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Why Morphological Metathesis Rules Are Rare:
On the Possibility of Historical Explanation in Linguistics

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As recently as seventy-five years ago, it was widely assumed that every explanation for synchronic linguistic phenomena must always be historical. Today, it is widely assumed that no explanation for synchronic phenomena in language can ever be historical. While certain types of putative diachronic explanations are indeed invalid, the current paper presents evidence that historical explanation in linguistics is not only possible, but sometimes even preferable. Specifically, the best account for why morphological metathesis rules are rare is the diachronic one that the possible sources potentially re-analyzable as such processes are themselves rare; this historical account is free of the unwarranted assumptions inherent in the competing synchronically based claim that morphological metathesis is rare because it is "marked". And a consequence of this conclusion is that morphological theory should allow the straightforward expression of metathesis rules--which means that a number of current approaches to morphology are overly restrictive in this respect, and so inferior to other approaches where morphological metathesis can be expressed straightforwardly.*

The solely diachronic approach to linguistics dominant into the twentieth century was trenchantly expressed by Hermann Paul 1880¹/1886²/1889²[in English]/1920⁵:20-21--here in a revised version of Bynon's 1977:18-19n.2 translation:

"It has been said ... that a historical analysis is not the only possible scientific analysis of a language. This, I must reject. What some consider to constitute a non-historical and yet scientific analysis of language is in fact no more than an incomplete historical one--incomplete ... due partly to the fault of the analyst and partly to the fault of the data. As soon as one goes beyond the mere statement of individual facts and attempts to comprehend the connection [between them--that is to say], to understand the phenomena... [--]one enters the realm of history, although perhaps unconsciously. ... [I]f one compares [morphologically-]related forms and derives them from a common basic form[, for example,]... one is making a historical observation. Thus, unless one is prepared to enter the realm of history, it is quite unjustifiable to claim that related forms are derived from a common source. Or[, again, if] we ... state that there is a phonological alternation between related forms ... [and] wish... to explain this[, we] ... are necessarily led to conclude that it is the after-effect of a sound-change... [--that is to say,] of a historical process. ... And so I cannot conceive how one could, with any hope of success, think about a language without discovering, at least to some extent, how it came to be as it is."

The generative-grammatical reaction to this (type of) view, however, has always been that, on the contrary, one can explain linguistic phenomena by positing synchronic grammars with particular structures in different speakers' heads--grammars whose rules predict, and therefore at least partially explain, numerous facts of language. And the ex-

planation for why these grammars are the way they are is that they result from the interaction of children's innate capacities for language acquisition with the synchronic data heard in their environment. Paul was right, of course, in pointing out that one can always ask why a particular set of data were uttered in the presence of a given language-learning child, but this represents a demand for higher-level explanation of a kind that is problematic and rarely immediately-realizable for any science, even a physical one. Thus, we now believe that at least synchronic explanation is possible, in linguistics. But, since speakers (' grammars) rarely possess knowledge of the history of their languages, many linguists have gone to the opposite extreme from Paul and denied the possibility of any historical explanation in our field. Paul Kiparsky once (1975:204-205) lamented this attitude as follows:

"...[O]ne semi-taboo left over from the days of structuralism... [is that against] historical explanation in linguistics. ...[W]e have inherited somewhat antihistorical prejudices[W]e still continue to consider ... [de Saussure's] distinction between ... two kinds of linguistics ["synchronic and historical"]) as somehow fundamental. Linguistics, understood either as the study of general properties of grammars... or [as] the study of how it is possible to learn a language, becomes a clearly nongenetic science This then suggests[,] in the end[,] that the investigation of linguistic change is a kind of applied linguistics, a nice thing you can do with a theory of language[,] once you've got it, or else it leads you to the view... that historical considerations are a source of 'external evidence...' which we can use for the testing of competing linguistic theories. ...[T]he presently dominant conception of what linguistics is about ... is ... [that] linguistic theory has no responsibility for historical explanation. A historical explanation, as far as the present theory of linguistics goes, is equivalent to no explanation at all. One result of this is that ... the field[,] as now construed[,] invites linguists to put things[,] at all costs[,] into ... synchronic grammar, so that it will be possible to talk about them officially[,] there."

But Kiparsky then went on to argue that historical explanation actually is possible, in linguistics. Here, I would like to support this point by providing a historical explanation for why morphological metathesis rules are rare. The existence *vs.* non-existence of such phenomena is important because most recent and current approaches to morphology either are unable to express these processes or else cannot integrate them with the rest of morphology. By ("pure morphological metathesis", I mean the marking of some morphosyntactic or semantic category on a form (solely) by the metathesis of two or more of its segments. If we think of morphology in general as the marking of various combinations of these categories on the forms bearing them, then metathesis definitely seems parallel to more familiar processes like suffixation. But few morphological frameworks are able to treat metathesis and suffixation uniformly, due to the notion of the morpheme that they employ (cf. Janda 1983 and references there).

Thus, the "traditional morpheme" (formerly assumed by many American structuralists, for example) is a 'unitary, localized linear stretch of

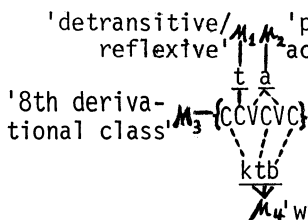
phonological material associated with a discrete portion of the meaning of a linguistic form'. And "traditional morphology" treats word structure essentially as morpheme-ology, the concatenation of traditional morphemes. This approach is thus well equipped to handle concatenations like Turkish müslümanlaştırtamadıklarımızdandınız 'you (familiar plural, or formal singular/plural) were [some/one] of those [people] who(m) we haven't been able to make [someone] cause to become Moslems / a Moslem', which is a string of eleven traditional morphemes. (These are: müslüman 'Moslem', -laş 'become', -tır 'cause', -t 'cause', -ama 'not be able', -dık '[relativ(iz)e(r)]', -lar 'plural', -ımız '1st-person-plural possessor/[agreement]', -dan 'of', -dı 'past', -nız '2nd-person-plural subject'; cf. Janda 1983:82--both this form and its analysis as here and there are due to Mürvet Enç.)

