology might develop. One is by analogic extension of what were originally (accidental) phonaesthetic similarities: *glow*, *gleam*, *glitter* >> *glitz*. The other is that one type of non-concatenative morphology may develop into another through re-analysis. This is more easily documented. Semitic and Afroasiatic languages provide many examples. One type of change, for example, involves the shift from a fixed syllabic/vocalic pattern to reduplication as a means of marking noun plurals in some Semitic, and possibly other Afroasiatic languages (Ratcliffe 1996). On the analogy of Classical Arabic qitˤtˤ pl.qitˤatˤ "cat" (f.), (formed on the same pattern, CiCaC, as *xirqa* pl. *xiraq* "rag"), one gets Maltese reduplicated plurals like *umbrella*, pl. *umbrelel* "umbrella" (Mifsud 1994). The fixed pattern plurals of Classical Arabic, in turn, are likely to have arisen from an earlier process of forming plurals by -a(a)- infixation (Greenberg 1955, Ratcliffe 1998). (i.e. CA *kalb*, pl. *kilaab* probably goes back to something like *kalb*, pl. **kalaab*).

A second, weaker variant of Comrie's hypothesis is that concatenative morphology is one possible source of non-concatenative morphology. This is demonstrably true (as the Germanic umlaut case shows), and as long as it is so, the paradox persists.

A third variant of the hypothesis, somewhat stronger than the previous, is that the change from concatenative to non-concatenative morphology is unidirectional. Speakers may reanalyze a concatenative opposition as non-concatenative (*fot/foti* >> *fot/føti*>> *fot/føt*) but the reverse will not occur (*f*ot/føt* >> *fot/foti*). From the evidence available to me at present this unidirectionality hypothesis does appear to hold. If so the paradox is strengthened. Of course this proposal depends upon drawing a sharp distinction in principle between *reanalysis* of an existing opposition and complete *replacement* of one term of an opposition through analogical extension (*hand/handa* >> *hand/hands*).

The third major hypothesis implicated in these issues is of course Chomsky's initial state/universal grammar theory of language acquisition. This theory asserts that the ability to acquire a language is equivalent to innate knowledge of universal language structures. What specifically these structures might be has never been explicitly specified, but the assumption has been that they are restricted to principles of syntax, since syntax supposedly shows less variation cross linguistically than lexicon or phonology (Chomsky 1993, p.1-3)[1]. It is this hypothesis which motivates word-syntax approaches to morphology—i.e. the attempt to incorporate morphological structures into the hypothetically innate and universal structures of syntax. Hence this is the ultimate source of the paradox pointed out in the first paragraph.

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[1] *"UG is a theory of the initial state (S0) of the relevant component of the language faculty….."*
" UG is concerned with the invariant principles of S0 and the range of permissible variation. Variation must be determined by what is "visible" to the child acquiring language, that is, PLD. It is not surprising then, to find a degree of variation in the P[honological] F[orm] component, and in aspects of the lexicon…. Variation in the overt syntax or L[ogical] F[orm] component would be more problematic, since evidence could only be quite indirect. A narrow conjecture is that there is no such variation: beyond PF options and lexical arbitrariness… variation is limited to nonsubstantive parts of the lexicon and general properties of lexical items."

Here too several aspects of the hypothesis should be separated out. First the initial state theory, often termed a constructivist theory of acquisition, assumes that the language faculty is unchanging, essentially the same in infants and adults. Acquisition involves the fleshing out of a pre-existent syntactic skeleton with the language particular details of phonology and lexicon. One problem with this theory is that it offers no explanation for the developmental stages observed by language acquisition researchers (section 3, below). Another problem, relevant here, is that it makes it very difficult to attribute grammatical change to reanalysis by children. If children bring to the task of acquisition an adult level competence, there is no reason why they should analyze adult structures differently from adults. At least there is no reason why grammar variation should be any greater between adults and children than grammar variation among adults of the same community. If grammatical variation is simply endemic in speech communities, with no age or generational bias, there is no reason why change should occur over time, and certainly no reason why it should have any unidirectional bias—as seems to be the case in the change concatenative to non-concatenative (as well as in other types of change such as grammaticalization, Hopper & Traugott 1993).

A second aspect of the initial state theory which is more subtle, but ultimately more important is that it incorporates an assumption that language and the ability to acquire it belong to the same ontological domain, in other words, that they are the same kind of thing. This may turn out to be the best hypothesis, but there is no a priori logical reason why it must be true—as Chomsky and other generativists appear to assume. There may be abundant empirical support for the idea that humans have an innate ability to acquire a language, that this is a species specific ability, etc. But none of this tells us what kind of thing this ability is, nor requires us to believe that it is the same kind of thing, or some subset of, the knowledge of a language which is eventually acquired. The standard Generative formula "language is part of the mind/brain" simply obscures the crucial issue behind a slash mark. Simply to assert that something (language) is a mental object does not necessarily tell us anything about the neurological structures of the brain which support it.

The alternative view, that a language and the ability to acquire it are different kinds of things is a priori equally plausible. In normal speech when we say that something is (all) in the mind we do not mean that it is part of the neurological structure of the brain. Rather we mean precisely that it is imaginary, not located in the real world at all. I would suggest that it is in this sense too that we should interpret the notion that "language is in the mind." This viewpoint is widely attributed to the philosopher Karl Popper, who draws an ontological distinction between world one—the domain of real or objective objects, which exist regardless of whether people are there to observe them—and world three or imaginary objects, including language, which exist and can exist only in the human mind. (World two is the domain of emotions and feelings.) On this schema the ability to acquire and process a language is a world one object, specifically a neurological function of some kind. A language, on the other hand, is a world three object, that is an imaginary object—an artificial set of cultural norms which members of a speech community agree to imagine. On this view language is structured, of course, but we can not assume that the structure which linguists discover through analysis of languages directly reflect neurological structures in the brain. The sources of structure vary. The process by which a language is acquired may effect how it comes to be structured as it is.

