

Lexical Relatedness, Head of a Word,
and the Misanalysis of Latin*

Brian Joseph and Rex Wallace
The Ohio State University

0. Introduction

Two opposing schools of thought concerning divisions within the realm of morphology can be discerned in the general linguistic and morphological literature. One is represented by the work of a good many structuralist (American and European) scholars and is characterized in part by a recognition of a difference between inflectional morphology and derivational morphology. A classic work such as Bloomfield (1933) as well as more recent works such as Anderson (1982) or Zwicky & Pullum (1983) are representative of this tradition.¹ The second 'tradition' (to use the term loosely, to be sure), represented by the work of some (but not all, witness Anderson and Pullum & Zwicky as above) followers of certain camps within the generative transformational school of linguistics, is characterized in part by an opposing view concerning derivational and inflectional morphology; in particular, no distinction is recognized between two such aspects of morphology. A representative work in this camp is Halle (1973).²

The issue is clearly an important one, for there are real differences in morpheme types which motivated the traditional derivational/inflectional distinction in the first place (e.g. derivational morphemes tend to be 'inner' while inflectional morphemes tend to be 'outer'); if no distinction between two types of morphemes is posited, however, some other means must be found for predicting morpheme behavior. Williams (1981) purports to do just that, so that his work can be placed squarely within the latter camp described above. Williams' arguments, therefore, need to be considered carefully, for his justification of the basic premise of the 'Halle (et al.)' school of morphological analysis (no inflectional/derivational distinction) is only as strong as his ability to account for the recurring differential behavior of certain morpheme types.

Williams thus is concerned with a number of issues connected with this central question of a putative difference between derivational and inflectional morphology. In the course of his discussion, he develops two crucial terms, related and head, whose definitions we give below in (1) since they figure so prominently both in Williams' discussion and in our critique of his work.

- (1) a. head (of a word): the righthand member of a morphologically complex word is the head. (248)
- b. related: X is related to Y if Y is the result of removing the head of X. (260)

Secondarily, Williams develops a 'theory of the paradigm' and applies his principles to an analysis of the Latin nominal and verbal system.

Some problems with Williams' analysis have already been pointed out, e.g. by Strauss (1982) and Churma (1983).⁴ However, much more can and should be said, for it can be shown that Williams' theory and his analysis are flawed from both a methodological and an empirical standpoint. Accordingly, it can be concluded that his conclusion that 'as far as the rules of formation go, there is no difference between derivational morphology and inflectional morphology' (283), the basic tenet of the second school of morphological thought noted above, cannot be regarded as demonstrated by Williams' argumentation.

1. Heads and headlessness--universality?

Williams' starting point for his discussion of morphology and word formation is affixation, which he defines formally as:

$$(2) \quad X \xrightarrow{\quad} X \text{ Af or Af } X$$

e.g. ((blue ish) ness)

An obvious question that arises at this point is: What about nonaffixation morphology, i.e. word formation processes such as those that give the relationships in (3)?⁵

(3)	breath	<--->	breathe
	life	<--->	live
	bath	<--->	bathe
	(push up) _V	<--->	(push up) _N ⁶
	permit _V	<--->	permit _N

Williams says that these can be accounted for by a class of rules he calls 'headless' rules, for they do not involve a 'head' in the sense he develops. Affixation morphology, on the one hand, necessarily does involve a 'head' in Williams' sense, inasmuch as there is branching in the internal structure of the word (Af + X / X + Af) and thus a right-hand branch to define a head.

Thus, for Williams, headless derivations as in (3) are systematically different from the 'headed' formations of affixally determined categories and forms. According to Williams 'headless rules always give rise to exocentric structures' (250). For the items cited by Williams (247) this claim is true. There are however other English formations not mentioned by Williams which do not involve right-hand (RH) branching elements and so must be considered 'headless'. Among these are ablauting verb formations like sang (sing), drove (drive), ran (run), found (find), etc. It is difficult to see what definition of exocentricity can be summoned forth to allow one to meaningfully call these ablauting verb formations 'exocentric'. Thus headless rules which figure in the formation of grammatical categories (especially 'inflectional' categories as opposed to what would be traditionally labelled 'derivational' processes), such as those involved in the inflection of ablauting verbs in English, show that the properties Williams assigns to headless rules are wrong.

Moreover, formations like sang (sing) in English appear in all crucial respects (e.g. function) to be parallel to affixation types, e.g. picked

(pick). If 'headless' formations differ systematically from 'headed' ones we might expect this difference to reside in the features characteristic of 'headness', i.e. we might expect 'headless' formations (since they do not have RH branching structure) not to possess features characteristic of a head. And yet formations like sang (sing) possess the feature which Williams uses to determine the head of English past tense formations: tense (250-251). It only follows that if sang (sing) possesses the feature tense, which is the criterion for determining head, then sang (sing) has a head. It just so happens that in this case the head feature is realized not as a right hand element, i.e. as a suffix, but as a simultaneous element.