A current extension of this approach redefines the traditional morpheme as a "(sub)lexical item" (= an item listed in the lexicon)--cf. Lieber 1980/1981, 1983a, Williams 1981, Selkirk 1982, Strauss 1982a, 1982b, etc. Such "two-dimensional", "syntactic morphology" treats word-structure as essentially "The Syntax of Words"--word-formation proceeds via the lexical insertion of traditional morphemes into quasi-syntactic trees (generated by context-free phrase-structure-type rules governed by X-bar theory--along with feature-percolation conventions, and perhaps also movement-transformations, empty categories and binding, interaction with Logical Form and the Projection-Principle, etc.). Along these lines, an analysis like that of Selkirk 1982 treats words like English ennoblement in the following way (here, with labeled bracketing, but also, equivalently, in the form of a tree-diagram):

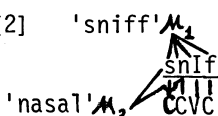
$N^0 [N [-l [V [-l [Aff [en] Adj [-l [noble] N Aff [ment]]]]]]$

A radically different, non-concatenative approach employs the "prosodic morpheme", which is 'either a prosodic template (e.g., a CV-skeleton, a syllable- or foot-structure, etc.) or a unitary, localized linear stretch of phonological material (an "autosegmental tier") associated with one or more positions in a prosodic template [and (presumably) with a discrete portion of the meaning of a linguistic form]'--cf. McCarthy 1979/1982a, 1981, 1982b, 1983a, 1983b, Halle and Vergnaud 1980, Marantz 1982, Yip 1982, Lieber 1983, etc. This "Three-Dimensional", "Prosodic"/"Autosegmental Morphology" treats word structure as the association of prosodic morphemes with one another (i.e., as the linking of one or more non-template prosodic morphemes with a prosodic morpheme template), mostly via the universal linking conventions of Autosegmental Phonology. Such a theory can elegantly account for the apparently discontinuous and incomplete (skeletal) morphemes in examples like the Classical Arabic word ktabab 'was registered', as shown in [1] below (here after McCarthy 1982b:192-193):

[1] 'detransitive/
reflexive'



[2] 'sniff'



And [2] above shows how, taking off from proposals of Fordyce 1981-MS, I have argued (in Janda 1983:86) that the non-linearity of Auto-segmental Morphology allows one to dispense with so-called "phon(a)es-themes", like the initial 'nasal' sn- in English sniff, etc., by positing overlapping morphemes (as also in the case of blends, like Reaganomics, where the segments /ən/ are shared by both Reagan and economics). Yet, though this theory goes three-dimensional, it still treats morphemes as things.

Thus, the most uniformly comprehensive approach to morphology is one based on the (non-root) "rule-morpheme", where the morpheme is not a thing but a process, 'a formal operation performed on a stem (i.e., a lexically-listed, or else derived, linguistic form), generally as a mark of some morphosyntactic or semantic features' (whereas root morphemes, on the other hand, are usually more or less like traditional morphemes or, sometimes, prosodic morphemes)--cf. Sapir 1921, Matthews 1965, 1967, 1970, 1972a, 1972b, 1974, Aronoff 1974/1976, Anderson 1977, 1981b/1982, Dowty 1978, Lieber 1980/1981, Thomas-Flinders (ed.) 1981, Thomas-Flinders 1982, Kiparsky 1982a, 1982b, Bach 1983, Schmerling 1983, etc. This "Rule-Morphology" (as in Anderson's "Extended Word-and-Paradigm" Morphology and, to a lesser extent, Kiparsky's "Lexical Morphology and Phonology"--plus, perhaps to an even greater extent, Schmerling's "Montague Morphophonemics") treats word structure as either the concatenation of stems (= compounding) or else the application of rule-morphemes to stems. Rule-morphemic operations include: addition (e.g., affixation), substitution (e.g., Ablaut), deletion (e.g., truncation), permutation (e.g., metathesis, as will be demonstrated here presently), introduction or alteration of prosodic templates, and combinations of any or even all of these. That is, since morphology is the marking of morphosyntactic and/or semantic categories on linguistic forms, and since such marking can be effected by changes that do not add anything(s) to those forms, but only replace, invert, or subtract, the only uniform way to express all such marking is in terms of processes, rules--including affixation. In a nutshell: affixation can be thought of either in terms of things or in terms of processes, but substitution, deletion, and permutation can be thought of only as processes. In Rule-Morphology, word structure is thus expressed formally, not by a single (level of) representation, but by a derivation that includes: (i) the lexically-listed form of the stem of a word, (ii) the set of rule-morphemes which have applied to such a stem, along with their outputs, especially (iii) the surface form of the word in question. In this way--because it employs the full range of phonological-type operations and represents word structure in terms of an entire derivation--Rule-Morphology is much more like the "phonology of words". (For further discussion, cf., again, Janda 1983.)

Such a theory can handle much more easily than any others examples like the Chickasaw (Muskogean family) non-future negative forms of verbs, which are obtained from positive stems via three simultaneous operations: prefixation of k- (in almost all cases), infixation of a glottal stop before the last consonant, as long as the latter is not part of a cluster, and replacement of the last vowel with -o. This rule can (following the Extended Word-and-Paradigm format of Anderson 1977, 1981b/1982) be formulated roughly as follows: [+ Verb, - Future,

+ Negative] /X₁CV/ \Rightarrow /k₁l₂o/; by it, one can form from losa

'it-is-black' the non-future negative i-klo₂so 'it-is-not-black', for example--via simultaneous addition (prefixation and infixation) and substitution. (For further discussion of this rule, see Kempler and Thomas-Flinders 1981, Kempler 1982, and Janda 1982a:144-145, 1983:91.)