If we adopt the hypothesis that knowledge of a language and the ability to acquire that knowledge belong to different ontological domains, are different kinds of things, it opens the way to looking at the maturation of the language faculty and the acquisition of a particular language as independent processes. Grammar does not grow in the brain. The brain grows, develops. As this is happening the child masters the linguistic norms of the community into which it is born. Normally-- in the course of normal language transmission-- the two processes occur to together. As a child matures it also learns a first language. But in the early stages a child may lack the cognitive ability necessary to process adult structures in the same way that adults do. At the same time the child may be highly sensitive to features of adult speech that are redundant or insignificant for the adult speaker. The ability to recognize the relatively subtle

difference in vowel quality between o/ø in a hypothetical pair like *fot/føti* matures before the ability to analyze the second member of the pair into a sequence of constituents: stem plus suffix. Memory also comes into play earlier than analytical ability. Hence it may be easier for young children simply to learn morphologically related words as individual non-complex words rather than to break them down into morphemes on the basis of distributional analysis as adult linguists do.

2. A Hypothesis

In short I propose that a Popperian theory of the ontology of language and Piagetian theory of acquisition offer a framework for the resolution to the paradox taken up in the introduction. The hypothesis is as follows: Non-concatenative morphology emerges and perseveres in languages because it is actually easier for the immature language faculty to process. Sensitivity to the fine points of human speech sounds and memory emerge earlier than the analytical and synthetic abilities necessary to process syntax and other combinatorial processes. Words which adults are able to analyze into components are heard and learned by young children as single items. But small phonetic differences among words are carefully noted by very young first language learners, and when such phonetic differences correlate in a systematic way with small differences in meaning (the same referent in singular and plural aspect, for example: man/men), this may facilitate the learning of such items, that is, their storage in, and retrieval from memory.

A theory of the language faculty, then, should recognize two distinct types of cognitive abilities which govern the organization of morphology. The first involves the recognition of minimal and systematic phonetic differences and similarities among language elements (distinctive features). This ability underlies the creation and processing of phonological inventories and certain types of paradigmatic morphological subsystems, including most non-concatenative morphology. The second involves the recognition that the sequence in which two or more elements occur can signify a relationship between them, and that the whole complex may indicate more than its parts. This ability underlies the creation and processing of phrases, sentences, and other syntactic structures and certain types of syntagmatic morphological subsystems, including compounding and much concatenative morphology. By hypothesis the first of these cognitive abilities develops earlier than the second as an individual matures. Thus those features of a language which require the first type of cognitive ability are learned sooner than the second and persist into adulthood.

This explains why concatenative structures may be reanalyzed as non-concatenative by very young children, while the opposite will not occur. Older children and adults, having both kinds of cognitive faculties, may simply replace non-concatenative by concatenative structures by analogy. (They may also analogically extend non-concatenative structures at the expense of concatenative ones, but actually seem to do so more rarely.) But since each generation of younger learners recreates non-concatenative paradigmatic contrasts, it takes time for analogic concatenative forms to spread through a speech community, and hence many non-concatenative patterns may persist for centuries or millennia. The exception is pidgins—where non-concatenative morphological patterns are, for the most part, simply not transmitted—as is expected if it is primarily very young children who are responsible for originating these patterns.

Thus the hypothesis explains:

1. why acquisition proceeds as it does (with production of single words proceeding the production of longer utterances)
2. why the change from concatenative to non-concatenative morphology is unidirectional
3. why non-concatenative (sub)systems share many properties cross-linguistically

4. why pidgins and creoles are so different in their morphological profiles from naturally transmitted in languages, and
5. (possibly) something about how language evolved.
Let us look at each of these areas in turn.

3.The stages of acquisition

The observation that children's linguistic abilities develop through a series of discrete stages dates from the earliest systematic research on child language acquisition at the beginning of the last century (Stern & Stern 1907, Nice 1917, Piaget 1929), and this observation has been repeatedly confirmed by studies of the acquisition of various languages over more than a century (Ingram 1989: 32-58). The hypothesis that the difference in observable linguistic behaviors reflects differences in cognitive ability, that is differences in cognitive, ultimately neurological, development in the growing infant is associated with the developmental psychologist Jean Piaget.

There is some difference among researchers as to the number of stages and how they are defined. But according to Ingram (1989), the following points appear to be uncontroversial. During the first year of life infants are unable to produce words, but they show an ability to distinguish the sounds of human speech from other types of sounds. (Jusczyk (2003: 63) in fact argues that infants' capacity for discriminating speech sounds is present from birth.) Babbling during this period reflects the infant's attempts to practice the use of the articulatory mechanism. The second stage of acquisition, roughly to the middle of the second year, is characterized as the period of single word utterances. Ingram (1989:41) proposes that this period correlates with the development of the ability to think symbolically, the principle of the linguistic sign. This period is followed by the period of the first word combinations. Most of the controversy then centers on how many stages there are between this stage and the final stage when a child has completely mastered adult syntax, and how much syntactic knowledge may or may not underly the child's production of multi-word utterances that do not conform to the rules of adult syntax. For the purpose of the present issue, however, it is the difference between the first two stages and the later ones that is significant. With regard to the acquisition of morphology in particular, it has been shown that there is an S-curve pattern, such that English irregular past tense or plural forms are initially learned without difficulty, are then replaced by analogically derived –s and –d forms, before the "standard" adult system is eventually mastered (Bowerman 1982, Brown 1973). Ingram (1989:65) observes:

"…. children do not produce overgeneralizations like 'foots' at the onset of plural acquisition, but instead, either omit the plural or else use it correctly. To account for this, we need another principle which elsewhere (Ingram 1985a) I have called the lexical principle: learn individual paradigmatic alternations as separate lexical items. This principle claims that the child first acquires paradigmatic variants like 'cat, cats', 'dog, dogs' as separate words, and only later realizes that there is a separable plural morpheme '-s'. The lexical principle predicts that the child will initially get 'foot, feet' correctly, and only later change the latter to 'foots'. (This is in fact, as we will see, true.)"