In fact the simultaneous realization of what are for Williams head features is common among the languages of the world. Numerous good examples are to be found among African languages. For example, Nida (1949: 63) reports that in Ngbaka, a Sudanic language, 'there are four principal forms of every verb' marked by different tonal configurations on the same segmental base: these tone differences 'indicate four principal tense-aspect contrasts':

(4) Ngbaka tense-aspect contrasts:

- a. 'to clean' wà wā wǎ wá
- b. 'to return' kpòlò kpōlō kpòló kpóló

Similarly, in Maasai, nominal cases are marked by tonal shifts (cf. Tucker and Mpaayei (1955), cited in Perlmutter (1982: 308)):

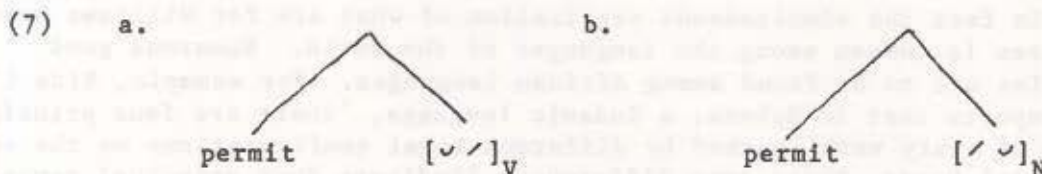
- (5) a. e-dol embartá
3-see horse/NOM
'The horse sees him.'
- b. e-dol embártá
e-see horse/ACC
'He sees the horse.'

Just as English ablaut past tense forms parallel suffixed past tenses, these Ngbaka verb categories and the Maasai case categories seem to correspond in all relevant characteristics to the verbal and nominal categories of a language like Latin (which figures so prominently in Williams' discussion) in which tenses and cases are marked by affixes, specifically suffixes.

In order to get around these problems with Williams' treatment of headless rules, one might propose to treat these cases (e.g. English ablauting verbs) as involving branching, in much the same way as affixation morphology does. A possible formalization of this is given below:



This allows one to capture the parallel nature of the ablauting and suffixal forms neatly. Similar treatments could be devised for each of the headless derivations indicated earlier in (3), for example



For English such a solution, though involving a considerable amount of abstractness, might be feasible. One could argue that since suffixing forms exist alongside simultaneous forms the two are to be treated in a similar manner. However, in languages (like Maasai, apparently) where no suffixing forms exist beside the simultaneous forms it is impossible to provide any motivation for a right-branching treatment. In these cases such an analysis would be quite ad hoc. Thus even if one accepts this abstract solution for English, its extension to other languages will not always be warranted and will often simply be arbitrary, something done solely for the sake of saving the theory. This arbitrariness makes it difficult to maintain that Williams' claims have any empirical content in such instances. Thus one must admit that the head cannot always be identified as the rightmost branching element, as Williams would have it.

This result, while unfortunate for Williams' theory, nonetheless is most welcome, for there are other problems with calling the right hand branching element the head of the word.

In particular, Williams' definition of 'head' would run afoul of languages which, unlike English, are generally prefixing. In such languages, for example Swahili, information which is determined by the right-hand 'head' of morphologically complex words in English, for example, part of speech or grammatically relevant features like case or tense, is instead determined by prefixes:

(8) Swahili (Nida (1949: 12-13))

- a. ni-na-mu-pika
I-past-him-hit
- b. a-taka-nu-pika
he-will-you (pl.)-hit

For such languages, someone working within Williams' framework would either have to start with a very abstract analysis in which all Swahili prefixal elements started out as suffixes or else allow for left-hand heads in some

languages. This latter step would mean that any claim of universality for the definition of 'head' would have to be given up (and note that Williams must have some interest in a universal definition, for he does apply his definitions to Latin later on in his article). Thus, Williams' definition of 'head' fails cross-linguistically really because it is too language-specific.

Moreover, it is not simply languages like Swahili that pose problems for this definition of head. As Williams himself notes (249), the prefix en- in English 'systematically converts nouns and adjectives into verbs, thus displaying the behavior of a head', as in:

(9)	dear	<--->	endear
	noble	<--->	ennoble

Thus even English has some non-right-hand heads--Williams 'explains' the head prefix en- away by saying that it is exceptional but it is a systematic exception: thus he is allowing his theory to 'leak',¹⁰ and in view of what we have seen concerning his notion of 'head' and a language like Swahili, perhaps this is a serious leak which he cannot and should not so readily plug up. It is just as easy to conclude from the behavior of the prefix en- in English that the Right-Hand Head Rule simply is wrong, and the problems with prefixing languages confirm this conclusion.

2. On the analysis of Latin and theory of a paradigm

We turn now to a discussion of the Latin nominal and verbal systems. Williams presents these analyses as (1) a way of illustrating the principles of lexical relatedness and his Right-Hand Head Rule and the way in which it might be applied to languages other than English and (2) as a means of 'explaining' why inflectional affixes appear outside of derivational affixes without recognizing a distinction between the two. In order to make such an explanation work Williams develops a Theory of the Paradigm. Williams' main testing ground for his theory and all that it encompasses--relatedness, head, syncretism, syntactic relevance, etc.--is Latin, specifically the Latin nominal and verbal systems.

However, Williams' analyses of Latin are seriously flawed in a number of respects. These include methodological problems as well as empirical problems, some of which are caused by Williams' methodology. As a result, it can be concluded that his Theory of the Paradigm and the principles upon which it is based are untenable.

2.1. Williams' corpus

The first major problem is methodological in nature. Williams at no point establishes what his corpus is for the description of Latin morphology nor does he acknowledge any sources. While Latin is a language which is well known (and thus such omissions are not as serious perhaps as for less widely known languages), the failure to give such information does present some difficulties; in view of the numerous errors and oversights of fact in Williams' Latin for instance,¹¹ what is one to make of his 'citations' of forms supporting his analysis? His failure to be explicit about sources makes it all the worse, moreover, that he arbitrarily rules

out from consideration at least one case and one declensional class (see below, section 2.2), for these are part of the description of every Latin grammar we have ever seen, even the most elementary ones.