Now, Aronoff 1974/1976, who first followed Matthews in adopting this approach systematically for generative analysis, concluded that the full range of rule-morphemic processes could be expressed only with transformational notation (as just illustrated immediately above):

"We have found two classes of rules [(copying--especially reduplication--and infixing)] which are best viewed as W[ord]-F[ormation] R[ule]s... and which force us to state WFRs in a particular manner... [--]namely[,] as transformations. This... forces us to divide ... [each] rule into two parts, a structural description and a structural change. The first part specifies only the base. The second part contains the base and the result of the operation of the WFR amalgamated into one unit" (p. 70).

However, McCarthy 1979/1982a, 1981 later argued that this notation is so powerful as to be able to do literally anything, and so he even went as far as to propose a restriction ruling out its use completely:

"Morphological[-]Rule Constraint (MRC)... [--]all morphological rules are of the form $A \rightarrow B/X$, where A is a single element or zero[,] and B and X are (possibly null) strings of elements. That is, morphological rules must be context-sensitive rewrite-rules affecting no more than one segment at a time, and no richer type of rule is permitted in ... morphology. ... [A] theory that incorporates the MRC strongly generates a smaller class of grammars than a theory without this constraint. Morphological transformations potentially allow any operation on a segmental string. For example, transformational morphological rules can freely move particular segments an unbounded distance within ... [a] word, copy all and only the vowels in a word, or reverse strings of finite length. If the segmental representation is further enriched ... [with] integral indexing of segments, ... then morphological transformations can perform their arbitrary operations on only the prime[-] or factor-of-twelve[-]numbered segments in ... [a] word with no further enrichment of the formalism. ... These examples, although bizarre, are not facetious. ... [A] morphological theory without the MRC allows all of these types [of rules] and[,] in some cases[,] values them more highly than morphological rules that actually occur in some language. A theory with the MRC is therefore significantly more explanatory [and] ... more constrained than[,] and consequently superior to[,] a theory that does not...["incorporate..." it,] all other things being equal" (McCarthy 1981:405-406, after 1979:356-361/1982a).

And, along similar lines, Marantz 1982 proposed a non-transformational analysis of all reduplication in languages, essentially mainly because:

"...[the "transformational notation for reduplicative processes"] allows morphological rules to be written that never occur[,] cross-linguistically. ... Given [its] formal apparatus ... , one can write many types of morphological rules which are not instantiated in natural languages. For example, although expressible in the transformational

notation..., mirror-image reduplication...[-]rules ... are not found in any language" (Marantz 1982:435).

Finally, Donca Steriade has reported (personal communication) that one of the most important considerations underlying the proposal of principles like the MRC was that of preventing morphological rules which metathesize entire morphemes with one another (i.e., switch their relative positions).

Thus, one principal empirical claim embodied in the MRC and the overall theory of Autosegmental Morphology is that natural languages should never have morphological rules like metathesis, which rewrites more than a single segment (or zero) in a string. The motivating background assumption here--to repeat--is that the transformational notation required for the expression of metathesis opens a Pandora's box of implausible-seeming morphological processes, like inversion of entire strings. Now, in fact, at least one such rule reversing the order of all the segments or syllables in a word actually exists. Stuart Davis has called to my attention Conklin's 1956 discussion of the Tagalog speech-disguise "baliktád"--whose very name includes the meaning 'inside-out, upside-down, inverted, or backward (referring to a homogeneous, inanimate, nonhollow entity)'--where:

"...[among] the eight types of structural rearrangement and affixation used in the formation of baliktád words... [are] complete reversal of the phonemic shape of the word base... [and] complete reversal of the syllabic shape of the original form (e.g. [,] 1-2-3 > 3-2-1)... [, so that] tamá:las < salá:mat 'thanks'..., tidpaka' < kapatid 'sibling' [(= 1-2-3-4-5-6-7 \Rightarrow 7-6-5-4-3-2-1 and 1-2-3 \Rightarrow 3-2-1, respectively)]" (pp. 136-137).

Of course, this speech-disguise operation is not a morphological rule, or even a normal-language phonological one, but it does show that a string inversion process is not so linguistically implausible. Rather, the most solid counterexamples to the MRC and to Autosegmental Morphology as a thereby restricted "thing-morphemic" (rather than rule-morphemic) theory of word formation come from the rules of morphological metathesis which exist in at least the two languages Clallam and Rotuman.

Clallam (or [tʰajam], as its speakers call it), is a Straits language within Coast-Salish(an)--cf. Thompson and Thompson 1969, 1971. It was originally "spoken ... in a number of villages along the north coast of Washington's Olympic Peninsula. ...[Unfortunately, o]nly a handful of elders [(= "seven")] still speak Clallam fluently" (1969: 215). As regards the phonology of the language, it suffices here to mention that its phonological inventory includes /p, t, (k [only in loanwords],) kʷ, q, qʷ, ʔ; p', t', kʷ', q', qʷ'; s, š, xʷ, X, Xʷ; c, č; c', č'; (l;) ɬ; ʎ; m, n, ŋ; w, j, h; i, e, ə, a, u/ (where I have slightly modified Thompson and Thompson's system of transcription). What it is much more important to mention, for the present purpose, is the Clallam morphosyntactic category of the "actual" aspect, which refers to an "action or state [as being] in effect at a particular moment", like the English -ing-form of a verb; it resembles the Slavic imperfective except that, unlike the latter, it is a marked form (cf. 1969:215). Actual/non-actual "... is the most fundamental aspectual distinction [in Clallam]; it pervades the ... language" (1971:274). Now,

roots of the form CVC... mark actual aspect by infixing a glottal stop after V (often along with other changes); thus, the non-actual of 'wipe' is $\text{ʔač}^{\text{v}}-$, while the actual is $\text{ʔa-ʔ-č}^{\text{v}}-$ (although, in order to occur as words, roots must be suffixed with some such element as $-t$ '[with/by some agent in] control [of the situation]', as in, e.g., non-actual $\text{ʔač}^{\text{v}}-t$ and actual $\text{ʔa-ʔ-č}^{\text{v}}-t$).