In short acquisition research consistently shows that the child's awareness of the phonological distinctions of the target language precedes acquisition of vocabulary. And the acquisition, or at least the production, of single words precedes any effort at combing words. The hypothesis is that while an adult will be able to analyze a form like *fot-i* (*ˌføti*) into constituents, abstracting out the phonetic effect of the suffix on the stem vowel, a child whose cognitive abilities do not yet allow it to process combinations of linguistic elements will hear both *foti*(*ˌføti*) and *fot* as unanalyzable lexical items, minimally differing from each other in form and meaning. An understanding of the principle of minimal contrast, necessary to the acquisition of phonology, precedes understanding of the principle of combination. Young learners first exploit the principle of contrast in the acquisition of lexicon.

4.What Happens in Diachronic Change?

The historical development of the Germanic "umlaut plurals" of the foot-feet type is probably the best-studied case of non-concatenative morphology emerging from concatenative. The processes involved in the development of this phenomenon can be classified under four headings: 1.phonetic effect, 2. phonemicization, 3. morphologization, 4. lexicalization and loss. These occur more or less in chronological sequence, but there are differences in how and to what extent each is played out in the different Germanic languages:

i. Phonetic effect rooted in articulation factors: Speakers unconsciously front a stem vowel in anticipation of a front vowel (-i) suffix (regressive long-distance assimilation). To begin with this is a subphonemic, i.e. phonetic effect. We infer this because umlaut emerges in all the living Germanic languages, indicating that it must have been present in some form in proto-Germanic. Yet the earliest written records of Germanic languages, composed after the individual branches had begun to diverge, do not record umlaut, indicating that the originators of the orthographic conventions did not perceive the vowels in *fot* and *foti* (presumably *[føti]) as phonemically distinct. (Lass 1997: 299: "Neither Proto-Germanic nor Northwest Germanic had /y, ø/; but these emerged in pre-historic old English as the result of i-umlaut of /u,o/…).

ii. Phonemicization of the vowel contrast. Speakers reanalyze the vowels in *fot/ føti* as phonemically distinct. We infer that this must have happened because in many Germanic languages, the front stem vowel is maintained in those parts of the paradigm where a front vowel suffix was originally present, even though this suffix, and hence the conditioning environment for the change, has in some parts of the paradigm been lost or replaced. This is the case for example in Modern Icelandic and Old English (adapted from Lass 1997: 342-44):

OE

	sg.	pl.
nom.	fo:t	fe:t
gen.	fo:t-es	fo:t-a
dat.	fe:t	fo:t-um
acc.	fo:t	fe:t

Modern Icelandic

	sg.	pl.
nom.	fotur	foetur
gen.	fotar	fota
dat.	foeti	fotum
acc.	fot	foetur

In these cases clearly we can not say that umlaut is a mechanical effect, nor can it be treated as the learning of a morpho-phonological rule, because there is no consistent environment which would trigger such a rule. The front rounded vowel in the plural of "foot" is a distinct phoneme in Icelandic, as it is in modern German. In Old English the front rounded vowels have merged with their unrounded front counterparts.

iii. Morphologization:

a. Vowel alternation becomes the sole marker of morphological contrast. This is also clear in the paradigms above. Thus in the Old English example above vowel alternation alone marks the number contrast in the nominative/accusative and the dative vs. nom./acc. contrast in the singular.

b. Vowel alternation is maintained in part of the paradigm (plural), but lost in others (case), thus becoming a more transparent marker of a grammatical category, namely plural. This is what obtains in modern German where umlaut no longer marks dative singular nouns-- although dative case as such still exists-- but is frequent as a plural marker. (But umlaut is nonetheless a redundant marker of number in German, since it always occurs in conjunction with suffixes.) In English where case marking is lost in nouns, umlaut survives only as a marker of number. (And is the only marker of number in the handful of nouns which retain it.)

c. Vowel alternation is extended by analogy as a plural marker to some words which did not originally have it. Trask 1996: 113: "… the old High German noun boum 'tree' had a plural bouma, with no umlaut and this should have come into the modern language as Baum, *Baume. Instead German has Baum Bäume." In English this kind of analogical extension is possible at least in a jocular sense: *moose/?meese* on the analogy of *goose/geese*.

iv. Lexicalization and Loss

Stem vowel alternation is largely replaced by segmental endings, as in English, where only a small closed class set of words retains it. These plurals might even be described as lexical or suppletive in English. In Dutch and the mainland Scandinavian languages these plurals have been replaced by suffixed forms.

Thus Germanic umlaut is initially a conditioned sound change. In classical Neogrammarian theory it is usual to regard sound change as an automatic, mechanical process that applies blindly and for this reason is capable of reeking havoc on other systems especially morphology. The ability of sound change to reek havoc on other systems is undeniable. But sound change (true phonemic change, as opposed to performance-based synchronic phonetic variation) is change in grammar—change in the way in which linguistic information is organized in the mind. In order to learn syntax, children have to learn to identify words. In order to identify words speakers must learn to identify a given word as somehow the same even though it may be pronounced differently in different syntactic contexts (depending on things like sentence intonation, stress, whether a preceding or following word occurs, how that word is stressed, whether it ends or begins with a vowel or consonant, etc.). Learners learn to ignore or "abstract away from" these minor variations in pronunciation. Germanic umlaut begins as a purely phonetic, sub-phonemic variation of this kind. Some speakers unconsciously front the quality of the vowel in a stem in anticipation of a high front vowel in a suffix. To the extent that later speakers do not abstract away from the slight change in stem vowel in the *fot-i* case (and similar cases elsewhere), this indicates that they are not processing this as a syntax-like sequence of the already known word *fot* plus a second word-like thing –i. Rather it indicates that they are learning *fot* and *foti* (~*føti*) as separate lexical items with different forms and different meanings.