Another aspect of the failure to establish a corpus is that Williams never specifies what he means by 'Latin'--is it Classical Latin only or archaic (Old) Latin as well? Is it Ciceronian Classical Latin in general or just Cicero's usage; does it include later Classical authors such as Pliny the Younger and Tacitus or not; is it elegant literary Latin (e.g. Virgil or Horace) or low-style literary Latin (e.g. Apuleius or Petronius), which is said to reflect popular speech (Pulgram (1958: 314))?¹²

This concern we voice here is not an idle one, for Williams' failure to specify his corpus and sources essentially makes his analysis untestable. His 'experiment' cannot be replicated, let alone fully analyzed and critically evaluated, because we do not know if he was just examining Ciceronian usage (though we doubt it) or what. However, under the assumption that he was somehow giving a 'Pan-Latin' collection of forms, i.e. roughly the familiar usage most people learn as 'Latin' in school, we offer the following critique, basing our analysis on such a form of Latin augmented by variants which must have formed part of the average educated Latin speaker's linguistic competence (inasmuch as they appear in authors of the Classical era).

We have relied on standard Latin reference works, such as Allen and Greenough (1903), Ernout (1953), and Leumann-Hofmann-Szantyr (1963). Since the point of reference for these grammars is the literary variety of Latin of the Ciceronian age, most of the forms we cite can be found in the writings of Cicero or his contemporaries. Since, however, the Latin taught in schools is in some important senses a 'Pan-Latin' variety, forms from pre- and post-Ciceronian writers of various social, ethnic, and regional backgrounds are included in these grammars. We have therefore not hesitated to cite forms from as early as Plautus (circa 200 B.C.) or as late as Tacitus (circa 100 A.D.).

2.2. Paradigms, syntactic features and their ranking in syntactic matrices

To return now to Williams' Theory of the Paradigm, it is essential to note that for him, paradigms consist of syntactic features (SFs), e.g. tense, case, person, number, and morphosyntactic categories (MSCs), e.g. morphologically distinct forms which are 'related' in Williams' sense of the term.

The SFs are hierarchically ranked so as to yield a syntactic matrix (SM) which is then filled with MSCs. The paradigm is therefore a constellation of related forms in which morphemes expressing syntactic features function as the heads of the related forms.

To account for syncretism in Latin nominal and verbal paradigms, Williams posits SFs and a ranking for these SFs so as to yield an appropriate SM. We give below Williams' detailed matrix for the Latin noun (Table A) and his less detailed¹³ one for the verb (Table B).

Table A

Syntactic Matrix of Latin noun (after Williams 1981: 267)

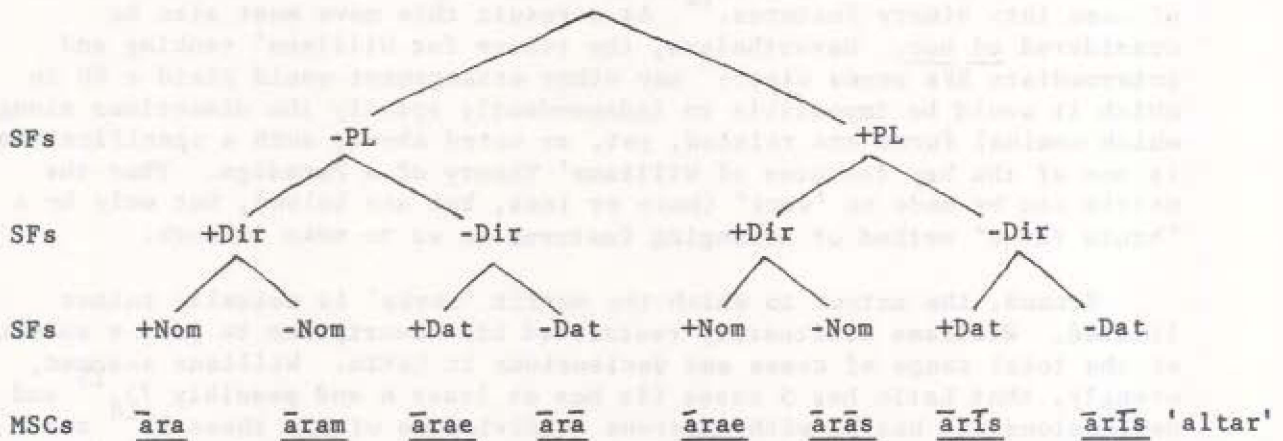
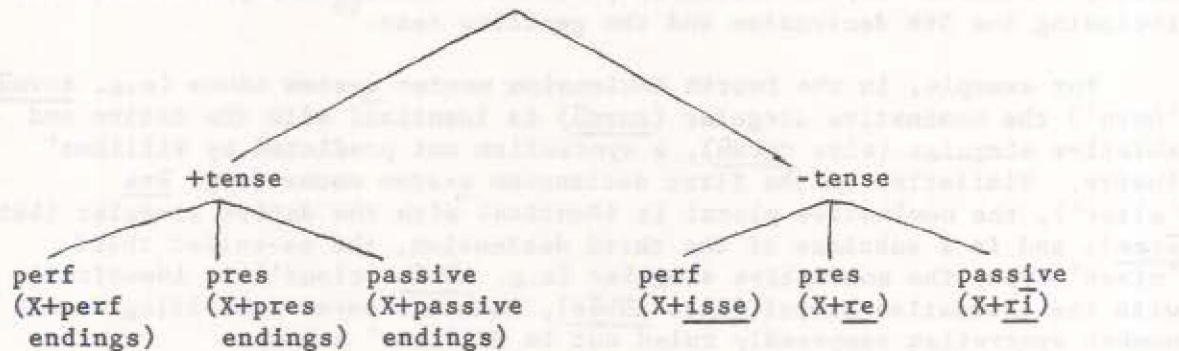


Table B

Syntactic Matrix of Latin verb (after Williams 1981: 269)



These syntactic matrices specify the dimensions along which items are related independent of any pair of forms cited, so that in the case of substantives the SM is supradeclassional, and in the case of verbs it is supraconjugational. This fact is formally expressed in terms of possibilities of paradigm-internal syncretism.