But roots of the form CCV... (of which there are many) mark actual aspect by metathesis of the second consonant in the root with its first vowel. Thus, examples of non-actual/actual pairs include, respectively: $\text{q}^{\text{v}}\text{q}^{\text{v}}\text{ʔ}^{\text{v}}/\text{q}^{\text{v}}\text{q}^{\text{v}}\text{ʔ}^{\text{v}}$ - 'restrain', $\text{x}^{\text{v}}\text{č}^{\text{v}}\text{ʔ}^{\text{v}}/\text{x}^{\text{v}}\text{č}^{\text{v}}\text{ʔ}^{\text{v}}$ - 'scratch', $\text{p}^{\text{v}}\text{k}^{\text{v}}\text{ʔ}^{\text{v}}/\text{p}^{\text{v}}\text{k}^{\text{v}}\text{ʔ}^{\text{v}}$ - 'smoke', $\text{t}^{\text{v}}\text{c}^{\text{v}}/\text{t}^{\text{v}}\text{c}^{\text{v}}$ - 'shatter', $\text{k}^{\text{v}}\text{w}^{\text{v}}\text{s}^{\text{v}}/\text{k}^{\text{v}}\text{w}^{\text{v}}\text{s}^{\text{v}}$ - 'count', $\text{s}^{\text{v}}\text{t}^{\text{v}}/\text{s}^{\text{v}}\text{t}^{\text{v}}$ - 'walk', $\text{x}^{\text{v}}\text{w}^{\text{v}}\text{k}^{\text{v}}\text{ʔ}^{\text{v}}/\text{x}^{\text{v}}\text{w}^{\text{v}}\text{k}^{\text{v}}\text{ʔ}^{\text{v}}$ - 'drag', $\text{č}^{\text{v}}\text{q}^{\text{v}}\text{w}^{\text{v}}/\text{č}^{\text{v}}\text{q}^{\text{v}}\text{w}^{\text{v}}$ - 'burn', $\text{č}^{\text{v}}\text{k}^{\text{v}}\text{w}^{\text{v}}/\text{č}^{\text{v}}\text{k}^{\text{v}}\text{w}^{\text{v}}$ - 'sting; shoot', $(/\text{č}^{\text{v}}\text{s}^{\text{v}}/\text{>})\text{č}^{\text{v}}\text{s}^{\text{v}}/\text{č}^{\text{v}}\text{s}^{\text{v}}$ - 'throw' ($\text{s} \rightarrow \text{s}^{\text{v}}/\text{č}^{\text{v}}$); Thompson and Thompson 1969:216, 1971:276 list at least fifteen more such examples. The most straightforward formulation of Clallam Actual Aspect Root Internal Metathesis is with the transformational format, as follows: $[+ \text{Verb}, + \text{Actual}]/\text{CCVX}/ \Rightarrow$

1234

Now, it is, of course, always possible to reanalyze permutations like metathesis as combinations of copying-insertion with subsequent deletion under-identity (with the copied segment(s)) of the original(s). But, for Clallam, no morphological or phonological rules can be independently motivated to insert a copy of the first root vowel, shift stress to this copy, and/or delete the original, copied vowel; nor is there any motivation for positing two (matching) vowels in roots underlyingly (but cf. Demers 1974 on the closely-related Lummi). Rather, any such alternative to the elegant single morphological transformation of metathesis given above would be both *ad hoc* and unnecessarily complicated.

As pointed out by Thompson and Thompson 1969:217, though, the most convincing evidence for the basic and unitary status of grammatical metathesis in Clallam comes from another morphological rule similar to the root internal one above. When certain CCV...-roots combine with certain suffixes which attract stress away from a root and thereby cause it to lose its vowel (and so be in the "reduced grade"), actual aspect metathesis is obviously blocked from applying within the root (since the structural description of the above rule requires a vowel there). However, for certain combinations of reduced-grade-root + suffix + suffix, in Clallam, metathesis marking the actual aspect still occurs--namely, between the suffixes. Thus, for example, with the verb $\text{x}^{\text{v}}\text{k}^{\text{v}}\text{w}^{\text{v}}\text{ʔ}^{\text{v}}$ - 'grasp; take hold of' in its reduced grade ($\text{x}^{\text{v}}\text{k}^{\text{v}}\text{w}^{\text{v}}$ -) plus the stress-attracting suffix $-\text{i}$ 'persistent(ly)' and the 'control'-suffix $-t$, there is the non-actual/actual alternation of, respectively, $\text{x}^{\text{v}}\text{k}^{\text{v}}\text{w}^{\text{v}}\text{-i-t}/\text{x}^{\text{v}}\text{k}^{\text{v}}\text{w}^{\text{v}}\text{-t-i}$. This case can be accounted for by the following rule of Clallam Actual Aspect Metathesis of the Suffixes 'Persistent' and 'Control' After Reduced-Grade Roots: $[+ \text{Verb}, + \text{Actual}, + \text{Persistent}, + \text{Control}]/\text{CCit}/ \Rightarrow$ /132/. (It is perhaps worthwhile to note that, while 1 23 root internal metathesis in Clallam could be judged to increase ease of articulation, by moving a vowel into the middle of a sequence of two consonants [even up to two obstruents], metathesis of suffixes can create sequences of up to three obstruents.)