Moreover once such alternations become established, they can be extended by analogy, as the example of German *Baum*, pl. *Baüme* (**Baume* expected) shows. Trask (1996:112) treats this as an example of Kuryłowicz's first law of analogy: "A complex marking replaces a simple marking." But it can also be taken as a counterexample to Kuryłowicz's second law of analogy: "a derived form is reshaped to make it more transparent and especially more similar to the simple forms from which it is derived." (*Baüme* is less transparent and less similar to *Baum* than **Baume* is.) It has often been observed that many of Kuryłowicz's "laws" are mutually contradictory. But Kuryłowicz, I think, is not be faulted for this. The first and second law simply reflect two countervailing trends in morphological change, both attested in the data. The questions this raises are: 1. Why should both trends exist? and 2. Are there distinct linguistic situations or distinct areas of linguistic structure where one or the other is more likely to prevail?

My answer to the first question is that the two contradictory trends reflect the difference between paradigmatic vs. syntagmatic principles in the organization of morphological knowledge. As regards the second question, it is clear that changes of the type covered by Kuryłowicz's first law will result in greater distinctiveness between members of a paradigm, while changes of the second type will make it easier to analyze words into syntactic atoms. By hypothesis, changes of the first type are typical of young language learners and are more likely to be initiated and accepted by them. Changes of the second type can only be initiated by cognitively more mature speakers.

As far as the area of linguistic structure where each type is likely to occur, let us start by observing that umlaut has been lost as a marker of dative (a change of the K2 type) in German, but extended as a marker of plural (K1). These two changes together could be taken as an example of Kuryłowicz's fifth law of analogy: "in order to re-establish a distinction of central significance, the language gives up a distinction of

more marginal significance" (Trask 1996:113), although what is lost or extended is not the distinction itself but only a partially redundant way of marking it. And it is not so much a matter of maintaining a central distinction (however that is defined) as maintaining a distinction at the derivational (semantic) end as opposed to the inflectional (syntactic) end of the derivation-inflection continuum. Number is externally referential: *dog* and *dogs* or *foot* and *feet* refer to different objects in the real world. In order to grasp such a difference a child need merely be able to understand the principle of the linguistic sign. Case is only internally referential. In order to understand the difference between dative and nominative case, a child needs to understand the syntactic roles that nouns can have in larger structures such as phrases and sentences. By hypothesis the cognitive ability to recognize a sign matures before the ability to combine signs into larger expressive units. By the time one is prepared to learn the syntactic function 'dative' one is also prepared to parse a word into syntagms, hence non-concatenative marking of case is less likely to arise, persevere, or be extended by analogy, than non-concatenative marking of number, which requires only that the mind be prepared to recognize that *dog* and *dogs* or *foot* and feet refer to different things.

5. Universal properties of non-concatenative and otherwise "idiosyncratic" morphology

On the theory that only syntactic principles are universal (by virtue of being innate), deviations from combinatorial principles in morphology must be explained as language-particular accretions on innate universal structure. The major problem with this, as I have argued up to now, is that it offers no way to explain how such accretions could possibly have occurred. But a second problem with the strict combinatorial approach is that it offers no way to account for similarities across languages in the forms which non-combinatorial morphology can take.

The approach outlined here, by contrast, encourages us to think that there really *should* be universal cross-linguistic properties of non-concatenative morphology, and these should be visible even in languages where such systems are small and un-productive. By hypothesis these universal properties are ultimately rooted in genetically determined cognitive faculties which come into play in the course of acquisition. Small, non-productive systems are not necessarily any less systematic or logical than large, productive ones. It may simply be that they are organized according to cognitive principles which are no longer central to adult competence.

In the absence of a comprehensive typological study any discussion of possible universals will necessarily be somewhat impressionistic. But let me begin by noting four widely found properties of natural language morphology which are not predicted by and cannot be economically explained by combinatorial principles, but which make perfect sense in the framework outlined here, which emphasizes the role of contrastive/ paradigmatic principles in the organization of linguistic knowledge, particularly in the early stages of acquisition. The first two of these refer to non-concatenative systems specifically, the other two to morphological systems generally.

i. Cross-linguistically non-concatenative morphological processes like ablaut or reduplication tend to be found in certain categories more than others. Bybee (1985:36-37) observes that the most likely categories for stem change in inflectional verbal morphology are aspect (found in at least 10 of the 50 languages in her sample), tense, and mood (found in at least three languages each) and that the least likely categories are person and number. In Classical Arabic stem change is involved in the marking of tense (vowel alternation: *kat<u>a</u>ba/yakt<u>u</u>b* "he wrote/he writes"), voice (vowel alternation: *k<u>a</u>t<u>a</u>ba/k<u>u</u>t<u>i</u>ba* "he wrote/it was written"), and valence (alternation in quantity: *k<u>a</u>taba/ kat<u>t</u>aba/ k<u>aa</u>taba* "he wrote/he caused s.o. to write/he wrote to s.o."), but not in the marking of person or number. In Germanic it is found in the marking of tense (*ride/rode*), deriving from Indo-European ablaut, and in valence (*sit/set, lie/lay*),

deriving from umlaut induced by a causative suffix which is later lost. In Japanese it is found only in the marking of valence (*hajimaru/hajimeru* "start (intr.)/start (tr.)"