In particular, with regard to the noun, Williams claims (268) that possibilities of case syncretism will be the same across declensions, and that only certain types of syncretism will occur: e.g. with number identical, dative = ablative, nominative = accusative, but not nominative = dative or nominative = ablative, nor any cross-number syncretisms (e.g. nominative plural = dative singular). This analysis and its predictions, however, encounter two major problems.

First, the hierarchical order of SFs which Williams assumes for the nominal SM is without any independent justification. In the description of the Latin noun he assumes that the SFs are to be ranked: +PL > +Direct > +Nominative/+Dative. However, Williams does not offer any principles for

such ranking and thus it must ultimately be considered ad hoc. Moreover, the SF case is divided into the categories +Direct, +Direct governing the nominative and accusative cases, -Direct governing the dative and ablative cases. But Williams again offers no substantive evidence for the division of case into binary features.¹⁴ As a result this move must also be considered ad hoc. Nevertheless, the reason for Williams' ranking and intermediate SFs seems clear: any other arrangement would yield a SM in which it would be impossible to independently specify the dimensions along which nominal forms are related, yet, as noted above, such a specification is one of the key features of Williams' Theory of a Paradigm. Thus the matrix can be made to 'work' (more or less, but see below), but only by a 'brute force' method of arranging features so as to make it work.

Second, the extent to which the matrix 'works' is actually rather limited. Williams arbitrarily restricted his description to just a subset of the total range of cases and declensions in Latin. Williams assumed, wrongly, that Latin has 5 cases (it has at least 6 and possibly 7)¹⁵ and 4 declensions (it has 5, with numerous subdivisions within those 5)¹⁶ and then proceeded to base his analysis on 4 cases (nom.-acc.-dat.-abl.) and three declensions (1-2-3). The reason is clear. It is difficult to make the Theory of the Paradigm work when all cases and declensions are taken into consideration.¹⁷ The predictions concerning case syncretism made by his theory prove to be wrong not only within the limited set of data (4 cases, 4 declensions) he considered, but also within¹⁸ an expanded data set including the 5th declension and the genitive case.

For example, in the fourth declension neuter u-stem nouns (e.g. cornū 'horn') the nominative singular (cornū) is identical with the dative and ablative singular (also cornū), a syncretism not predicted by Williams' theory. Similarly, in the first declension a-stem nouns (e.g. āra 'altar'), the nominative plural is identical with the dative singular (both ārae); and in a subclass of the third declension, the so-called third 'mixed' type, the nominative singular (e.g. nūbēs 'cloud') is identical with the accusative plural (also nūbēs), both instances exhibiting cross-number syncretism supposedly ruled out in Williams' schema.

Moreover, with the addition of the genitive case, one finds besides the troublesome syncretisms Williams himself notes but dismisses as 'accidental' (see footnote 17), such mergers as genitive singular = accusative plural for first declension nouns with genitives in -ās (e.g. familiās 'of a household'). Finally, by taking in the fifth declension, more unpredicted syncretisms such as genitive singular = nominative/accusative plural (e.g. diēs 'day') are found. The complete range of these syncretisms (excluding the locative and vocative) which falsify Williams' account is summarized in Table C below.

Table C

Some examples of syncretism in Latin noun declensions

	NOM SG	GEN SG	DAT SG	ABL SG	NOM PL	ACC PL	GLOSS
Declension 1:		ārae	ārae		ārae		altar
		familiās ^a					household
Declension 2:		hircī			hircī		he-goat
Declension 3:	canis	canis					hound
		nūbēs ^b			nūbēs	nūbēs	cloud
Declension 4:		manūs			manūs	manūs	hand
	cornū ^c	cornū	cornū	cornū			horn
Declension 5:		spēī	spēī				expectation
	diēs ^d	diē/diēs	diē	diē	diēs	diēs	day

a. The genitive ending -ās was, in literary varieties of Latin during the age of Cicero, restricted to the noun familia when meaning 'household'. This ending is attested more frequently in the archaic period (for examples see Ernout (1953: 19-20)).

b. Third declension nouns like nūbēs 'cloud' which follow the 'mixed' i-stem declensional pattern cannot be considered declensional aberrations. We have counted 33 nouns, in addition to nūbēs, which follow this declensional pattern (see Allen and Greenough (1903: 30)). Doubtless there are more.

c. The singular of ū-stem neuters like cornū 'horn' was indeclinable by the beginning of the imperial period (roughly the beginning of the reign of Augustus). The first attestation of a dative in -ū is found in Livy (Ernout (1953:65)). Genitive singulars in -ū are found in Celsus (floruit 50 A.D.) (OLD, 446).

d. During the Ciceronian age there was a considerable amount of variation in the genitive singular of diēs 'day'. Allus Gellius (Att. Noct. 1,1) informs us that Caesar, in his book Dē Analogiā, advocated the use of a genitive singular diē. This form is also attested in Virgil (Georgics 1, 208). A genitive singular diēs is found in the Annales of Ennius (413). Two additional genitives are found in Virgil: diei (Aen. 9, 156) [dieī] and diei (Aen. 1, 636) [dyey] or possibly [dyī].