The crucial thing about the Clallam suffixal metathesis rule in the actual aspect is that it falsifies McCarthy's Morphological Rule Constraint (MRC; given above)--and not only because it rewrites more than a

single segment in a string as part of a morphological rule, but also because it rewrites (the order of) a string of two entire morphemes as part of such a rule. And, given the latter situation, it is hardly possible to argue that each Clallam root which can appear in the reduced grade and be followed by the suffixes 'persistent' and 'control' is listed in the lexicon with an extended stem-"allomorph" representing each of the two orders for those suffixes. Instead, the only available option seems to be that of treating suffixal metathesis as a general morphological rule of the language. (Actually, even if one adopted the lexical-listing approach for expressing alternative suffix orderings, it would still be necessary to posit a metathetical redundancy rule, in order to capture the generalization that the relation between the two suffix orders is a regular one.) And, since Clallam clearly has a morphological metathesis rule for its suffixes, the likelihood is just that much greater that the morphological rule for marking the actual aspect within roots is also one of metathesis.

A morphological metathesis rule can similarly be motivated for Rotuman, an Eastern Oceanic language (within Austronesian [within Austro-Tai]) thus completely unrelated to Clallam (cf. Churchward 1940, Biggs 1965, Milner 1971, Pawley 1979, Geraghty 1983, Besnier 1983-MS). Rotuman is spoken by at least 3,400 inhabitants of the isolated volcanic island Rotuma (size: 18 square miles), which is located approximately 250 miles north of Fiji, 300 miles south of Tuvalu, 600 miles west of Samoa, and over 600 miles east of the Solomon Islands. It is also spoken by many Rotumans living in Fiji. The phonological inventory of Rotuman includes /p, t, k, ʔ; f, s; v; ʃ; m, n, ŋ; l; r; h; i, e, a, ɔ, u/ (and the transcriptions that follow are given in terms of these, rather than in any [other] system of orthography or transliteration).

With virtually no exceptions, all members of major lexical classes in Rotuman have two basic allomorphs, generally called (after Churchward 1940) [the forms of] the "complete phase" and the "incomplete phase" (cf. also Hale 1846, Hocart 1919, Churchward 1929, Haudricourt 1957-1958, Biggs 1959, Thompson and Thompson 1969, Ultan 1971, Anttila 1972, Anderson 1974, 1975, Sohn 1980, Janda 1982a, 1983). The complete(-)phase (form) is assumed, on the one hand, by any noun which is unmodified and definite (or "complete") and/or emphatic, and, on the other hand, by any verb which is perfective (or "conclusive"/"actual"/"complete"/"completive") and/or emphatic--this also holds for adjectives used as verbs. The incomplete phase is assumed by any major-lexical-category item in a noun phrase which is not (NP-)final, and by any final item in a noun phrase which is, overall, indefinite (or "incomplete") and non-emphatic; the same holds, *mutatis mutandis*, for non-final *vs.* final items within compound words. The incomplete phase is also assumed by verbs that are imperfective (or "inconclusive"/"contingent"/"incomplete"/"non-completive") and non-emphatic (which corresponds to most [uses of] verbs, in Rotuman)--again, this also holds for adjectives used as verbs. What is most striking about all this, though, is that, for words whose complete phase ends in a ...VCV-sequence where the first vowel is higher than the last one, the incomplete phase is formed by metathesis of the final vowel with the consonant preceding it. Thus, ʔépa 'the-mats' (definite; complete phase) corresponds to ʔéap 'some-mats' (indefinite; incomplete phase)--Rotuman metathesis is ac-

complicated by shortening and (probably also) desyllabification of the penultimate vowel, which also loses its stress to the final one. Additional examples of complete-phase/incomplete-phase pairs in Rotuman include, respectively: aíre/aíer 'true', tíko/tísk 'flesh', ?ípa/?iap 'pigeon', púre/puéer 'to rule; decide', úlo/úli '[a kind of sea-bird]', fúpa/fuáp 'to distribute', fóra/fzár 'to tell'. Such forms motivate the rule of Rotuman Incomplete Phase Metathesis given in [3] below (cf. also Cairns 1976):

[3] [- Complete Phase]
 /X [V] C [V] /
 [- low] [- high]
 [- high]^a [+ low]^b
 2 3
 /132/. Condition: a > b.

The higher-than condition on the first vowel here may seem a strange one for a metathesis rule, especially one that doesn't even affect the first vowel, but the above formulation in fact allows the simplest overall analysis of

Rotuman. And, anyway, morphological rules clearly aren't subject to any requirement of phonetic/phonological naturalness, particularly since phonological rules themselves aren't completely so (cf. Anderson 1980/1981a). Rotuman words whose complete phase forms are anything other than ...V₁CV₂ with V₁ higher than V₂ have incomplete phase forms marked other than by metathesis: ...VV becomes ...V^V (with shortening, probably desyllabification, and stress-shift), and other ...VCV becomes ...VC (with apocope), whereby the remaining vowel is front(ed) (i.e., there is a kind of Umlaut) if the originally final vowel was a front one. Now, some scholars have argued that metathesis takes place even in these other ...VCV-forms (along with subsequent syncope and/or coalescence), but this analysis requires rules of greater complexity than those in the account just sketched. And, in any case, a finding that metathesis takes place in the incomplete-phase forms of all Rotuman words ending in ...VCV would only strengthen the claim that the language in question has an extremely regular morphological metathesis rule. (The alternative--independent--auto-segmental-morphological accounts of Saito 1981-MS and Besnier 1983-MS, neither of which posits a metathesis rule, not only are both more complex than the present analysis, but also arguably involve a morphologically unmotivated placement of Rotuman vowels and consonants on separate autosegmental tiers and/or the separate "projection" of such vowels and consonants in a way that belies the dependence of what happens to incomplete phase vowels on the relative position(s) of neighboring consonants. I hope to demonstrate this soon in Janda 1984-MS (in preparation), where I will also present four arguments justifying the deriving of Rotuman incomplete phase forms from--roughly--their complete phase counterparts.)