I suspect that a similar pattern holds for nouns, and that non-concatenative morphology is more likely to be found in categories like number, and possibly gender, rather than, for example, case. I am not aware of any systematic study of this question. But in Afroasiatic languages (which I have surveyed in this regard, Ratcliffe 1992) internal stem change in the plural is widespread, found in dozens of languages in the Chadic, Cushitic, Berber, and Semitic subgroups (Greenberg 1955, Newman 1990, Zaborski 1986, Ratcliffe 1992). But the only example of internal stem change marking case in these languages that I have come across is in Somali, where for some nouns the distinction between absolutive, nominative, and genitive is marked solely by tonal difference (Mous ms., Saeed 1999). Stem change marking gender is slightly less infrequent, found in a few Cushitic languages, where tone is again used (Somali ínan "boy," inán "girl," Mous ms.) and very rarely and sporadically in Semitic (Classical Arabic ʕariis "groom"/ ʕaruus "bride").

What unites the categories where non-concatenative morphology appears to be most frequent is that they fall somewhere in the middle of the derivation/inflection, or semantic/syntactic continuum. Person marking on the verb is a major constituent of the verb phrase, taking the place of a subject NP. Case marking only makes sense if one understands the syntactic relationships in a sentence. These categories are thus closely connected with syntactic structure. Categories like nominal number, and verbal tense and aspect, on the other hand have external reference. Yet unlike, say, the members of a compound like *black-berry*, the inflections indicating verbal tense, aspect, nominal number, and so forth do not by themselves refer to an object or action. Rather they refer to a particular way of conceptualizing an object or action. The word /dogz/ refers to two or more individuals of the class "dog," but we can not decompose this into two signs with direct semantic reference to the objective world. The first part /dog/ clearly is a minimal sign which means "one dog." But then /z/ would have to mean "at least one other and possibly all the other dogs," which is obviously not the meaning of /z/ in /frogz/ or countless other cases. In other words /z/ does not itself directly refer to anything in the external word. Rather singular and plural refer to different ways of conceptualizing a particular referent: as an individual or en masse. The same is true for verbal categories like aspect, which distinguishes between an action occurring once or instantaneously and the same action occurring frequently or over a period of time. In short, it seems that the morphological categories which are most likely to be expressed in terms of non-concatenative oppositions are those which are least interpretable as atoms of syntactic structure or sequences of discrete signs.

ii. The phonological structures which are exploited in non-concatenative alternations seem to be drawn from the same set cross-linguistically. These include quantitative alternation with no change in quality (vowel lengthening, gemination, and reduplication) and qualitative alternation with no change of quantity (alternation of a vowel segment or of a single vowel or consonant feature, and alternation of stress or tone). McCarthy & Prince (1998) propose that alternations of the first type are based on universal prosodic categories: mora, syllable, foot, and prosodic word. Thus alternations based on vowel length or gemination, like the Arabic *kattaba*, *kaataba* opposed to *kataba*, cited above, can be described in terms of a difference of a single mora. Most examples of partial reduplication involve the addition of an empty heavy or light syllable. Apophonoy (or ablaut) is often based on alternations in a single distinctive feature (front/back in the case of Germanic umlaut plurals and Indo-European ablaut, high/low—/u/, /i/ contra /a/-- in the case of Arabic and Semitic stem-vowel alternations indicating tense or aspect). Segeral (1998:29) proposes that universally "only a very small subset of logically possible combinations of two vowels is predicted to be able to contract an apopohonic relation." I am skeptical of the details of Segeral's proposal because it relies on a non-optimal analysis of the alternations in Arabic, but the central idea that there could be cross-language and cross-family universals in apophony is promising.

These two features of non-concatenative systems can be analyzed very elegantly in the schema proposed by Ford & Singh (2003:19)[2], wherein any morphological relationship is described by a "Word Formation Strategy" having the following form:

[X]α↔[X']β where

(i) X and X' are words;
(ii) α and β are morphological categories;
(iii) ↔ indicates an equivalence relation (a bi-directional implication);
(iv) X' is a semantic function of X;
(v) ' indicates a formal difference between the two elements of the relation of the morphological operation;
(vi) ' can be null if α ≠ β

This is intended for and can be used to capture any morphological relationship. But it seems ideal for describing certain kinds of non-concatenative relationships, particularly those where a minimum phonological difference correlates with an abstract conceptual contrast like number or aspect. For example the morphological relationship between *goose* /gu:s/ and *geese* /gi:s/ could be represented as follows:

[Cu:C]sg. ↔ [Ci:C]pl.

The relationship between /dog/ and /dogz/ can also be represented in this framework.

[X]sg. ↔ [Xz]pl.

But the first case allows a further reduction into distinctive features:

[Cv:C]-pl. ↔ [Cv:C]+pl.
-front +front

Since the consonants and syllable structure are constant between singular and plural, this can be even further reduced to:

[X-front]-pl. ↔ [X+front]+pl

The point here is that the Ford/Singh schema represents one kind of ideal, which comes closest to being actualized in some kinds of non-concatenative morphology. Oppositions like *goose/geese* represent a highly economical and iconic way to relate form and function. Grammatical categories like number and aspect which are based on a basic, usually binary, conceptual contrast are easily characterized in terms of features (technically "distinguished" rather than distinctive features). It is therefore most "natural" (transparent and economical) to express these categories by minimal phonological differences which can also be characterized in terms of (distinctive) features such as difference in the quality or quantity of a vowel in otherwise identical stems. Another example of this kind of iconicity in non-concatenative morphology is provided by oppositions like Arabic *kataba* ("write," two arguments) vs. *kattaba* ("cause to write," three arguments), *kaataba* ("write something to someone," three arguments), where a minimal contrast in phonemic quantity correlates with a minimal contrast in valence (Ratcliffe 2005): The addition of a mora equals the addition of an argument, and the direction of spread correlates with the grammatical role of the new argument. (Leftward spread, gemination, means new argument in subject role. Rightward spread, vowel lengthening means new argument in object role.)