Williams is less explicit about syncretism in the verb, but it is clear, to judge from his verbal Syntactic Matrix (see Table B), that he cannot account for syncretism in the Latin verb either. In particular, two forms of the 2 sg passive ending in primary tenses are to be found, -ris and -re, and the latter produces 'tensed' forms which are syncretic with the 'untensed' present active infinitive (as well as the rare 2 sg passive imperative), for all the conjugations including irregular verbs, for example:

- (10) a. amā-ris ~ amā-re 'you are loved' = amā-re 'to love' (and cf. also amā-re 'be loved!')
- b. fer-ris ~ fer-re 'you are carried' = fer-re 'to carry' (and cf. also fer-re 'be carried!')

The variant ending -re is not at all rare,¹⁹ and runs throughout the whole of the primary system including the present indicative and subjunctive, imperfect indicative and subjunctive, and future indicative. Since this ending is well-represented, the syncretism it causes is probably not to be treated as 'accidental'. Since this syncretism cuts across a major division, tensed vs. untensed, of the syntactic matrix tree, as well as personal ending and mood categories, it is not accounted for in Williams' system. Similarly, Williams cannot easily explain, if at all, the syncretism of the future perfect indicative active with the perfect subjunctive active in other than 1 sg and 3 pl forms,²⁰ e.g.:

- (11) a. dīxerit 'he will have said' ~ dīxerit 'he might have said (Subj)'
- b. tulerimus 'we will have carried' ~ tulerimus 'we might have carried (Subj)'

Thus, Williams' Theory of the Paradigm does not achieve for the Latin noun or verb what it is supposed to. With regard to the noun, no one ranking of features can yield the appropriate SM for all Latin nouns; moreover, contrary to Williams' predictions, case syncretism in Latin does indeed depend on declension, gender, and in some instances on the particular subclass within a declension or individual lexical item in question. With regard to the verb, similarly, syncretisms occur which the Theory of the Paradigm cannot account for.

2.3. Ordering of morphemes

In Williams' framework there is no special rule for the introduction of inflectional affixes. As a result, Williams must have some explanation for the fact that inflectional affixes tend to be 'outer' while derivational affixes tend to be 'inner.'²¹ Williams accounts for the position of the rightmost inflectional morpheme in a word by means of the notion 'syntactic relevance.' Morphemes which bear 'syntactically relevant' information must appear in ultimate head position in words, i.e. the rightmost position, so that the syntactically relevant feature can percolate up to the syntactic level (264). In the Latin verb, for example, Williams claims (264) that 'tense'²² is syntactically relevant 'in that it

determines the case of subjects.' As a result, the personal endings of the Latin verb appear in ultimate head position, e.g. dictābi-t 'he will repeat.' The notion 'syntactic relevance' only accounts for the position of the rightmost morpheme. The implication of this notion is that there will be only one syntactically relevant morph per word, inasmuch as only one morph can be rightmost in the word. A serious problem arises, however, since within both the Latin noun and the Latin verb, more than one morph can in fact be syntactically relevant.

In the noun, the case-ending is the rightmost morpheme, and it is for Williams (264) syntactically relevant. However, it is often the case that the gender of a Latin noun is determined by a pre-final (derivational) morpheme; for example, all the abstract nouns in -tās such as the nominative pie-tās (from underlying /pietāts/), gen. pietātis 'dutifulness', are feminine and all the nouns in -ētū, e.g. rosētum 'rose garden' (derived from feminine rosa 'rose'), are neuter, and so on. Gender is a syntactically relevant feature in that it determines the form of adjectives dependent on the noun, i.e.:

(12) (Cicero Topica 23, 90)

- a. prīma pietās . . . nōminātur
 first/fem dutifulness is mentioned
 'dutifulness is mentioned first'
- b. *prīmus pietās . . .
 first/masc

Thus gender is a feature which in Williams' system must be able to percolate upwards to the node dominating the word in question, and therefore would be predicted to be rightmost; however, such morphemes are never in ultimate head position.

Similarly, regarding the verb, there are constructions in which the occurrence of a subjunctive mood form higher up in a sentence causes a verb which would otherwise be indicative to instead be subjunctive; this is the phenomenon known as 'subjunctive by attraction' (see Hale & Buck 1973: section 539), as in:

(13) (Cicero Dē Ōrātōre I, 61, 260)

cum ita balbus esset, ut eius ipsius artis cui
 since so stammering was/3sg that that-very-art/gen which

studēret prīmam litteram non posset dīcere
 study/3sg subj first-letter/acc not could/3sg subj say/inf

'Since he was such a stammerer that he could not pronounce the first letter of the very art he was studying.'

in which the subjunctive studēret occurs in place of the imperfect indicative studēbat by 'attraction' with the subjunctive posset. Thus mood markers are syntactically relevant in that they can affect the forms of words associated with them.²⁴ Yet they never occur in final position and

are always 'inner' with respect to the personal endings.

Thus the notion 'syntactic relevance' cannot be used to get the order of morphemes in Latin nouns and verbs to come out correctly, since it predicts that certain elements should be in ultimate head position when in fact they are not. Williams' system, therefore, fails to account for this aspect of the ordering of morphemes in Latin words.

Similarly, Williams' framework has difficulties accounting for the position of inflectional affixes which are not syntactically relevant. Ostensibly, Williams accounts for the position of these affixes outside of derivational affixes by relying on the notions head and relatedness. However, it is difficult to see what value these notions have for determining the linear order of morphemes, since, in a stem like dic-tā-bi-, with the morphological analysis:

(14) dic-tā-bi-
say-FREQ-FUT

both the 'derivational' morpheme -tā- and the 'inflectional' morpheme -bi- are 'heads', based on Williams' criteria for 'headness' (pp. 248-253), yet neither one is more 'head'-like than the other; thus there is nothing which should cause -bi- to appear to the right of -tā-.