For Rotuman, as for Clallam, there is no independent motivation for positing a set of other rules designed to avoid the need for metathesis. In particular, writing a rule to copy final vowels in the position between the final consonant and the penultimate vowel in terminal ...VCV sequences unnecessarily complicates other, independently needed rules of the language. And so Rotuman likewise appears to require some kind of morphological metathesis rule--either one like that given above, or else some autosegmental-morphological equivalent where, af-

language[-]change and [-]acquisition.")

But, in the case of Clallam and Rotuman metathesis, there is absolutely no evidence currently available indicating that aphasic or phasic children and adults experience any difficulty whatsoever in producing or understanding the process in question. In fact, for Rotuman, we have the opposite kind of evidence. For one thing, Rotumans apply their incomplete phase metathesis to English loanwords, so that 'the-sugar' is súka, but 'some-sugar' is suák, while 'watch' was heard and borrowed by Rotumans as the incomplete phase, uáç, of complete-phase úca. And, for another thing, recent psycholinguistic experiments on morphology reported by Boyce, Browman, and Goldstein 1983 have led them to conclude that "non-concatenative morphology [in general] doesn't cost [listeners any "efficiency in word recognition"] if the process[es at issue are]... regular." But Rotuman metathesis is nothing if not extremely regular.

Thus, in the present case, the only real evidence which suggests that morphological metathesis is marked comes from the fact that it is rare--but the only explanation offered for why it is rare is precisely that it is marked. This is the not-so-rare circularity of markedness. At this juncture, it seems instructive to reflect on the reasoning behind the approach just discussed. Some linguists are apparently appalled by the prospect of employing a notation--like the transformational one--which, if unconstrained, could potentially express bizarre (types of) rules of morphology unattested in the world's languages. Indeed, they are so appalled by this that, in order to rule out unattested (types of) phenomena, they are willing to pay the price of having to claim that certain infrequent but attested morphological rules (like metathesis) are essentially grammatically impossible. Other linguists recognize a qualitative difference between there being absolutely no examples of some (type of) phenomenon and there being even just one example of it, so that they are reluctantly willing to predict the existence of many bizarre, unattested things as long as they can thereby express everything that is attested. These two remarkably different attitudes usually lead to quite distinct choices of notational formalisms--and this can be illustrated graphically, in the particular present case of morphological metathesis, as in [5]:

[5]a. Notation A (e.g., Autosegmental Morphology, including the MRC):
 ATTESTED linguistic phenomena | UNATTESTED linguistic phenomena

	Me- tath- esis	
"UNMARKED" (Actual/ Possible)	"MARKED" (Improbable/Impossible)	...

b. Notation B (e.g., Rule-Morphology, including the transformational format, but with no MRC or other constraints):
 ATTESTED linguistic phenomena | UNATTESTED linguistic phenomena

	Me- tath- esis	
(Actual/Possible)	(Possible/Probable)	...

Now, in fact, the second approach discussed and illustrated above ac-

tually can rule out the scarcely imaginable bizarre unattested (types of) phenomena in question, by adopting independent (although perhaps *ad hoc*) constraints to that effect. Hence, in the present case, the choice really comes down to the following: On the one hand, we can accept the MRC, and hence be forced to employ Autosegmental Morphology, rather than Rule-Morphology and its necessarily concomitant transformational format--with the result that morphological metathesis essentially has to be treated as ungrammatical, along with bizarre, unattested (types of) linguistic phenomena. On the other hand, we can reject the MRC and Autosegmental Morphology (as an overall theory of word-formation), adopt Rule-Morphology's transformational notation and thereby account straightforwardly and elegantly for morphological metathesis, but then also be forced to propose some rather *ad hoc* constraints in order to rule out bizarre, unattested (types of) phenomena.¹ What must decide this issue, then, is clearly the answer to the question of whether morphological metathesis really is marked.

The evidence from loanword-applicability and regularity of grammatical metathesis in Rotuman certainly seems to go against markedness at least for that language. And the Tagalog speech-disguise facts discussed earlier incontrovertibly show that humans are capable of performing much more complex operations on strings than mere metathesis. Furthermore, McCarthy and Marantz have readily conceded that copying-transformations are a necessary part of Universal Grammar, which means that such processes must be among the set of human cognitive capacities:

"Adapting the proposals of an earlier version of this article...[= Marantz 1982,] McCarthy (1981[:]...412-413) suggests that reduplicating skeletal affixes should carry a feature [+ reduplication] which 'has the effect of causing automatic copying of all the melodic elements in some morpheme--formally, all the daughters of some *M* in a particular tier.' This copying[-]process may appear to require 'transformational power', but, as McCarthy points out, 'The copying induced by the presence of the feature [+ reduplication] is part of universal grammar, not part of some language-particular reduplication[-]transformation, and[,] consequently[,] it is irrelevant to the whole problem of restrictiveness...' (p. 445n.7).