[X]val ↔ [X+1μ]val+1arg

Thus minimally contrastive non-concatenative morphology is one ideal toward which morphological systems tend to evolve. Yet this ideal is never reached, for two reasons. First such morphology tends to have inherent structural limitations which hinder or prohibit its productive extension. For example, a singular-plural opposition expressed through a contrast back-front on the unique vowel of a CvC word is transparent and easily extendable by analogy to other CvC singulars with back vowel (hence *moose/?meese* is at least imaginable). But what are speakers to do when confronted with a singular stem which has a non-back vowel or a word with more than one vowel? Concatenation of an affix may be a less economical mode

[2] Also earlier in Ford, Singh & Martohardjono (1997).

of expression, but in general does not encounter productivity restrictions of this type. (For this reason concatenative modes of expression are often available as a default even in highly non-concatenative systems—such as the Arabic noun plural (Ratcliffe 1998). The second reason is that as speakers mature and they become able to control the less economical but more powerful (productive) combinatorial morphological mechanism, there is no longer any need to try to extend the non-concatenative contrast. Thus concatenation of transparent morphemes according to syntactic principles is another ideal toward which morphological systems tend to evolve. The competition between these two contradictory tendencies is the cause of the complex and "messy" character of so many natural language morphologies.

iii. Phonological underspecification/ minimality of affixes.

One of the principal differences between words and (bound) morphemes is that the latter are often phonologically underspecified, with their phonological form partially determined by the base to which they attach. For example, in Turkish, vowels in suffixes are only specified for the feature height, not for backness or roundness. The latter features spread from the last vowel of the stem—a phenomenon known as "vowel harmony": *at* "horse" *atlar* "horses," *ev* "house" *evler* "houses." In English the productive inflectional suffixes indicating past tense (alveolar stop) and noun plural/ 3sg pres. (alveolar sibilant) can be analyzed as segments specified only for place and manner features and unspecified for voice, with the voice feature spreading from the preceding segment (the last stem segment or the epenthetic vowel that intervenes when the stem ends in a consonant at the same point of articulation as the suffix). Underspecified affixes represent a stage intermediary between the ideal of combinatorial morphology, in which morphemes, being word-like things, should be fully specified in phonological form, and the ideal of non-concatenative morphology in which a morphological contrast is expressed through a minimal phonological difference between words.

Patterns like these are often treated as morpho-phonology—the result of a phonological rule that applies only in morphologically defined environments. (Vowel harmony does not apply inside stems in Turkish: /ateʃ/ "fire" /asker/ "soldier." Voicing of sibilants after voiced segments in not a general phonological rule of English: *false* /fals/, *farce* /fars/ vs. *falls* /falz/, *bars* /barz/.) Morpho-phonology is an inherently awkward problem for combinatorial morphology. There is no problem if generally applicable phonological rules disrupt the underlying phonological integrity of morphemes, but why should a language have or preserve rules whose sole purpose is to distort the underlying phonological integrity of certain classes of morphemes? Is it possible that all morpho-phonology can be analyzed as phonological underspecification—and hence as a step in the process toward paradigmacity? I am not yet ready to venture such a proposal, given the wide variety of phenomena that have been included under the umbrella of morpho-phonology. But at the very least morpho-phonology represents a trend toward lexicalization as opposed to syntagmization in morphology.

Optimality Theory attempts to explain phonological alternations essentially as the result of competition between ease of articulation and maintaining the integrity of the signal. Assimilation, such as is seen in the Turkish and English cases above, can be interpreted as due to ease of articulation. That it is blocked in the case of stems and applies in the case of inflectional suffixes, shows that the integrity of the stem is more highly ranked than the integrity of the inflectional suffix in these languages. This is explained under the hypothesis offered here as follows: the paradigmatic contrast between words (including that between singular and plural) is significant for learners and speakers of all ages, but can be maintained with a minimum of phonological marking. The integrity of affixes marking plural (and certain other classes) as independent syntagmatic units is not significant for very young learners and hence is relatively less important.

iv. Syncretism. It is very often the case that languages use phonemically identical affixes to mark different categories, even opposite categories (polarity). One obvious example is the English /-z,-s/ as plural for nouns but third person singular for present tense verbs. In Arabic /t/ occurs in the verbal conjugation as an indicator of second person (as opposed to first) but also as an indicator of feminine (as opposed to masculine) in the third person. In verbal derivation it

indicates reduction of valence (Ratcliffe 2005). In the nominal morphology /t/ indicates feminine (as opposed to masculine), and also singulative (as opposed to collective), but occasionally (in conjunction with stem change) also plural (ʔax-"brother" ʔixwat- brothers"). Affixes like these function less like words, which should have direct external reference and in which the one form/one function principle is the norm, and more like distinctive features which only serve to mark two or more items in a system as different.

It is notable too that syncretism does not result from any shortage of the segments available to a language. Languages tend to use only a subset of their segment inventory in affixes, but to use them nonetheless for multiple functions. In English inflectional morphology only the alveolar stops and sibilants are used. In Semitic, of approximately 30 reconstructable consonants only the following seven are used in the affixal morphology of the Classical Semitic languages (Akkadian, Aramaic, Hebrew, Classical Arabic, and Ge'ez): ʔ,h,m,n,t,ʃ,y (Fox 2003:11, Anttilla 1989:196). The more "marked" segments-- pharyngeal fricatives, glottalized/ pharyngealized obstruents, and fricative laterals are not used as affixes. This is paralleled in phonology where not all phonetic features present in a language are used to make phonemic distinctions (e.g. aspiration in English).

Connected with these latter two points is the diachronic trend observed by Hopper & Traugott (1993:145): "In the process of grammatical attrition and selection that accompanies morphologization, we can identify two tendencies:

(a) A quantitative ('syntagmatic') reduction: forms become shorter as the phonemes that comprise them erode.