In actuality, Williams accounts for the ordering of inflectional morphemes outside of derivational by using the paradigm, which is constituted by syntactic features, inter alia (see section 2.2 above). Thus the property of bearing a syntactic feature, whether 'syntactically relevant' or not, becomes, in Williams' theory of the paradigm, a further way of distinguishing among morpheme types. In the stem dic-tā-bi-, -bi- will appear outside of -tā- by virtue of the fact that it possesses a syntactic feature, the criterion for being involved in a paradigmatic relationship, while -tā- does not. Thus, Williams accounts for the order of morphemes in words like dictābit in essence by creating a three-way division in affixal morphemes based on the notions 'bearing a syntactically relevant feature' and 'bearing a syntactic feature'.²⁵ For example, the personal ending -t possesses a syntactic feature and moreover that feature is syntactically relevant; and hence it must be in ultimate head position. -bi-, however, only possesses a SF and that feature is not syntactically relevant; as a result, its position is inside of -t. The affix -tā- possesses no SF and so automatically has nothing of relevance; as a result it occupies the innermost position in the linear order of affixes.

Therefore Williams can indeed dispense with a rule introducing inflectional affixes, but it is accomplished at the cost of introducing a three-way distinction among affixal morphemes. But even this three-way distinction does not enable Williams to account for all aspects of the order of affixes in all Latin words.

In particular, there are sequences of morphemes containing elements of the same feature designation, so that any decision as to which one is more of a 'head' and thus outside the other, is purely arbitrary. A form of this type is the 3rd person singular future perfect indicative, e.g. dictāverit 'she will have said', which is to be morphologically analyzed

as:

(15) dic-tā-v-eri-t

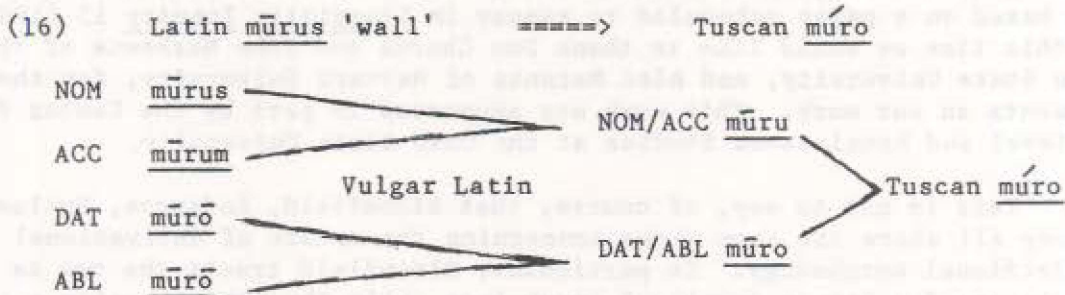
say-frequentative-perf-fut-3sg

(root)- [-syn rel][-syn rel][-syn rel][+syn rel]
[-syn feat][+syn feat][+syn feat][+syn feat]

Both the -v-, as a marker of the perfective aspect, and -eri-, as a marker of the future tense, would bear syntactic features in Williams' system (see section 2.2) but these features would not be syntactically relevant in that they would not affect the form of other words dictāverit is connected with. Yet it is a fact about Latin that the -v- must always appear inside -eri-; this fact shows that making use of a three-way distinction among morpheme types through these features, the way Williams implies, cannot account for all aspects of the ordering of morphs within words in Latin.

2.4. Diachronic falsification

Williams' theory can be falsified in one other way. Under the reasonable interpretation that synchronic predictions about case syncretism delimit possible diachronic developments, Williams' analysis cannot explain certain developments in nominal paradigms between Latin (in the general sense) and Romance. In the Tuscan variety of Italian, for example, all of the singular forms (except the genitive) of o-stem nouns fall together as a result of various diachronic developments (loss of s# and m#, merger of unaccented o and u):²⁶



The transition from one chronological stage of a language, e.g. Latin, to another, e.g. Tuscan, can be viewed as a series of changes in successive synchronic language stages. Therefore, the impossibility of a merger synchronically of NOM/ACC with DAT/ABL due to general principles such as those Williams tries to develop would make it impossible, in his framework, for a language like Latin to develop into a language like Tuscan,²⁷ for at some point a merger otherwise ruled out by his system would have to be tolerated synchronically. Indeed, taking Williams' position to its extreme in diachronic terms, it seems that he is making a strong--but in our view improbable--claim about sound change, namely that no sound change can occur which would cause an 'illegal' syncretism. The Tuscan example, and numerous others like it, including the loss of inflection in English paradigms, would seem to falsify this strong diachronic interpretation of Williams' theory.²⁸

5. Conclusion

To sum up, we have presented a number of criticisms of Williams' analysis which together have the effect of negating the value that his theory of the paradigm and his notions 'relatedness' and 'head of a word' might have for resolving the question of a putative difference between derivational and inflectional morphology. Whatever the merits of Halle et al.'s stance on this issue--we personally feel that it has none--Williams' analysis in no way furthers the case for no derivational/inflectional distinction. Indeed, in view of the considerable difficulties Williams' analysis encounters upon closer inspection, one might well say that his account instead argues for the need to recognize such a distinction in morphology.

Many of Williams' problems, moreover, stem from his failure to draw on reliable and complete sources on the Latin language. While we do not feel that only specialists in a particular language should ever write about that language--and in fact we ourselves above cite data from languages we have no direct knowledge of--in the case at hand more careful attention to the facts of the language would have altered much of the analysis in the first place, thereby avoiding the pitfalls we have pointed out.