But even more important is the question of whether there exists an explanation other than markedness for why morphological metathesis rules are rare. This is because, if one or more such alternative explanations can be found, then virtually the only evidence supporting the claim of markedness for that type of process will be undercut. And here is where historical considerations enter in. To claim that morphological metathesis rules are marked because they are rare is equivalent to asking why such processes, if they're not marked, aren't more common. But such a question presupposes that all possible (unmarked) morphological rules--in fact, all possible linguistic phenomena in general--should be widely distributed across the languages of the world. However, this would be true only if people were free to think up languages on their own, independently--in which case the odds would indeed be that all (or at least most) grammatical devices cognitively available to speakers would be selected a fair number of times. But speakers don't make up their languages; they mainly acquire them from others, either by successfully inferring others' elements and rules of

grammar from those others' speech-output or else by unintentionally reanalyzing others' grammatical elements and rules, again on the basis of others' speech-output. Hence, unless there were morphological metathesis rules in the first human language or languages--rules which were then passed down, across time, eventually to speakers of Clallam and Rotuman--we must conclude that such processes can have arisen only by way of reanalysis. And it is indeed unreasonable to assume that all possible (types of) linguistic phenomena were present in the first language(s)--especially all possible (types of) morphological rules.

Thus, we must now ask what things could possibly be (and so have been) reanalyzed as morphological metathesis rules. One likely candidate for such a source would be phonological metathesis rules. However, despite Chomsky and Halle's remark in SPE (1968:361) that "... metathesis is a perfectly common phonological process", several large-scale studies of phonological metathesis have shown it to be primarily a sporadic source of sound change, rather than a normal type of synchronic phonological rule accounting for alternations. Ulтан 1971, for example, concluded that:

"... metathesis is usually recessive in comparison with other processes...[;] it is prone to greater interference from more dominant ones like... reduction, assimilation, dissimilation, and epenthesis or anaptyxis" (pp. 36-37).

And Webb's 1976/1977[?] findings are, in this sense, even more negative:

"... I have examined many cases of metathesis cited in the literature ([in]Webb 1974). Most of the examples were found to be sporadic, with very few or no alternations [supporting them] in the grammar of a particular language. ...[Such c]ases where only a handful of alternations exist ... are best handled suppletively. ...[A]t best[,] synchronic examples of systematic metathesis are very rare. The few cases which most strongly suggest metathesis as the correct generalization are questionable on independent grounds. ...[T]he weight of the evidence strongly argues against metathesis as a phonological process. ... Diachronic examples are apparently more numerous[,] but tend ... to be restructured. Once restructuring has taken place, there are generally few, if any, alternations which remain. This is not generally true of other phonological rules, ... [which] regularly leave evidence of their prior existence ... [in] the form of alternations. ... [In fact, many putative diachronic] metathesis[-]rule[s] ... may ... never ["have"] existed ["as such"] in the synchronic grammar of a particular language" (Webb 1977[?]:87-88; cf. also Passy 1890, Wechssler 1900, Sievers 1876¹/1901⁵, Lehmann 1962¹/1973², Pawley 1982-MS, and many others--but also Grammont 1933¹/1971⁹, Martinet 1955¹/1964², Semiloff-Zelasko 1973, Silva 1973, Sohn 1980, and some others).

Finally, McCarthy himself has claimed that:

"...there exists a quite limited set of possible metathesis[-]rules, which we could characterize as a preliminary theory of natural metathesis. Although linguistic theory allows full transformational formalism in phonological rules, it is nevertheless subject to this sort of substantive constraint. Therefore, only small subset of the formally possible metathesis[-]rules will actually occur, since many possibilities will be excluded on phonetic grounds. ...[H]owever, it is impossi-

ble to place any such constraints on the phonetic [or phonological] naturalness of morphological rules[, because]... it follows directly from l'arbitraire du signe that phonetically [or phonologically] determined considerations of naturalness have no place in morphological rules[--which helps justify the higher-than condition on the first vowel in Rotuman incomplete phase metathesis, for example, as discussed in connection with [3] above--RDJ]. Therefore, any constraint on ... morphology must be an essentially formal one, like the MRC[--although we have, of course, already seen that the MRC is actually overly restrictive, and also although it may actually be possible to elaborate a set of substantive constraints on morphological rules, after all²]"] (McCarthy 1981:406, after 1979:360/1982a).

Furthermore, we cannot assume that all phonological metathesis rules --insofar as these exist--will or even can be morphologized, but only a restricted subset. Therefore, if morphological metathesis rules can arise only due to a restricted phenomenon (= reanalysis) happening to a relatively rare phenomenon (= phonological metathesis), then we can in fact predict that metathesis in morphology should be rare--but without any need to claim concomitantly that such a grammatical process is somehow difficult to learn, use, or comprehend, or that it is otherwise "marked".

Now, actually, there are a few other sources which could be (and so could have been) reanalyzed as morphological metathesis--some, probable, and some, only imaginable. The root-internal metathesis rule in Clallam, for example, could--on the basis of comparative evidence (cf. Thompson and Thompson 1969:217-218 and Demers 1974, plus references there)--be analyzed as having arisen from a situation where a difference in the segmental structure of suffixes had an effect of conditioning differences in stress placement in preceding CVCV...-roots which in turn led to the deletion of different root-vowels in different grammatical categories. (Such a scenario, though, requires the not uncontroversial claim that, at some stage, all bisyllabic roots in a group of Salishan languages had two identical vowels.) Rotuman metathesis, on the other hand, could have resulted from some analogical desire to create consonant final surface forms in the incomplete phase that would parallel the consonant final forms produced by apocope of the vowel in the encliticized indefiniteness marker =ta (added to the equivalent of complete phase forms)--schematically, ...VCV=ta > ...VCV=t. (The encliticization and apocope just mentioned are described in Churchward 1929, 1940, but not the possibility that Rotuman metathesis arose by analogy to the result of these--viz., that incomplete phase forms come to be consonant final in this way, too. Such an idea is pure speculation on my part, though not, I believe, unfounded.) The important thing in both these cases, however, is that such other possible sources potentially reanalyzable as morphological metathesis are likely to be even rarer than phonological metathesis--which again leads us to expect that morphological metathesis rules should be rare, once more without the invocation of any vague and unsupported notions of markedness. (In view of the absolute rarity of phonological metathesis rules reordering adjacent consonants and vowels in a way parallel to Clallam and Rotuman morphological metathesis, it is in fact likely that both those processes arose via reanaly-

sis of one or more non-metathetical rules, whether exclusively phonological or exclusively morphological or of both types.)