(b) A qualitative ('paradigmatic') reduction: the remaining phonological segments in the form are drawn from a progressively shrinking set. This smaller set of phonemes tends to reflect the universal set of unmarked segments. They tend especially to be apical (tongue tip) consonants such as [n],[t],and [s], the glottal consonants [ʔ] and [h], and common vowels such as [a],[u],[i], and [ə]."

The logical endpoint of quantitative reduction is underspecification—string of segments >> single segment >> single segment not specified in some features >> single feature. The logical endpoint of qualitative reduction is syncretism—fewer segments means the ones that remain must increasingly be used for many functions. The long term trend described here can be explained in terms of reanalysis and reshaping of morphological systems in terms of minimally contrastive paradigmatic alternations.

5. Pidgins and creoles

A Pidgin is defined as "a marginal language which arises to fulfill certain restricted communicative needs among people who have no common language" (Romaine 1988: 24, citing Todd 1974). Creoles are traditionally defined as pidgins which have become native languages, though specialists in pidgin and creole studies seem to moving toward the view that there is no sharp categorical difference between them (Lefebvre, White & Jourdan 2006). Creoles and pidgins typically have distinctive structural characteristics which distinguish them from normally transmitted languages. The most salient of these is the severe reduction of the morphological resources of the lexifier language. Non-concatenative morphology is virtually never transferred from lexifier to pidgin and even ordinary concatenative marking of inflectional categories is severely reduced. McWhorter (1988) identifies the following features as characteristic of the creole type: extreme reduction of inflectional morphology, absence of allomorphy, transparency of derivational morphology, and absence of tone as a marker of lexical contrasts or morpho-syntactic categories such as tense. All of these relate specifically to the reduction of morphology, though only one specifically relates to the absence of non-concatenative morphology (tone as tense marker). It has also been observed that the vowel systems of pidgins and creoles are often simpler than that of the lexifier, and the lexicon is generally much smaller. Pidgins and creoles typically make greater use of syntactic structures to make distinctions which are made in either the lexicon or the morphology of the lexifier. Thus Tok Pisin reverts to the lexical compounds *gras bilong hed*, *gras bilong pisin*, to express

meanings which in English are carried by distinct lexical items "hair" and "feather," respectively (Romaine 1988:35).

Since pidgins/creoles are typified both by how they are transmitted, and their structural features, it would seem reasonable that there is some connection between the two. A well-known attempt to establish such a connection is Bickerton's bioprogram hypothesis (1981, 1990). Bickerton proposes that creoles have the features that they do because they are created de novo by children using only innate resources with limited help from the "degenerate" input provided by the pidgin. This hypothesis, it seems to me, locates the source of pidgin/creole distinctiveness in the wrong place. Creoles are distinct from normally transmitted languages primarily because they share the structural properties of the pidgin from which they are derived. There is an abrupt transition from the lexifier to the pidgin. The transition from the pidgin to the creole is more subtle. Scholars of creole genesis have cast doubt on whether a clear distinction between pidgin and creole phase can be discerned at all in many cases (McWhorter 1998, Lefebvre 1998, Smith 2006). Smith (2006), for example, argues that Suriname creole was formed in its essential features in a very rapid period long before substantial numbers of children could have learned it as a first language. It also seems to me that Bickerton's theory overlooks what is clearly the most robust and uncontroversially typical feature of pidgins and creoles—namely morphological impoverishment.

My proposal is this: Creoles have the features that they do because they are derived from pidgins. Pidgins have the features that they do because they are created by adults. The structure of pidgins and creoles does not reflect some pure form of the initial state of language competence, rather it reflects the mature, adult state of competence. For an adult it is easiest to memorize a small set of symbols and string them together in complicated ways to expand the expressive repertoire. For a child it is easier to memorize many individual items, especially where such items differ in systematic ways in both form and meaning. Pidgins and creoles are not primitive, or childlike systems of communication. They rely upon cognitive processing mechanisms which are both ontogenetically and phyolgenetically advanced—that is, found only in human beings who have matured past infancy. They only appear simple to adult linguists because we too have developed the cognitive skills necessary to process them. Let me refine this position a bit in order to avoid unnecessary controversy. It is quite possible that the common structural features of pidgins and creoles reflect universal human cognitive abilities which are biologically determined. I am not arguing for or against this position here. I am arguing that pidgins and creoles provide no convincing evidence that these cognitive abilities are present at birth. My hypothesis is also not incompatible with Bickerton's proposal that certain syntacto-semantic distinctions (e.g. punctual/non-punctual) encoded in creoles reflect innate cognitive distinctions present at birth. (As Romaine 1988:260 points out, Bickerton's position differs interestingly from Chomsky's in that it emphasizes the innateness of semantic distinctions rather than formal structures.) My position is that the ability to encode such distinctions *syntactically* is not present at birth. A nice example is aspect. The marking of aspect non-concatenatively is, arguably, the most economical and iconic way to do so, as I have suggested above. Aspect is the inflectional verbal category most likely to be marked by stem change in normally transmitted languages (Bybee 1985). Aspectual distinctions are also acquired early in first language acquisition (Clark 1998:382). In pidgins and creoles aspect is typically marked exclusively syntactically—through a separable particle, usually derived from an auxiliary verb or adverb of the lexifier. This distinction nicely illustrates the hypothesized difference between the adult language faculty, in which syntax is the principal encoding device, and the early stage of the language faculty in which paradigmatic modes of encoding mature first and are gradually superseded.