Footnotes

*This paper is a revised version of a paper read at the 1982 Annual Meeting of the Linguistic Society of America. Sections 2.1, 2.2, and 2.3 are based on a paper scheduled to appear in Linguistic Inquiry 15 (1984). At this time we would like to thank Don Churma and John Nerbonne of the Ohio State University, and Alec Marantz of Harvard University, for their comments on our work. This work was supported in part by the Center for Medieval and Renaissance Studies at the Ohio State University.

¹This is not to say, of course, that Bloomfield, Anderson, Pullum and Zwicky all share the same views concerning the nature of derivational and inflectional morphology. In particular, Bloomfield treats the two as sub-types of a larger domain of morphology while the others assign each to separate components and do not necessarily place the two together within a single larger component.

²Compare, for instance, the following passage from Halle's article (p. 6): 'the examples discussed above have been chosen from the domain that traditionally has been called derivational morphology. As far as I can tell, facts that traditionally have been treated under the separate heading of inflectional morphology must be handled in completely parallel fashion to those discussed above. I know of no reason why the list of morphemes should not include also the inflectional affixes or desinences, or why the rules of word formation should not include rules for positioning the inflectional affixes appropriately or for handling such other inflectional phenomena as reduplication, stem ablaut, etc.'

³Here and elsewhere, when citing Williams' paper, we give only the relevant page numbers.

⁴ Strauss, for instance, attacks Williams--convincingly, in our view--on the issues of semantic compositionality and structural well-formedness. Churma, moreover, points out that, contrary to Williams' claims (251), compounds do occur in which there is internal inflection, such as publications list, abstracts committee (and we note in passing that such compounds with inflected first members occur in a number of ancient Indo-European languages, e.g. Vedic rathe-~~s~~thā- 'standing on a car' with locative first member (see MacDonnell (1916: Section 187.2) for more examples) and possibly, though it could be a late univerbation rather than an old compound, Latin aquaeductus 'passageway for water' with a dative first member (Buck (1933: 353))).

⁵ We use double-headed arrows (<--->) intentionally here to beg the question of the 'direction' of the derivation in these instances; we wish only to emphasize the relatedness of the members of each pair.

⁶ Williams (250), in describing the formation of nouns like push up from verb + particle combinations, states the relevant rule as follows (Williams' example (19)):

(1) word ---> phrase

(N ---> VP)

which seems to us to have the direction of the arrow reversed; deriving the noun push up from the verbal unit push up strikes us as far more natural than deriving the verb from the noun.

⁷ For a discussion of the notion exocentric and examples of exocentric morphological constructions see Nida (1949: 94).

⁸ The diacritics \ - √ / mark low, mid, contour, and high tones, respectively. Nida does not specify what the semantic distinction among these forms is and it is hard in some ways to reconcile the facts he cites with the description of Ngbaka given by Thomas (1963), though Thomas (135-141) does give a number of 'headless' (in Williams' sense) derivations such as bɪ 'black' <---> bɪ 'blacken' which would be problematic for Williams' treatment. Tiv, as described by Goldsmith (1976: 36-45), following Arnott (1964), may be a better example of a language with simultaneously realized inflectional markers. We thank Don Churma for bringing Tiv to our attention.

⁹ The formalization of the 'structure' of ablauting verbs described in (6) would actually parallel the structure of suffixing verbs as diagrammed by Williams (250: (20b)).

¹⁰ See footnote 17 for another instance where Williams is not disturbed by an 'accidental' array of facts counter to the predictions of his theory.

¹¹ The omissions are noted in section 2.2 below. The other errors of fact are as follows:

a. Williams generally fails to indicate the length of Latin vowels

(vowel length is phonemic in Latin, e.g. ōs 'mouth' vs. os 'bone'). For example, first conjugation Latin verbs generally (there are very few exceptions, e.g. dare 'give') have a long stem vowel -ā-, e.g. lūdificās 'you deride' (stem lūdificā-), amābis 'you will like' (stem amā-). Williams consistently (13 times) fails to indicate that this stem is long.

b. Williams cites (269) only one (-rī) of the two (-rī/-ī) present passive infinitive endings. The third conjugation regularly uses the ending -ī, e.g. capī 'to be seized'. The remaining conjugations (1, 2, 4) use the ending -rī.

c. Williams claims (268) that the third declension neuter nominative/accusative singular ending is -us. Most Latin third declension neuter nouns are counterexamples to this statement, e.g. animal 'animal', cor 'heart', calcar 'spur', ōs 'mouth', os 'bone', nōmen 'name', mare 'sea', etc. (see Allen and Greenough (1903: 26-30)). There are a few neuter nouns of the third declension which do end in -us, e.g. corpus 'body', opus 'work', genus 'family'. However, the -us in these cases is part of the stem, not a nominative/accusative neuter ending.

d. Williams' morphological analysis of Latin verb forms is inconsistent and in some cases simply wrong. Williams' analysis of the first and second conjugation future morpheme illustrates this point well. On page 264 Williams notes that -bi- is the Latin future morpheme. However, embedded in his discussion of morphosyntactic categories (270) is a diagram of the structure of the Latin stem lūdificāb(i) 'delude' in which the future morpheme is analyzed as -āb-. Incredibly, in the first sentence below this diagram the morpheme is noted simply as -b-. Of the three segmentations cited by Williams, -āb- is impossible, for it obscures the relationship between the -ā- vowel of the first conjugation presents and the -ā- of the future, amās vs. amābis, and cannot work for the second conjugation futures, e.g. sordēbis 'you will be worthless'. For the remaining segmentations -b- and -bi-, at least two possible analyses exist. Redenbarger (1976: 7 and 1980 class lectures) argues that the underlying representation for this morpheme is /b/ and that -i- is epenthesized in the environment C+ C (where + indicates a productive morpheme boundary), e.g. /amā+b+t/ ---> amābit. While such an analysis is conceivable it is not as attractive in our opinion as an analysis which recognizes two lexical variants, -b- and -bi-. The advantages of this analysis as opposed to the one suggested by Redenbarger are discussed at length in DeWandel (1982: Chapter 1).