Given this finding, we can conclude that there are no compelling reasons to characterize morphological metathesis as marked, even though it is rare. And so an approach to morphology which can express such a (type of) process straightforwardly and in a manner unified with the rest of word-formation--as Rule-Morphology can--must be adjudged superior to any theory that cannot (like Autosegmental Morphology, especially if it includes the/an MRC).

But, more importantly, we can also conclude from the above that historical explanation thus belongs to more than the history of linguistics--that it has a possible role to play even in the synchronic study of language. In fact, returning to the topic of the history of linguistics as it was discussed at the outset of this paper, we can note that the real progress which we can demonstrate to have been made during the last seventy-five years is our realization that historical explanation is not the only kind available in linguistics--that, e.g., the synchronic fact of language acquisition also has a crucial explanatory function--and in a way which makes synchrony and diachrony extremely hard to keep separate. In fact, along these lines, it turns out that Hermann Paul was at least partly right, all along--and was even a forerunner of good generative linguistics--because the passage by him quoted earlier goes on to finish (Paul 1880¹/1886²/1889²[in English]/1920⁵:20-21--here in a revised version of Bynon's 1977:18-19n.2 translation):

"The only aspect ... which might conceivably remain as suitable for non-historical investigation[, in language,] might be general considerations regarding individual usages of language... [--]that is to say, the behavior of the individual speaker relative to general usage, which ... include[s] language-acquisition. ... [H]owever, precisely these considerations are to be intimately connected with the analysis of the historical development of language."

And, since Paul's time, such possibilities for diachronic explanation in linguistics--that is, for combined diachronic and synchronic explanation--have been noted and exploited by mavericks like Kiparsky (as in the already cited Kiparsky 1975) and, even earlier, Joseph Greenberg 1966, 1969, 1978, 1979. Indeed, Greenberg spelled out this whole research program in his 1977 LSA presidential address "Rethinking Linguistics Diachronically" (published as Greenberg 1979). The case of the almost missing morphological metathesis rules suggests that there are undoubtedly a lot more explanations out there just waiting to be found if we, too, will only consider rethinking linguistics diachronically.³

FOOTNOTES

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1 Actually, it is quite likely that many types of bizarre, unattested morphological (and general linguistic) phenomena can be ruled out by independent constraints which are anything but ad hoc. In particular, a cursory survey of the psychological literature on the ability of humans to perform various kinds of operations on strings of symbols (verbal or otherwise) suggests that there may exist general cognitive constraints in this area. If this extremely plausible initial finding can be confirmed, then it will be the case that the use of linguistics-specific constraints like the MRC and the notation of Autosegmental Morphology to rule out the phenomena in question is at best redundant and at worst misguided. For example, if the average person is cognitively unable to perform operations on only the prime-numbered segments in a word (without the use of pencil and paper, and within a reasonably short amount of time), then the use of the MRC to prohibit morphological rules of this type probably represents only a Pyrrhic victory.

2 The possibility that substantive constraints can also be placed on morphological rules, after all, is suggested by considerations like those discussed in Footnote 3 (immediately above, keyed to a passage in the main text two pages before where this footnote is keyed). Such constraints may well not be so specific in their reference to segmental phonology as those discussed by McCarthy as governing phonological metathesis--but, on the other hand, one could also approach--and constrain--morphological rules in a purely substantive way. Rather than proceed such that anything storable with the transformational format is a possible rule of morphology unless it is ruled out by some explicit constraint (whether morphologically ad hoc or cognitively general), that is, one could just list the attested (types of) morphological rules which are currently known and then simply stipulate that only process-types which are on that list are possible rules of morphology.

3 It remains to say a final word about the occasionally proposed putative diachronic explanations which are actually invalid that were mentioned in the first paragraph of this paper. What I have in mind here are claims like: "Linguistic situation X is a counterexample to synchronic generalization Y, but X doesn't really count as a counterexample because a common/natural/plausible historical process gave rise to it." The problem in such cases is that children who acquire a language exemplifying situation X presumably have no knowledge of the historical developments which led to X (or to any other aspects of their language)--nor do many of them ever gain such knowledge when they later

are adults. Hence, for them (whether as children or as adults), either X is an exception to Y or it isn't. Language-learning children who possess generalization Y (due either to their genetic endowment or to having been exposed to data which force them to it) and then encounter exceptional situation X have a chance to change X so that it will no longer be a counterexample (or, perhaps equivalently, a chance not to learn X at all). If they still acquire X, then it must count as a counterexample--or else lead one to question whether Y is a valid generalization, after all. Thus, a claim of the general type just discussed is indeed diachronic--since it describes how, over time, the data needed to lead a child to a certain linguistic analysis came to be in his or her environment--but it is not really explanatory, since it does not account for why such a child goes ahead and adopts an exceptional situation in the face of a regular generalization which it violates--other than perhaps by saying that the child does so because of what he or she has heard others do/say, which still doesn't make the exceptionality go away. At most, then, claims of the kind here under discussion explain how a counterexample arose, not why it is allowed to continue to exist. In the sense of such claims, everything has a historical explanation--from which it is clear that, not really so paradoxically, diachronic considerations like them actually explain nothing at all.

REFERENCES

[To present here the full five pages of bibliographical references for this paper would have swelled it to a length unfair to the other authors represented in this volume. Consequently, it has--in consultation with the organizers of BLS X and editors of these Proceedings--been decided to omit all of the references here, and to request all those interested in obtaining them to solicit them from the author at the following address: Department of Linguistics
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