6. Ontogeny and phylogeny

I speculate that non-concatenative morphology, specifically minimal contrasts between signs based on features such as tone, pitch, duration, loudness (stress), or the formant frequencies of vowels, represents a residue of pre-syntax phase of hominid communication surviving in human language. Let me be clear: it is not the particular contrasts themselves as found in particular languages (e.g. *foot/feet*) which I regard as evolutionarily primitive. Where records exist we can observe things like umlaut or tonogenesis emerging in relatively shallow historical (as opposed

to evolutionary) time. Rather it is the cognitive and neurological faculties which allow for processing of non-concatenative morphology that I propose to be evolutionarily primitive and prior to the faculties necessary for processing syntax. Such morphology is constantly emerging in normally transmitted languages, because the newborn infant has or almost immediately develops the ability to learn and process a primitive communication system based on a small repertoire of minimally contrasting signs, but only later develops the cognitive abilities necessary to process adult-like syntax.

This speculation is based on comparison with other animal communication systems and a kind of ontogeny recapitulates phylogeny argument from first language acquisition. In general, animal communication is restricted to individual signals, without anything like syntactic combination of signals. Most animal communication systems have a very small repertoire of signals. These make use of articulatory distinctions which are severely limited by the articulatory apparatus of the animal in question. Nonetheless the acoustic and auditory mechanisms used by other mammals to distinguish the different signals are similar to those used in human speech: "Acoustic analyses of chimpanzee calls show that they make use of phonetic 'features' that play a linguistic role in human speech" (Lieberman 2003:8, 1968) Differences in intensity of signaling (qualitative differences) and repetition (quantitative differences) are exploited even by bees and birds, respectively (Pinker 1994:334). The remote ancestors of human beings must have evolved brains which were prepared to learn a communication system of this kind. At some point a different sort of cognitive faculty, one which allows the processing of syntax, would have evolved. By hypothesis the evolutionarily older type of cognitive faculty is present at birth, the second type develops later.

Carstairs-McCarthy (2005:181) has reached a rather similar conclusion: "non-affixal (non-concatenative or modulatory) morphology preceded affixal (or additive) morphology in language evolution." Carstairs-McCarthy argues that (non-concatenative) morphology first arises from morpho-phonology, which in turn arises from the phonetic effect of contiguous words on each other in a syntactic string. And he argues much as I have above, that the development of morphology out of free words in a diachronic change is not merely a shift from phrase-syntax to word-syntax, but a "shift from a domain of grammar in which the syntagmatic dimension is dominant to one in which the paradigmatic dimension has at least equal importance" (2005:183). I would only add to this that for such a shift to occur there must be a predisposition on the part of some speakers (by hypothesis very young ones) to analyze (originally syntagmatic) linguistic structures paradigmatically. Carstairs-McCarthy also draws attention to the fact that it is inefficient for languages to have two distinct ways to organize grammatical information. The less efficient (or at least less productive) paradigmatic mode must have evolved earlier, because if the more productive syntagmatic mode had already been available there would have been no evolutionary advantage favoring the development of the paradigmatic mode. (The analogy I would draw here is with anaerobic respiration (fermentation) which is still available to eukaryotic cells, even though it has been superseded by aerobic respiration, which is much more efficient.) However, paradigmatic modes of organization are independently needed for organizing phonological inventories on the basis of distinctive features.

Most of the speculation on proto-language (Comrie 1992, Bickerton 1981, 1990) assumes that it started with strings of words with no morphology. But my question is what form would the words have taken? Would they have been systematically related to each other? Would the lexicon have been structured? I would suggest that the answers to these questions are probably positive and that therefore morphology, specifically non-concatenative morphology would have preceded syntax. Although it seems somewhat absurd to speculate in detail about what this system of communication would have been like, for the sake of exposition, I imagine that a simple phonological sequence perhaps a CV syllable like *pa* or *ka* would have been a signal of some kind and that a change in the signal such as lengthening *paa*, *kaa* or raising the tone or pitch *pá* , *ká* would have indicated an intensification of the signal. The next evolutionary step would have been associating the signal to a specific referent, for example an animal of some kind, with the intensified signal then used to indicate many of the animals or a particularly large

version of the animal, or some other minimally semantically different aspect of the same referent

7. Conclusion

This paper has tried to pull together results from several sub-fields of linguistics, in most of which I can claim no special expertise. The solution I propose may not be the right one. But I hope to have shown that the problem is a real one. If languages structured only according to syntactic principles with no morphology are intuitively natural, then why are virtually all naturally transmitted languages, learned by children, "unnatural"? Why are the most "natural" languages in fact artificial languages and pidgins, created by adults? Why do languages tend to become more "unnatural" over time?

What linguists do, have always done, is to try to uncover underlying order in the superficial diversity and disorder of language data. But in different eras different claims have been made about the ontological status of the underlying patterns so discovered—historically older vs. psychologically real. In essence these two types of claims are mutually contradictory. It is reasonable to equate earlier in a developmental sense with earlier in an evolutionary sense. (This is the logic of ontogeny recapitulates phylogeny). But it is, I think fundamentally wrong to equate earlier in a historical sense with either of these two. Indeed the opposite equation is what we should expect: historically late = developmentally early. If children are responsible for recreating language, then their new creations must reflect neurological functions which are developmentally early, and possibly also older in an evolutionary sense. There is no reason to think that children learn complex alternations by internalizing the whole history which has led to them. Rather they are themselves are the creators of this history and they must do it by restructuring these alternations through the use of cognitive faculties which are different from those employed by adults.

When linguists interpret non-concatenative morphology in combinatorial terms they are not observing or describing the language faculty (much less a primitive, initial state of it). Rather they exercising the faculty as it exists in adults. The fact that linguists can reanalyze non-concatenative contrasts in syntagmatic terms (Lieber 1992 or Stonham 1994) or, alternatively analyze transparent head-modifier compounds in terms of static paradigmatic contrasts (Anderson 1992 or Ford, Singh & Marohardjono 1997), does not constitute a demonstration there is only one mode of morphological organization. Rather it shows that the language faculty consists not only of two different modes of morphological organization (paradigmatic and syntagmatic), but also an ability to translate between them. This too is clear from the possibility of translation between languages with different morphological profiles and from patterns of reanalysis in diachronic change.

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