¹²The relation among these several sociolects and varieties is a complex sociolinguistic question to which we do not even pretend to have an answer here; we merely acknowledge that this is a factor which any truly adequate analysis of Latin morphology must ultimately grapple with, and note that Williams never even recognizes the existence of such an issue.

¹³Williams' verbal matrix omits the imperative and subjunctive moods as well as the imperfect and future tenses. Moreover, his ternary division for the verb implies that the passive stem is in some way distinct from the active stem, an observation which the facts of Latin clearly do not warrant, for the present stem is the base for the addition of both active and passive personal endings, cf. amā-mus 'we love' ~ amā-mur 'we are loved.'

¹⁴A binary analysis of SFs is not even a necessary feature in Williams' system, for he gives (269) a ternary division for verbal forms, into passive, present, and perfect stems (see Table B).

¹⁵The six secure ones are nominative, genitive, accusative, dative, ablative, and vocative; the one additional questionable one is the locative. Not all nouns form locatives (i.e. locatives are not widely enough attested to allow one to infer full productivity for this case/category). Moreover, locatives, when they do occur, are formally distinct only for some third declension nouns (e.g. rurī 'in the country') and otherwise are identical in form to the genitive case or the dative/ablative depending on declension and number (see any handbook of Latin for details). Similarly, the vocative is distinct in form only for singular second declension masculine nouns (except for r-stems, though puere occurs once (Plautus Pseudolus 241)) and otherwise is identical with the nominative. Thus one can sympathize to some extent with Williams' having ruled the vocative and locative out of consideration; but the decision is arbitrary and nowhere does he justify it, let alone even mention it.

¹⁶The grammars and handbooks of Latin divide the nominal system into five declensions. This division was instituted by the ancient grammarians (see Leumann-Hofmann-Szantyr (1963: 256)). As any Latinist would readily admit, however, this division is somewhat arbitrary and does not accurately represent the diversity which exists within each declension. For example, second declension r-stems form a distinct subclass apart from o-stems (see Allen and Greenough 1903: 21); within the third declension at least four subclasses must be recognized: stems ending in an obstruent, stems ending in a sonorant, 'pure' i-stems, and 'mixed' i-stems (see Allen and Greenough (1903: 24-31).

¹⁷As Williams himself recognizes with regard to (only) the genitive (268-269): 'the genitive singular is something of a problem, since it is syncretic with the nominative plural in I and IIM and IV. It is impossible to express this syncretism in the theory outlined here, and it must thus be viewed as 'accidental' syncretism.' This statement is rather odd, given the fact that earlier (267), Williams states that he 'will ignore the genitive, which can be fit into the theory in a number of ways.'

¹⁸Not to mention, of course, the additional problems that would arise if the vocative and locative cases were both taken seriously.

¹⁹The 2 sg passive -re is the more frequent variant in the archaic period. By the classical period, however, the variant -ris was preferred in the present indicative while -re was preferred in the imperfect and future indicative and the subjunctive (see Ernout 1953: 122).

²⁰Originally, the future perfect and the perfect subjunctive were distinguished by means of vowel length, short i (-eri-) in the future perfect, long ī (-erī-) in the perfect subjunctive. Traces of this distinction can be found in the archaic poets, e.g. Plautus uēnerīmus (Bacch. 1132). This length distinction was neutralized by the classical period and as a result the future perfect and perfect subjunctive were syncretic in all but the 1 sg (see Ernout 1953: 218 for the 3 pl).

²¹The appearance of an affix to the right of a root morpheme is accounted for by Williams' affixation rule (246).

²²We suspect finiteness is a better term, as the personal endings do not themselves indicate tense in the sense of temporality.

²³See Allen and Greenough (1903: 140 ff.) for details.

²⁴We have given this example because it is unlikely to be semantically controlled. Other sequence of tense/mood phenomena traditionally described for Latin could well be semantic and hence not relevant here.

²⁵According to Williams a morpheme which contains a syntactically relevant feature by definition contains a syntactic feature. As a result there can be no morpheme with the feature designation [+ syntactically relevant] and [- syntactic feature].

²⁶For a concise discussion of these diachronic developments in Tuscan see Elcock (1960: 24, 43, 51-52).

²⁷We are assuming here that Vulgar Latin (i.e. the language roughly equivalent to Proto-Romance) was a coexisting sociolect with literary Classical Latin (i.e. roughly the variety of Latin Williams attempts to describe) and that many speakers were competent in both varieties. If such an assumption is unwarranted--the relation of the two varieties of Latin is indeed a complex issue and we do not presume to have a simple answer to it--then the diachronic evidence cited here may well not count against Williams' account (though, of course, all of the synchronic considerations mentioned above still would). See also footnote 12 and section 2.1 above.

²⁸In essence Williams' theory predicts that grammatical conditioning on sound change should be a common phenomenon. However, good instances of grammatical conditioning are very difficult to find. For a discussion of grammatical conditioning on sound change and a reaffirmation of the Neogrammarian position, see Hock (1976, especially pp. 211-218).

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